Reviewer: Jeremy Collie
Title of Paper: PATH Final Report for Fiscal year 1998
Section: 1.0 Executive Summary and 2.0 Spring/Summer Chinook

Comments:

a) scientific soundness of the methodology

p 31. I like the use of CART diagrams. However, the branches are inconsistently labeled, in that not all the hypotheses are included (e.g. B1).

p. 43. Eqn. 1 is ad hoc. I understand that to keep survival less than unity, the improvement was applied as a reduction in the mortality rate (*A*). A more straight-forward way would be:

$$S_{improved} = 1 - A(1\text{-Proportional Reduction})$$

where *A* is the fractional mortality rate.

b) general suitability of the data for use in the analyses

c) validity of inference and conclusions reached

p 13. In all the prospective models, there is a tendency for the spawner-recruit projections to be over-optimistic. The possibility of over-escapement may also be overstated. If and when the stocks recover we may find that overcompensation does not occur.

p 19. Are assumptions of the hydro-regulation model consistent with climate assumptions in the life-cycle model? The same climatic conditions (e.g. PAPA drift and Astoria flow) will affect hydro conditions (river flows) and marine survival. Boxes 10 and 12 in Fig. 2.1.1-1 must be consistent. Certain combinations of conditions would be inconsistent. For example we would not expect river flows to vary randomly if the marine climate were cyclical. These linkages should have been identified in retrospective modeling. I am not sure they have been accounted for in the prospective modeling.

p. 41: The qualitative evaluation of A6/A6' concentrated on potential improvements in passage survival, and concluded that A6 would probably perform worse than A2. However these comparisons did not consider the delayed mortality of transported smolts, which would not occur under A6. I think it might be useful to re-evaluate A6 with no transportation, flow augmentation, but without the major system improvements, which are difficult to quantify. Life-cycle modeling would be straightforward because there would be no transportation. I would not expect the performance of A6 to differ much from A2, but it could be a useful standard to compare with A3.

d) suggestions for improvements and extensions to the analytical approaches used

e) opportunities for integration of the different component analyses into an adaptive management approach

Table 2.2.4-2: Note that action A3 with the 3-year delay consistently outperforms A3 with the 8-year delay and B1, illustrating the benefits of taking action sooner rather than later.

f) relative priorities for future work on these analyses

p 13. There is an opportunity to learn from the spring/summer chinook analyses to simplify the fall chinook analyses. All the hypotheses that were found to be unimportant could be eliminated from the fall chinook prospective modeling to obtain a much more simplified modeling framework.

p 55. How was the relationship between habitat protection and the Ricker *a* value established? Unless quantitative analyses of this relationship can be established, I would give evaluation of alternative habitat scenarios a low priority. The effects of the alternative habitat scenarios are relatively small and they lead to some counter-intuitive results because of the variable harvest rates.

p 65. Additional sensitivity analyses for spring/summer chinook are not warranted at this point. Hypothesizing additional sources of mortality will not change the ranking of actions unless that mortality acts differentially on certain groups of fish. Prospective modeling has shown conclusively that increased transportation is very unlikely to increase survival. Any additional mortality on transported fish would only strengthen this conclusion.

Reviewer: Jeremy Collie
Title of Paper: PATH Final Report for Fiscal Year 1998
Section: 3.0 Fall Chinook

Comments:

This chapter provides a comprehensive treatment of fall chinook, including passage data, run reconstruction, retrospective and prospective model results. It is useful to scrutinize the components of the passage models before embedding them in life-history models and to resolve as many of the outstanding issues as possible.

a) scientific soundness of the methodology

p 75: What is the mechanism whereby spill effectiveness (SS) is greater than one? Eqn. 2 does not asymptotically approach 1 as P_w approaches 1.

p 79: It is surprising that the transportation survival of chinook salmon has never been measured, considering that most of the juveniles are now transported. The assumed value of 0.98 will certainly affect estimates of *D*.

p 80: The tagging estimates of reach and project survivals seem quite low, especially in 1997.

p 104: Note that the natural log of the ratio of observed R/S to the predicted R/S is equivalent to the difference between the observed log R and predicted log R because S is the same for the observation and prediction. Survival rate indices do not provide a time series of density independent **mortality** estimates.

p 118: Note that 0.98 is the assumed direct transport **survival**, not **mortality**.

p 121: Table 3.2.1-1 has two β_{MIN}s and no T_{RLS}.

