

Seven questions about the Cumulative Risk Initiative

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Executive summary

The National Marine Fisheries Service (NMFS) has been developing two modeling tools which they are using for assessing extinction risks and management options for ESA-listed salmon populations in the Columbia and Snake River Basins. These models provide the basis for what NMFS calls the "Cumulative Risk Initiative" (CRI). Both these models have flaws which make the conclusions cited by NMFS (NWFSC 1999) and the Corps of Engineers (USACE 1999) more optimistic than can be justified by the data. The Independent Scientific Advisory Board (ISAB) has asked NMFS to explain or correct some of these problems (ISAB 1999), but none of these problems has been substantively addressed as yet by responses from NMFS that have been posted on the CRI website.

This report summarizes what I believe are the most serious problems with the CRI. Most of these problems have been brought to NMFS' attention, sometimes by several parties. This discussion centers around the CRI analyses of spring/summer chinook, both because these populations are in the most trouble, and because they are the populations to which the CRI has devoted the most attention .

In short, NMFS is using two very simplistic models in order to make some of the most important decisions ever made about saving these last vestiges of salmon populations that, not so long ago, numbered in the millions. NMFS' assumptions and judgments consistently err on the side of optimism, with little explanation or scientific justification. For example, **the CRI focuses extinction risk analyses on an analytic quasi-extinction threshold of one fish**—an analysis threshold which is lower than values typically used in extinction risk assessment, and which causes the risks of extinction to be underestimated. Their justification is that this threshold is "conservative." In fact, it is one of the least conservative thresholds they could have chosen.

Furthermore, despite the prevailing practice in endangered species risk assessments, **CRI models ignore population and environmental trends, focusing instead on average population growth rates**. The populations that are the subject of the CRI analyses, however, have been declining at an ever-accelerating pace since at least the early 1980s, a period NMFS considers to be stable in terms of the hydropower system. Focusing only on average population growth rates from the 1980s has a similar effect to assuming that conditions today are no worse than they were a decade or more ago. Textbooks and peer-reviewed papers cited by NMFS make it clear that such an assumption is not appropriate in light of the available population data. NMFS has not discussed why focusing only on the averages and ignoring the trends is scientifically credible, though this issue has been raised repeatedly (most notably, by the ISAB).

NMFS has not discussed nor answered questions about why the CRI models were recently revised to ignore the post-1990 population information. The most recent CRI analyses use only brood year data from a decade or two ago (1980-1990 brood years), ignoring the 1991-1994 brood year data that are now available. NMFS in fact used the full range of data in earlier analyses, and thus had to go to some trouble in order to make these revisions. Because these populations have been declining at an accelerating rate, NMFS' selective choice of older data produces more optimistic results than when their analyses were based on the whole record. For

example, using the CRI's extinction modeling method, the latter half of the 19-year record yields 10-year probabilities of extinction that are almost four times higher than extinction probabilities based only on the earlier period.

The CRI and the draft Environmental Impact Statement (DEIS, Anadromous Fish Appendix A) rely on a questionable sensitivity analysis method which the source they cite says should not be used. NMFS justifies this choice by arguing that it is intuitively appealing. The problem is that the method chosen is more a method for ranking variables according to the way mortalities are allocated, than it is a sensitivity analysis. It produces results that are very different from the results produced by the standard, textbook way of conducting sensitivity analyses. The CRI results, in fact, do not even make sense, because they point to improving habitat quality in wilderness areas as the number one management tool for recovering Idaho spring/summer chinook populations.

To date, none of the CRI publications available on the CRI website has had anything to say about model validation. Some simple comparisons illustrate how important an omission this is. For example, survivals assumed or calculated by the CRI turn out to be significantly different from survivals available in the literature. **Of particular interest is smolt-to-adult survival (SAR), which CRI analyses assume to be almost four times as high as those that actually have been measured for many years.** Because so much discussion of Snake River dam removal focuses on this life stage, it seems important to make sure the parameter settings in the CRI analyses are consistent with available data.

