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Comment

Cost-Effective Management of Snake River Chinook Salmon: Response to Wilson et al.

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Introduction

Management decisions related to recovery of Snake River spring- and summer-run (SRSS) chinook salmon should be informed by sound science. The Comment by Wilson et al. (2009 [this issue]) has the apparent purpose of discrediting the quality of our analysis in Halsing and Moore (2008), such that the results would not be used to inform decisions. But we agree on use of our results: although our methodology is sound, the results should not directly guide such decision making. We state this at important points in our paper: in the abstract,

> Application of our results to salmon management is limited by data availability and model assumptions, but these limitations can help guide research that addresses critical uncertainties and information (p. 338);

in the final paragraph of the introduction

> Direct application of the results to salmon management, however, is limited by data availability and model assumptions (p. 340);

and, in the final paragraph of the paper,

> This approach can be utilized by resource managers with responsibility for salmon recovery as new evidence becomes available on the biological effectiveness and economic cost of additional management measures (p. 349).

Our stated purpose was to develop and demonstrate a methodology for linking biological and economic models to address SRSS chinook salmon management. Perhaps Wilson et al. did not understand our intentions or believed they needed to reinforce the above points based on concern that managers would apply our results. We believe, however, that the Wilson et al. comment is very misleading and quite strained in its effort to depict our analysis as inappropriate for a purpose for which it was never intended.

Biological Modeling

New Data and Models in Recent Literature

Our paper notes that the manuscript was originally submitted in 2005. In the review process, there were several unusual delays during which time new models and results were developed. Although we made good-faith efforts to incorporate these results, Wilson et al. cite 8 papers published from 2006 through 2008 that include information on freshwater recovery measures and models that could replace the CRiSP model in our methodology. We believe that the expectation to incorporate results from virtually contemporaneous publications is an unreasonable standard.

D Value

Wilson et al. also criticize our use of relatively large D values ($D = 0.7, 1.0$) in the analysis. Their point is quite misleading because it ignores both our use of $D = 0.553$ (Williams et al. 2005) for the primary results and the clear statement that the other values were for a sensitivity analysis assessing the relative importance of this parameter. For example, 2 tables and one figure report results for $D = 0.553$, whereas only one Table reports results from the sensitivity analysis with a range of D values.

Other Issues in Biological Modeling

Wilson et al. question whether we used age-dependent fecundity in the matrix models. We did use it, and our paper notes that we used values from Kareiva et al. (2000) in doing so and generated baseline results for λ that were nearly identical to their results. Nevertheless, in

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transferring information for publication, we regrettably entered the wrong information, thus creating this confusion.

Wilson et al. also question the role of latent mortality in our analysis, which we limited to alternatives with dam breaching. Their point is that because of the uncertainty in "the degree to which and mechanisms by which the hydrosystem causes latent mortality of juveniles in the estuary early-ocean life stage," it is inaccurate to limit the analysis in this way. They note that other measures to speed smolt migration may also reduce latent mortality. Although we do not dispute the point, we note that latent mortality is commonly described as latent or delayed mortality attributed to fish passage through the hydrosystem (e.g., Boyce et al. 2006). This suggests that impacts of collection, spill, or other effects of hydrosystem passage are the primary cause of latent mortality and that these effects are best assessed in analysis of breaching, as we did in our paper.

Finally, Table 5 in our paper contained numerical errors, as Wilson et al. note. This was caused by mistakenly copying one column of numbers from one spreadsheet into another to formulate Table 5. The values in Halsing and Moore's Table 4 are correct. We regret the error and have included a corrected table here (Table 1).

Table 1. Corrected version of Table 5 in Halsing and Moore (2008).*

Alternative number	*λ*	*Cost ($)*	*Δλ*	*Δcost ($)*	*Δcost/ Δλ*
A3	0.861	−11.211	N/A	N/A	N/A
C3	0.867	−10.986	0.006	0.225	37.52
B3	0.873	−6.906	0.006	4.080	674.18
D3	0.879	−6.681	0.006	0.225	36.98
A2	0.891	−2.250	0.013	4.431	349.49
C2	0.898	−2.025	0.006	0.225	36.15
B2	0.904	2.055	0.006	4.080	649.59
D2	0.910	2.280	0.006	0.225	35.63
D7	0.911	34.956	0.000	32.676	88,500.81
D10	0.915	172.259	0.005	137.303	29,715.42
A16	0.964	207.306	0.048	35.047	724.49
C16	0.970	207.531	0.007	0.225	33.30
B16	0.977	211.611	0.007	4.080	598.27
D16	0.984	211.836	0.007	0.225	32.81

**Cost-effective management alternatives and their marginal analysis for the Minam River index stock of Snake River spring-summer run chinook salmon for* D = *0.553. Management alternatives are defined in Table 1 and in Methods section of text. Annual Cost is in millions of 2003 dollars. Lambda (λ) is the mean annual population growth rate arising from a management alternative. The* Δcost *and* Δλ *are changes in annual cost and growth rate, respectively, of moving between cost-effective alternatives. The* D *is delayed differential transportation mortality, the ratio of survival-and-return rates for smolts that were transported around dams to those that migrated downstream in the river.*