p 127: I have always questioned modeling survival as a function of distance traveled, not time. In the FTT-WTT relationship did you consider a parametric relationship with temperature in the intercept, rather than splitting the data into two time periods? There is no mention of the FLUSH model being calibrated to reach survival data. Note that for both CRiSP and FLUSH, the tagging data measure the travel times of the **survivors**. If there is a distribution of fish travel times, the faster migrating fish may experience higher survival. If so, travel times and survivals cannot be compared independently with the tagging results. It is not necessary to model the migration of each individual smolt but it may be necessary to introduce a distribution of travel times.

p 129: Eqn. 3.2.1-11 has two parameters, *b* and α which are not described in the text. I would expect this equation to be normalized for prey abundance if it is a predation mortality. I would also expect a summation over predators.

p 134: I was relieved to see that there is only one life-cycle model for fall chinook [Eqn. 3.2.2-2]. However, this is not a parsimonious model. I would like to see its fit compared with a simpler model in which passage mortality is simply a function of the number of dams encountered or WWT.

p 136: Ideally the year effect would be common to all four of the fall chinook stocks, perhaps reflecting oceanographic conditions in the North Pacific. Was a model of this form tried? Presumably only the Deschutes and Snake River stock had shared year effects. I am not sure that the shared year effect is very useful if it only applies to two of the stocks. Is there an a priori reason that these two stocks should be more correlated?

p 137: In prospective models, the water years are chosen randomly from a historical distribution. The water regime in a given year is affected by climatic conditions which are also reflected in regime shifts and affect the maturity schedule. The treatment of climatic influences should be consistent within the model. The regime-shift hypothesis should be rejected, based on the Weight of Evidence Report. Climatic patterns can be modeled with an autocorrelated version of Eqn 3.2.2-3. The same autocorrelation pattern could be used in the random selection of water years.

b) general suitability of the data for use in the analyses

It is crucial that these run reconstructions be correct, since they form the basis of the life-history modeling. The run-reconstruction methodology is confusing (p 100-103) because recruits are defined at the mouth of the Columbia River, yet an attempt is made to back calculate the effect of ocean harvesting. To do so it is necessary to specify the ocean survival rate and catches. It might be more straightforward to specify recruitment at a reference age (e.g. 3), rather than by different age groups at the mouth of the Columbia River. A flow chart of the run reconstruction might help other reviewers to understand the procedure (cf. Hankin and Healey 1986, CJFAS 43:1746-1759).

c) validity of inference and conclusions reached

p 122: Some of the component relationships upon which CRiSP is constructed are weak. Direct dam mortality is assumed constant; variations in mortality are attributed to predators. It is possible that some of the year-to-year variability occurs in dam passage, which could account for the lack of relationship between observed and modeled survival in Fig. 3.2.1-2.

p 123: Eqn. 3.2.1.4 should be $S_T = S_M \cdot S_R$. This assumption has important consequences for the life-history modeling. Since the time spent migrating is only 15% of the total, most of the mortality during this stage is attributed to the rearing phase. I think it is this assumption that accounts for the large difference between the migration-only and migration+rearing CRiSP models.

p 131: The partitioning of direct dam mortality suggests that an increase in SS (I am not sure how this could be attained) could have a much greater effect on overall direct dam mortality than FGE because bypass and turbine survival is quite similar, whereas the spill survival is higher.

p 141: Hatchery supplementation is based on the natural spawning of hatchery reared fish. Do the hatchery spawners have the same productivity as wild chinook salmon, and if not, is productivity inherited by subsequent generations? The 1988 brood year which was entirely of hatchery origin had a high survival; we are now starting to see the second generation of these hatchery-reared fish. Declines in ln(R/S) for SRB chinook are apparent starting with brood year 1985, which is considerably later than the step functions introduced in the life-cycle model. FLUSH does slightly better than CRiSP in predicting the very low ln(R/S) in 1991. What happened in brood year 1968?

I agree that habitat changes are more likely to be reflected in Ricker *b* than *a*.