The problem ripples through the CRI models because these unusually high estimates of post-Bonneville (smolt to adult) survival force the models to use unusually low estimates of egg-to-smolt survival—again inconsistent with available data. **If commonly accepted survival estimates are used in the CRI models, the results being distributed by NMFS turn out to be wrong: it turns out that the most important variable isn't first-year survival; the most important variable should be post-Bonneville survival—which other NMFS-chartered analyses have determined include delayed impacts from the hydro system.**

Finally, given that the CRI is supposed to be a "Risk" initiative, it seems odd that NMFS is **not using standard risk assessment tools.** The CRI relies instead on unvalidated, simple models which ignore trends and uncertainty in the data, trends and uncertainty in the environment, and uncertainty in implementation. The primary justification offered to date is essentially that they only have a few months, and so simple models are better. It is difficult to believe that any peer-reviewed journal would accept this kind of rationale as a justification for selecting a model. Making 100-year predictions using averages from simplistic models, which rely on overly optimistic assumptions, and have not been validated, does not seem to be an adequate way to deal with such an important decision; nor does it appear to be scientifically credible or appropriate.

Seven questions about the Cumulative Risk Initiative

1. What justification is there for defining quasi-extinction as one fish in a single year?

The CRI's extinction model is used to estimate how serious the risk of delay might be. The question is, will these populations go extinct if we wait another 10 years or so before deciding to remove the Snake River dams? The CRI model's answer to this question depends on the number of fish assumed to define "quasi-extinction." In selecting a quasi-extinction definition of ≤ 1 fish in a stream for one year, NMFS chose one of the least conservative standards they could have chosen, not the most conservative as they claim. Furthermore, in contrast to current practice in risk analysis (Burgman et al. 1993, Musick 1999, NRC 1995), their models ignore the declining trends of these populations' average growth rates.

Many species exhibit compensatory population dynamics, meaning that once a population has dropped below a certain level, the damage is irreversible and the population is doomed. For this and other reasons, population biologists often use a "quasi-extinction" threshold below which they believe risk factors such as genetics and demographic stochasticity become significant. NMFS Conservation Biology Division scientists have argued in the past that because of demographic and environmental stochasticities, as well as compensatory effects, thresholds should be set high enough to adequately account for such risks (Wainwright and Waples 1998). The justification offered by the CRI for using a threshold of one fish is that, because salmon do not all return to spawn at the same age, true extinction would actually require extinction of all extant brood years. This argument still does not address the fact that these populations have been small enough, for enough years, that genetic and demographic risks are most likely already large (Myers et al. 1995).

Botsford and Brittnacher (1998), for example, used a quasi-extinction threshold of 100 chinook spawners, arguing that, for Pacific salmon, compensation occurs at around 100 females. Mundy (1999) used a threshold of 15 fish in a brood year, although, following established practice, he focused more on rate of decline as an indication that a population was beyond rescue. Nicholson and Lawson (1998) defined quasi-extinction as ≤ 50 spawning coho in a basin. Nicholson and Lawson's standard is consistent with the IUCN's category of "critically endangered" for very small or restricted populations; though for small and/or fragmented populations undergoing rapid decline, the IUCN defines "critically endangered" to mean a population of < 250 fish (Musick 1999)¹. The IUCN's 250 fish standard is similar to the NMFS' 1995 standard for population survival used by PATH²—namely 150 or 300 fish, depending on the population³.

The point here is that, although there is no established standard extinction threshold for salmon, the implications of the "quasi-extinction" threshold chosen are significant because of the implications about the risks of delay. For example, the CRI predicts that it will take 49.1 years for the Marsh Creek spawning population (presumed in the CRI to currently be 83 fish, although this year the population was actually 0) to decline to 1 fish. Using that same model, this conclusion means that the Marsh Creek population would drop from 83 to 15 fish in 16.6 years, or to 50 fish in 2.2 years. If "quasi-extinction" is defined as 50 fish, this analysis would imply

that the population has 2.2 years left, not 49.1. If the fact is taken into account that these are small, fragmented populations which have recently undergone rapid declines, and the IUCN threshold of 250 fish is applied, then the CRI's extinction model would indicate that the mean time to extinction for all the index populations is zero.