Section 3.3.1: The passage model results differ between CRiSP and FLUSH. The main differences seem to be that CRiSP predicts that a higher proportion of smolts were transported and assumes that more time is spent rearing than migrating ($T_M = 0.15T_T$). What is the explanation for the lower proportion transported in FLUSH? Do the smolts arrive at the dams too late to be transported? When migration and rearing are considered together, the passage models predict similar total survival rates (Fig. 3.3.1-3).

p 147: The estimates of the Ricker *a* value are very high to compensate for the extra mortality from the *M* and *STEP* terms. An *a* value of 4.5 corresponds to 90 recruits per spawner. Do these high productivities make sense if the extra mortality terms are turned off? I experienced similar problems with overestimating *a* with sockeye salmon. Are the parameter estimates in Table 3.3.2-3 sensible? If a+ln(b)=13.17 and ln(b)=-8.96, does this mean that a=13.17+8.96=22.13? Perhaps I misunderstood this table.

p 148: The estimated *D* values are generally low which, when multiplied by bypass and transportation survival, implies that the total survival of transported smolts is lower than that of in-river migrants (Fig. 3.3.1-1). The *D* values are later adjusted upwards, but even so, the survival advantage of transported smolts is minimal at best. I recommend dropping the *D*-value formulation. It has been troublesome ever since it was introduced in life-cycle models, and there is little evidence that transported smolts have significantly different total survival than in-river migrants.

In Section 4.3, the 48-year recovery is the limiting standard. In Fig. 3.4.4-3, it is surprising that the CRiSP migration and rearing model predicts higher recovery probabilities. I understand that this scenario attributes more of the mortality to the rearing stage, but when the stages are combined, the cumulative mortality should be about the same magnitude as the migration-only scenario. I would not expect rearing mortality to be affected by action A2. I wonder if the rearing mortality was omitted from this graph?

In Fig. 3.4.5-7 the system survivals are considerably lower for the migration and rearing scenario which is opposite from Fig. 3.4.4-3.

Fig. 3.4.5-8: The FLUSH system survivals are considerably more variable from year-to-year than CRiSP. Is this because FLUSH is tied more closely to interannual differences in water flow?

d) suggestions for improvements and extensions to the analytical approaches used

Fig 3.1.2-3: The most striking feature of this figure is the large hatchery influence on the Snake River Brights starting in 1984.

Fig 3.1.2-6: Note the very low ln(R/S) for Snake River Fall chinook following the 1985 brood year. This decline seems to correspond temporally with high proportions of hatchery strays in the spawning population (Fig. 3.1.2-3). To test the hypothesis that hatchery-reared (h) and wild (w) chinook salmon have different productivities, I fit a modified Ricker model of the form

$$R = S(\alpha_w p_w + \alpha_h p_h)\exp(-bS)$$

where α is recruits per spawner at low stock sizes and p_h is the hatchery fraction read from Fig. 3.1.2-3. As hypothesized, $\alpha_w > \alpha_h$, but the additional parameter did not significantly reduce the sum of squares ($p>0.1$). Other factors, perhaps in addition to the hatchery fraction, are needed to explain the temporal pattern of survival rates. The extrinsic effects of hydro, hatchery, habitat, and harvest are considered in Section 3.2.2; it is worth considering that the productivity of hatchery strays may be less than of wild chinook salmon. The type of *F*-test I employed could be used as a simple test of whether the stock-recruitment fits on page 106 are significantly different between time periods.

e) opportunities for integration of the different component analyses into an adaptive management approach

f) relative priorities for future work on these analyses

Reviewer: Jeremy Collie
Title of Paper: PATH Final Report of Fiscal Year 1998
Section: 4.0 Analysis of Effects of Proposed Actions on Snake River Steelhead

Comments:

a) scientific soundness of the methodology

p 185: The section on In-River Juvenile Survival is confusing. In Table 4.7.1-5 where are the bold estimates. I would expect the exponent on Reach Survival to be 1/(Number of projects) not -(Number of projects).

p 188: Without the parentheses, assumption 2) says that survival of in-river migrants under the current operation are approximately 80% of the survival of fish passing in-river? I assume that the former includes the survival of fish that are bypassed but not transported. Please explain what is meant more clearly. The final sentence states that the range of survival differences is 3.6% to 10.5%. It is difficult to see how these percentages were obtained from Table 4.7.1-5. Are these percentages plus or minus; what are they percentages of? It might be clearer to discuss the straight differences in survival and not covert to %.

b) general suitability of the data for use in the analyses

p 171: It would be useful to start this section by describing the present status of Snake River steelhead. Are they listed? It would be useful to plot the escapement of the aggregate ESU if only for comparative purposes.

c) validity of inference and conclusions reached

Most of the comparisons made in this chapter suggest that the in-river survival of steelhead smolts is not expected to be significantly different than chinook smolts. Table 4.7.1-4 suggests that FGE may be higher for steelhead than for chinook. A smaller increase in survival is required for steelhead to achieve SARs from the reference period in 1964-69. These qualitative analyses suggest that, given appropriate management actions, Snake River steelhead should recover no slower, and perhaps more quickly, than chinook salmon.

f) relative priorities for future work on these analyses

p 200: I question the usefulness of detailed passage modeling without stage-specific data to validate the passage models. In the absence of spawner-recruit data, survival standards for steelhead may need to be based on smolt-to-adult survival (SAR).

Reviewer: Jeremy Collie
Title of Paper: PATH Final Report of Fiscal Year 1998
Sections: 5.0 Sockeye, 6.0 Experimental Management, and 7.0 PATH Analyses in FY99

Comments:

a) scientific soundness of the methodology

b) general suitability of the data for use in the analyses

c) validity of inference and conclusions reached

p 211: Hydro management actions ought to benefit sockeye salmon, provided there are actually Snake River sockeye salmon in the river system. Many of the in-river survival studies have been conducted with the more numerous hatchery chinook salmon. It is important to assess whether these studies would apply equally well to sockeye salmon smolts. For example, descaling rates seem to be higher for sockeye than for other salmonids.

p 216: Active adaptive management is better than passive adaptive management in the sense that it is always better to account for learning about uncertain parameters. If the best passive adaptive policy (action) is quite different than the status quo, learning rates about uncertain parameters may be sufficiently fast that a more experimental policy (action) is unnecessary (Collie and Walters 1991, 1993).

Table 6.2-2: From the previous PATH analyses, the key management indicators are known (Step 4).

Table 6.3-1: I think that this table of possible experimental manipulations is very useful. The 2-pool drawdown option has merit provided that it is started soon. If delayed it would not be sufficient to meet the survival standards. A 2-pool drawdown would be very informative about two of the key uncertainties in Table 6.2-3: the length of the transition period to equilibrium conditions and juvenile survival rate after drawdown. A 2-stage implementation of the 4-pool drawdown could also reduce temporal confounding of factors affecting survival by virtue of its "staircase design" (Walters et al. 1988, CJFAS 45:530-538).

It may be impractical and risky to turn hatchery production on and off for periods of time. Impractical because of the need to maintain brood stock and risky because the predator populations in reservoirs could inflict a depensatory predation rate on wild salmon smolts in years of low hatchery production. If depensatory predation is a risk, hatchery production may need to be reduced more gradually. The option of Intensive hatcheries and Intensive/reduced transportation may be more feasible for resolving the confounding between transportation and hatcheries.

The options in this table consider mainly temporal comparisons, such as intensive/reduced transportation and hatcheries. The spatial scale of experimentation and the possibilities for spatial contrasts should also be considered. The spatial scale is generally quite large and involves comparisons between Snake River stocks and other Columbia River stocks. However, it may be possible to establish up-river/down-river comparisons, e.g. by transporting smolts only from the up-river dams and allowing lower river stocks to migrate in the river.

Fig. 6.4-1: I agree that the SRP did recognize the need for simpler life-cycle models for evaluating experimental options.

d) suggestions for improvements and extensions to the analytical approaches used

e) opportunities for integration of the different component analyses into an adaptive management approach

p 223: PATH could calculate the Expected Value of Perfect Information for the key uncertainties in Table 6.2-3. These calculations would suggest how much it is worth to resolve key uncertainties. PATH could also extend the prospective models to simulate the collection of new data and thereby the rate of learning about uncertain hypotheses. The methodology for this type of simulation is outlined in Walters (1986) book; example applications are (Collie and Walters 1991, 1993). The general question is "If a certain hypothesis is correct (e.g. equilibrated juvenile survival rate) how long would it take to detect it under the different actions. These types of simulations determine how much of the EVPI is realistically attainable.

f) relative priorities for future work on these analyses

I give a higher priority to the experimental management tasks than to further sensitivity analyses. I am afraid that additional sensitivity analyses of new factors may delay the implementation of management actions, and thereby make the survival standards more difficult to attain. Proceeding with the experimental management tasks will focus attention on key uncertainties in the life-cycle models, and also on the types of monitoring that will be required to measure the performance of management actions.

[Editor's note: This document provided 4/25/99 by Dave Marmorek for posting on the PATH web site.]