Table 1. Expected years to extinction, using the Dennis model, for different quasi-extinction thresholds

Stream	Quasi-extinction threshold (time predicted for the population to decline from its current size to a spawning population of 1, 15, or 50 fish)		
	1 fish	15 fish	50 fish
Marsh	49.1 years	16.6 years	2.2 years
Johnson	279.7 years	114.3 years	40.8 years
Imnaha	81.8 years	45.1 years	28.7 years
Bear Valley	151.0 years	68.5 years	31.8 years
Poverty	336.0 years	170.9 years	97.5 years
Sulphur	317.4 years	113.4 years	22.6 years
Minam	173.8 years	74.68 years	30.6 years

The ISAB has also questioned the choice of such an extreme threshold (ISAB 1999), but NMFS has so far not addressed this concern. Since the extinction model results depend on the "quasi-extinction" threshold chosen, it is important that NMFS justify their choice of such an idiosyncratic threshold.

2. What justification is there for using an average growth rate to conduct risk assessment?

All the CRI analyses (extinction as well as management analyses) are based on average growth rates called λ . The problem is that actual growth rates today are less than these average λ s, because these populations are declining at accelerating rates.

Figure 1 shows the average number of offspring that survived to reproduce (recruits) per spawner for Snake River spring/summer chinook. Until the mid-1980's, the average replacement rate was more than one, meaning that, on average, at least one offspring of each spawner in those brood years made it back to its natal stream to spawn: until about the mid-1980's, these populations were, on average, increasing. Since the mid-1980's, however, the population growth rate has been less than replacement, which means the populations have been declining. Because for the past 10-15 years these populations' growth rates have not only been below replacement, but have been further and further below replacement with every passing year (Figure 1; see also NMFS 1999 Figure 1, p. 30), then these populations have been declining at an ever-faster pace.

Dennis makes clear that it is not valid to use his method for populations that exhibit this kind of trend because the results will be overly optimistic (Dennis et al. 1991). Neither is it valid to use the average population growth rate λ as a representative variable with the Leslie matrix models which the CRI is using to evaluate management options (Caswell 1989, Burgman et al. 1993).

5-year average R/S, 13 Snake River spring/summer chinook populations

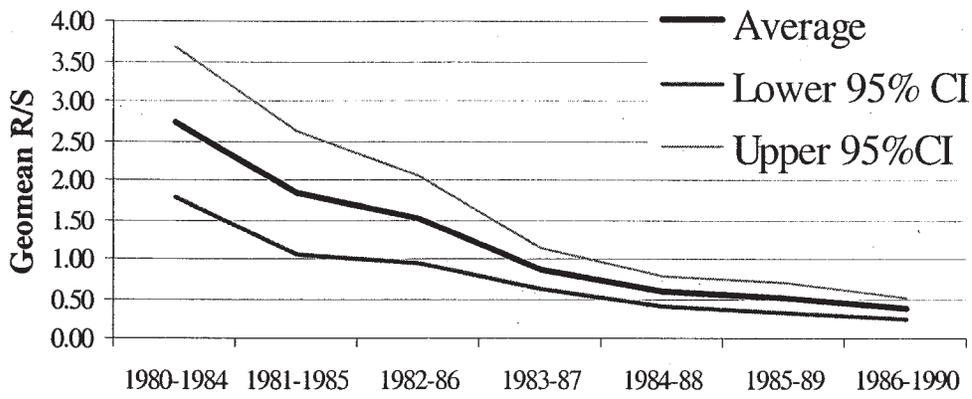


Figure 1. 5-year running average of the geometric mean recruits per spawner, for spring and summer chinook in 13 index streams of the Snake River basin (Beamesderfer et al. 1997, from Mundy 1999).

These declining trends in population growth rate illustrated in Figure 1 also apply, of course, to individual populations such as Marsh Creek (Figure 2). A curve fit through the spawner data shows that not only is the population declining, but the *rate* of population decline is actually increasing: this means that, if the trend continues, then not only is the population going extinct, but at least since 1980 it has been going extinct at an ever-faster pace. Populations that exhibit this kind of trend should not be modeled using Dennis' method (Dennis et al. 1991, NMFS 1999), and when populations are in this kind of trouble, it is not valid to use λ as the response variable with Leslie matrix models either (Caswell 1989, Burgman et al. 1993).

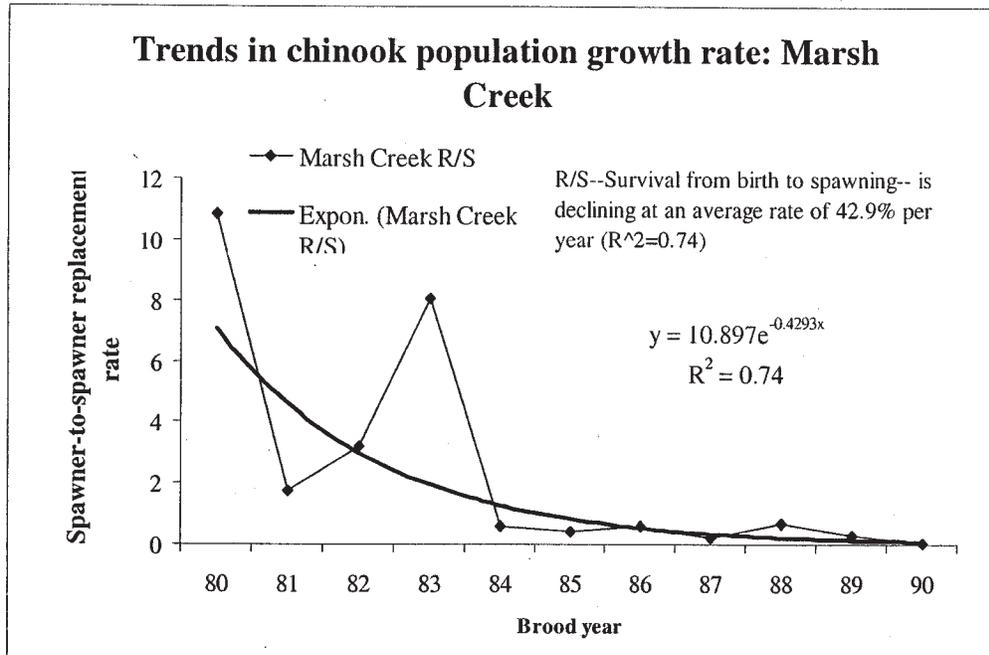


Figure 2. Example of rate of decline of a Snake River spring/summer chinook population. When the average population growth rate is declining like this, extinction estimates based on the Dennis model, and future population estimates based on the Leslie matrix, will be overly optimistic. In the case of Marsh Creek, the population growth rate has been declining since 1980 by an average of 43% per year.

The assumption of a constant, average population growth rate also impacts NMFS' arguments about how much λ would have to increase in order to recover these populations.

For example, in the most recent draft DEIS (Anadromous Fish Appendix) it is claimed that in order to save these populations, the average population growth rate would have to increase by 5-50% (USACE 1999). However, because the analysis starts from a 10-year average growth rate estimated from 1980-1990 brood years⁴, it misses the fact that the rate of return has actually been declining, and so predictions based on 1990-present would be more pessimistic than the analysis period NMFS has chosen.

For example, Table 2 shows a comparison of population growth rates from early and later halves of the available data. Depending on the stream, average λ s dropped by 17% to 46% between the 1980-1986 and 1987-1993 halves of the total available data: Although the population growth rates were declining even then, the average λ s for the index streams were all better than replacement during the early 1980s; but since then, population growth rates have all been less than replacement.

Table 2. Decline in average population growth rates λ , 1980-1993 (data analyzed were from NMFS 1999).

Stream	λ , 1980-1986	λ , 1987-1993	% change

Marsh Creek	1.12	0.78	-30%
Johnson Creek	1.11	0.93	-17%
Imnaha	1.08	0.78	-27%
Bear	1.11	0.91	-18%
Poverty	1.15	0.87	-24%
Sulphur	1.40	0.83	-40%
Minam	1.38	0.74	-46%

Since this dismal trend has continued another 10 years beyond the period NMFS is currently using for their extinction analyses, it means that even if efforts to save these populations began immediately, then the improvements suggested by the CRI would be far from adequate to save these populations. In reality, to save these populations, first the ongoing decline in growth rate would have to be stopped, and then that trend would have to be reversed. That is a more daunting task than simply increasing the growth rate from what it was in the early 1980's. Furthermore, the estimated 10-year risks of extinction would not apply to the 10 years beginning today, but rather to the 10 years beginning in 1991.

How does the fact that the average population growth rate is declining affect extinction risk estimates? Figure 3 shows how distinctly different conclusions about the effects of delay are reached, depending on whether the average growth rate of the stocks is calculated using data from 1980-1986 or 1987-1993 brood years. Although the population was already in decline, analysis using the CRI model, based on the average of pre-1987 data, makes it appear that the population would grow in the future at a healthy pace. Unfortunately, because the population was not stable, but was instead declining dramatically, estimates based on more recent data make clear that the population was headed rapidly toward extinction. As it turns out, the prediction based on more recent data was closer to accurate: the curve based on old data would have predicted about 150 spawners this year in Marsh Creek, instead of the zero that actually returned 5.

Because the model results are distinctly different for recent years than for previous years, it means that using an average growth rate for the period, as NMFS does, seriously underestimates the likelihood of extinction and the effects of delay.

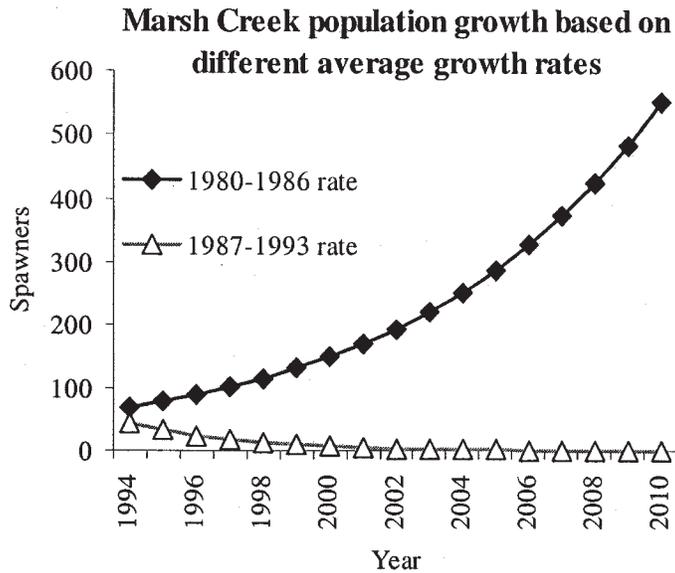


Figure 3. Population growth predicted by the Dennis model, using data from 1980-1986 and from 1987-1993.

This example illustrates one of the problems with conducting risk assessment using an average of any kind, let alone to analyzing a population whose growth rate has been declining for many years. Average growth rates such as λ —whether expressed as an arithmetic mean, geometric mean, median, or other measure of central tendency—are generally considered to be overly optimistic as well as of questionable value for risk assessment, particularly in conservation biology (Boyce 1992, Burgman et al. 1993, Holling and Clark 1975, NRC 1995). There is also the issue of calculating an average from a highly variable small sample size, from populations that are already low enough to be threatened with extinction. It is no surprise that the regressions used in the Dennis models to estimate extinction risks have very low R^2 (less than 0.1—meaning that these regressions explain less than 10% of the variation in the data).

3. Why use 1980-1990 brood year data for the analyses when previous analyses used 1980-1993?

NMFS recently revised both the extinction and matrix models in the 12/13/99 spreadsheets available on the Web (<http://research.nwfsc.noaa.gov/CRI>). It is not clear why these analyses were revised using only the 1980-1990 brood year data⁶, instead of the full available range that had been used for previous analyses⁷. Performing analyses only on earlier data produces more optimistic results because, as noted above, population growth rates have continued to decline: using only data from 10-20 years ago produces lower extinction probabilities, and higher λ s (Table 3).

Table 3. Comparison of CRI extinction and matrix model results using early versus full

range of data available⁸.

Data used:	Extinction model λ		10-yr probability of extinction		Leslie matrix λ	
	Original: 1980-1998	Revised: 1980-1994	Original: 1980-1998	Revised: 1980-1994	Original: 1980-1998	Revised: 1980-1994
MAR	1.26	2.43	0.149	0.001	0.898	0.898
JOH	1.08	1.28	9.00E-04	1.00E-04	1.017	1.064
IMN	1.00	1.05	3.78E-06	9.00E-06	0.926	0.966
BEA	1.16	1.21	1.19E-02	3.00E-03	0.939	0.948
POV	1.10	1.13	1.75E-04	6.93E-07	1.034	1.034
SUL	1.48	1.56	0.102	0.071	0.983	1.020
MIN	1.41	1.4	0.037	0.042	0.861	0.990
Geomean	1.201	1.385	0.0033	0.0005	0.949	0.987

Basing calculations on data from more than a decade ago, and then using the results to estimate 10-year extinction probabilities starting from today, produces misleading results. The 10 years for which the chance of extinction is cited in Table 3 end this year. To be accurate, if there is some reason why only the older data can be used, then the CRI should be giving the 20-year probability of extinction in order to estimate how these populations will fare in the coming decade. In addition, the CRI should also acknowledge that the probability of "quasi-extinction" (as NMFS defines it) for two of the seven populations is actually 100%, because Marsh and Sulphur populations have both hit zero twice since 1990.

4. Why does the CRI rely on a questionable sensitivity analysis method?

The CRI reports include an example sensitivity analysis using the standard textbook method, which they subsequently discard, relying instead on a different, questionable method for their conclusions. The conclusions reached by the CRI using this questionable method play an important role in the "A-Fish" Appendix of the DEIS (USACE 1999).

The standard textbook way of doing sensitivity analysis is called the elasticity method. When applied to the CRI matrix models, it indicates that mortalities affecting the adults have the most impact on the results. The method preferred by NMFS, which is essentially the standard percent method, indicates that mortalities affecting fish in the first year of life are the most important, and that each subsequent year of life is less important. In effect, the two methods find nearly opposite results.

The problem with the standard percent method favored by NMFS is that it fore-ordains results: in a linear model like these Leslie matrices, the model will end up being most sensitive to the variable that has the largest mortalities. If mortalities are highest in early stages, then the model will be most sensitive to early stages. It is thus not actually necessary to carry out the "standard

percent" sensitivity analyses, because the results are determined by the magnitude of mortality in each stage. For this reason, NMFS' preferred method is rejected by a standard textbook on matrix models cited by the CRI (Caswell 1989). Caswell argues that the constant percent method, though simple, fails to provide any insight into sensitivity independently of the particular values assumed.

Caswell advocates using instead what he says has long been the textbook standard approach to Leslie matrix sensitivity, namely the elasticity method—the same method included as an example and subsequently discarded by the CRI. Caswell says the elasticity method has the great advantage of providing unbiased information about the relative sensitivities of Leslie matrices to any changes in the matrix. As the CRI acknowledges, the elasticity method produces results consistent with expectations about the relative importance of post-Bonneville survival. Nonetheless, the discussion and conclusions in the "A-Fish" Appendix of the DEIS (USACE 1999) about the relative appeal of different courses of action, are all based on the method which their own reference source (Caswell 1989) finds unsatisfactory.

It is important to note that the elasticity method itself is far from perfect and can also be misinterpreted and misused (Mills et al. 1999, Crooks et al. 1998). The elasticity method tends to highlight life stages with high survivals, and the standard percent method tends to highlight life stages with low survivals. Because sensitivity analysis results can easily be biased by parameter definitions and analysis methods, extending such results to conclusions about the real world and what courses of action should be taken requires assuming (1) that the models themselves represent the current, as well as future, real world; and (2) that the sensitivity analyses represent the way the models and the future world would respond. Perhaps the most disturbing aspect of the sensitivity analyses conducted in the CRI report is that these simple models are treated as though they directly represent reality, and that sensitivity analysis represents the way the real world would respond. Ordinarily, a model would have to undergo extensive validation, sensitivity analysis, and calibration before such conclusions could be justified.

5. Why does the CRI underestimate post-Bonneville mortality, and over-estimate first year mortality instead of using values from available literature and PATH?

The CRI assumes post-Bonneville survivals that are about 4-fold higher than have been observed in recent years, and survivals to the first birthday (through the rearing stage) that are about one-fourth of what they are generally believed to be. These assumptions play an important role in NMFS' claims that the hydro system is now having negligible impact on these populations.

Smolt-to-adult return (SAR) values used by the CRI are much higher than available literature would support (e.g., Lindsay et al. 1982, Lindsay et al. 1989, Mullan 1987, Mullan et al. 1992), and certainly higher than the values estimated by PATH (Bouwes et al. 1999) or the Corps of Engineers (USACE 1999). Smolt-to-adult return estimates, age adjusted round trip to below Bonneville, have most recently been around 1% (Bonneville Power Administration et al. 1999); but the CRI models assume close to 4%.

Because they overestimate post-Bonneville survival, the CRI models have to put the overlooked mortality somewhere, and NMFS chose to put it in first year: what the CRI calls S_1 . Again, the

numbers NMFS is using are not consistent with available data: CRI estimates for S_1 range from 1.4% to 3.1%, whereas values in the literature are much higher.

These two parameter problems lead to a problem with the conclusions NMFS and the Corps of Engineers are citing from the CRI: NMFS claims that sensitivity analyses show that first-year survival (S_1) is the most important parameter, and that estuary survival (S_e) is less important. When S_1 and S_e are corrected so that the values are consistent with available data, the rank-order reverses: interestingly, with these corrections, CRI results come more into line with PATH. When the best available data are used, delayed or "extra" mortality, which occurs in the ocean, turns out to be the most important variable, instead of first-year survival as is argued by NMFS.

6. Why has there been no discussion of model validation?

It is baffling that model validation has not yet been mentioned in any of the CRI reports nor in the DEIS (USACE 1999). If these reports were submitted to peer reviewed journals, surely they would have to compare model predictions to actual data, to discuss the impacts of errors and assumptions, and to compare methods and results to other models. Because these models may be used to justify delaying a very risky decision, they should at least have to pass the same muster that would be required for peer-reviewed publication.

There are excellent standard textbook methods readily available, such as those recommended by Hilborn and Mangel (1997) and Forrester and Senge (1980). Hilborn focuses on how to determine the relative acceptability of potential model structures, given limited and ambiguous data; Forrester focuses on how to demonstrate the reasonableness of the model structure, assumptions, and results, to a skeptical audience. The CRI does not even acknowledge that any kind of validation might be needed.

A consistent theme in the CRI is the argument that simple models are better than more complex models there is not enough data. Just because a model is simple, however, does not mean it is valid. In many cases it may be, but then again, other simple models may actually be better—or they may not. There are certainly other simple models available, and simply announcing that NMFS does not have much time, therefore this simple model will have to do, does not constitute a very convincing argument for model validity. A linear model such as $Y = aX + B$ is also simple, but it, like the simple models used in the CRI, would need to be confronted with data in order to evaluate whether it is legitimate for the problem at hand (Hilborn 1997). In particular, Mundy (1999) applied an equally simple model to the same populations and produced narrower confidence intervals and gloomier results (which have also turned out to be more accurate), partly by focusing on how the population growth rates are changing. Since the CRI is, after all, a risk assessment, it seems important to consider such an approach.

There is very little explanation of how choices were made for parameter values. The CRI reports often do include descriptions of *what* the choices are, but rarely provide discussion of *why* those choices are valid. Model sensitivities often depend on how variables are defined, and the sensitivity analysis method favored by NMFS essentially does little more than rank variables by how mortalities are partitioned in the model. Any peer-reviewed journals would most likely require more discussion of how and why these choices are made, and how results would change if choices were made more consistently with the way it has been done in other models such as

PATH.

Finally, given that these are being used as predictive models, with time periods as long as 100 years, it is also important to evaluate error propagation. One problem, of course, is that, because of dam construction, only data since 1980 is being used, and the data that are available are highly selective and spotty. When data availability is so problematic, one approach is to use weight-of-evidence methods such as were used in PATH. Another textbook decision analysis approach for dealing with a scarcity of data is to focus on how different plausible scenarios might affect the rank-ordering of alternatives. This approach, which was used by PATH, has the advantage of being not only analytically defensible but also intuitively appealing (Keeney 1976, Keeney and Raiffa 1992, Marmorek et al. 1998, NRC 1995, von Winterfeldt and Edwards 1986). The CRI has so far failed to include any analysis of the potential impacts of errors.

7. Why is the CRI not using textbook risk assessment methods?

The CRI is supposed to be a *risk* initiative. Yet, other than a questionable method for sensitivity analysis, and some probability distributions based on the estimated error in a regression, it uses almost no standard risk assessment tools. Standard risk assessment consists of analyzing trends, sources of error, and uncertainties; but trends, sources of error, and uncertainties are not what is analyzed in the CRI. Risk assessment, particularly risk assessment for ESA-listed populations, is not supposed to be carried out using means or medians (Boyce 1992, Burgman et al. 1993, Derby and Keeney 1990, Fischhoff et al. 1990, Holling and Clark 1975, NRC 1995): as the ISAB pointed out, risk is about trends and environmental variability, not measures of central tendency.

The structure of the Leslie matrix models is the basic salmon life-cycle, and that structure can easily be used in a stochastic (e.g., Monte Carlo or Latin Hypercube) simulation to produce response variables that do not rely on assumptions of population and environmental stability, and which can be compared to actual data, such as spawner population sizes over time, survival rates for different life stages, the rate of change of Recruits/Spawner (R/S), or a five-year running geometric mean R/S.

For both the extinction analyses and the management analyses, the CRI relies on a single response variable, λ , which is not representative of the populations being analyzed. Because the CRI's assessments of how long these populations have before they go extinct, and what it would take to save them, are based on average population growth rates from the 1980's, they imply that these populations are in no more serious trouble today than they were in the early 1980's. Ultimately, this simplistic assumption creates a high burden of proof for demonstrating that relying on this single response variable, λ , is a valid way to do risk assessment.

Conclusion

NMFS argues that because time is short, it is important to use standard, well-established methodology. They are indeed using some elementary textbook methodologies, but they are violating the requirements for valid application of those methodologies. Values assumed for key variables are not consistent with the best available data: post-Bonneville survivals used in the CRI are too high, and first-year survival values are too low. The central analyses on which the

conclusions are based rely on a sensitivity analysis method which is not recommended by the textbook which is cited. When survival estimates consistent with available data are used, results are significantly different from the results cited in the EIS Anadromous Fish Appendix (USACE 1999), but more consistent with PATH: post-Bonneville survival, and thus possibly delayed impacts from the hydro system, moves to the top of the list, rather than first-year freshwater survival.

NMFS should indeed be using standard, textbook risk assessment methodology. Risk assessment is not about averages, medians, or other measures of central tendency, particularly when the measures of central tendency are themselves declining. There is a long history in fisheries management of model-driven decisions turning out to be overly optimistic, and part of the reason for these repeated failures is that the role of environmental trends and variability has been underestimated. Snake River salmon are finally at the point where the trends seem obvious enough that models might actually detract from understanding the problem and figuring out what needs to be done. Nonetheless, given the success record of model-driven fisheries management decision making, it seems clear that if models must be used to evaluate risks to these populations, surely they should err on the side of caution, rather than optimism.

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¹ Note, however, that Musick, who was summarizing current work by the American Fisheries Society committee that is trying to define extinction risks for marine fishes, finds that, "While these [IUCN] criteria are effective at flagging rapid population changes in the short term, they grossly overestimate the extinction risk for many if not most marine fish species" (Musick 1999 p. 7).

² PATH (Plan for Analyzing and Testing Hypotheses) was initiated by NMFS and BPA in the wake of the Idaho vs. NMFS suit as a means of addressing differences in management advice generated by different groups with competing modeling frameworks. PATH membership represents the full range of scientific views and hypotheses that have surfaced. The primary objective of PATH was "to identify, address and to reduce uncertainties in the fundamental biological issues surrounding recovery of endangered spring/summer chinook, fall chinook, steelhead and sockeye stocks in the Columbia River Basin" (Marmorek 1998).

³ Note that PATH did not consider these to be quasi-extinction levels, but rather levels below which there is greater risk, and where less is known about population response.

⁴ The most recent extinction and matrix model analyses posted on the CRI website have revised all the calculations in order to only use 1980-1990 data, omitting the 1991-1993 brood year data used in previous analyses. See question #3.

⁵ To be fair, in 1998, the previous year, the estimated spawner count for Marsh Creek was 164. The overall downward trend is clear, but the variability is also large—which actually increases the chance of extinction, as noted by the ISAB and others.

⁶ In order to calculate recruits per spawner, analyses for brood years 1980-1990 would include 1991-1995 returns.

⁷ In order to calculate recruits per spawner, analyses for brood year 1980-1994 would include 1995-1999 returns.

⁸ The Leslie matrix λ s shown on the models currently posted on the CRI website are very nearly the same for both analysis periods for Marsh Creek and Poverty Flat, but there appear to be errors in some of the calculations. NMFS says they will have corrected models available sometime in January.