Cost-Effectiveness Analysis

Executing the Cost-Effectiveness Algorithm

Wilson et al. question how we chose the growth-rate constraints for the cost-effectiveness algorithm. Although an economist would understand the algorithm, we appreciate the opportunity to describe its application in more detail. Begin with the constrained minimization problem. We found a cost-effective alternative by choosing a solution for

$$\min_{i\in I} c_i \text{ subject to } \lambda_{\mathrm{i}} \geq \lambda^0, \tag{1}$$

where $I = \{i \mid i = 1, \ldots, n\}$ is the index set of n alternatives, c_i is cost of alternative i, λ_i is mean annual population growth rate of alternative i, and λ^0 is a growth-rate constraint. We repeatedly reset λ^0 and applied Eq. 1 until the set of cost-effective alternatives was found for the n alternatives.

Halsing and Moore analyzed 76 alternatives, with each defined by a specific alphanumeric code and characterized by values of λ and cost. Wilson et al. compare alternatives A3 and A5. Alternative A3, which is cost-effective, has a λ-cost pair of 0.861, -$11.211 million. (A negative cost is a benefit.) Alternative A5 has a λ-cost pair of 0.860, -$10.349 million, and alternative A4 has λ-cost pair of 0.856, $0.863.

Now consider the problem of choosing the alternative to

$$\text{minimize } c_i \text{ s.t. } \lambda_i \geq 0.856. \tag{2}$$

Alternative A3 solves this optimization because it is the lowest cost alternative that meets or exceeds λ of 0.856. Alternatives A4 and A5 are inferior to A3.

The algorithm's next application uses a growth-rate constraint that is the observed λ value just higher than A3's level of 0.861; this is 0.862. Consider the problem of choosing the alternative to

$$\text{minimize } c_i \text{ s.t. } \lambda_i \geq 0.862. \tag{3}$$

Alternative C3 solves this and thus is a member of the cost-effective set.

The optimization's next constraint is the observed λ value just higher than C3's λ: 0.868. The process of repeatedly applying Eq. 1, while changing the constraint, continues until the range of λ values is considered and the set of cost-effective alternatives is found.

In a second point, Wilson et al. argue that use of λ values to the thousandths place is imprudent and should be replaced by values to the hundredths place. They provide a conceptual argument related to the technique and an empirical argument based on experience with these models in the salmon context. We note, related to the conceptual argument, that Caswell's (2001) textbook reports λ values to 3 or 4 digits past the decimal place (10 thousandths). Thus, reporting λ to the thousandths seemed prudent.

Finally, Wilson et al. conducted a cost-effectiveness analysis with λ values rounded to the hundredths place.

The results are in their Table 1. One would expect, in light of the different rounding, they would find different alternatives in their cost-effective set. They did. Yet they also incorrectly conclude that alternatives C17 and B17 are cost-effective solutions. Referring to their Table 1, note that A16 dominates both C17 and B17 by achieving a higher λ at a lower cost than C17 or B17. More formally, A16 solves the problem of choosing the alternative to

$$\text{minimize } c_i \text{ s.t. } \lambda_i \geq 0.93. \qquad (4)$$

Economic Benefits of Dam Breaching

Wilson et al. claim we "applied inconsistent economic analysis assumptions across scenarios" and "used inconsistent accounting of costs and benefits between alternatives." This is a strong, yet misleading criticism. Their point boils down to one instance—and not a general inconsistency—which involves the fact that we did not explicitly count the economic benefit of a free-flowing river when reporting the cost of dam breaching. Instead, we reserved the inclusion of this particular benefit to the paper's Discussion (which Wilson et al. fail to mention). There, we state that economic benefits would accrue from recreation on or aesthetic qualities of a free-flowing river and that these benefits, estimated by Loomis (2002) at up to $311 million annually for the recreation component, could be subtracted from the costs of dam removal in the cost-effectiveness analysis (Halsing & Moore: 349).

Why did we handle these benefits in this way instead of incorporating them into the earlier analysis? First, we did not want the analysis of breaching to become entrenched in the social controversy surrounding benefits of breaching. This is, in part, why we conducted a cost-effectiveness analysis, not a benefit–cost analysis. Second, the methodology applied by Loomis (2002)—involving survey questions on hypothetical behavior—applies "stated preference" methods rather than "revealed preference" methods. The former use data on hypothetical human decisions, whereas the latter use data from actual decisions. Most economists recommend use of stated-preference methods only for estimating benefits of passive-use values (Portney 1994). The hypothetical behavior assessed by Loomis, however, involves recreational choices, which involve use values. Thus, the research is methodologically controversial.

In sum, the general statement of Wilson et al. about inconsistent assumptions is incorrect and ignores our discussion of recreational benefits.

Scope of Analysis

Wilson et al. criticize the fact that we analyze SRSS chinook salmon, but not other endangered or threatened species in the basin. Yet this is the first major limitation that we listed in the discussion (p. 348), where we discussed the broader effects of dam breaching on other species. We noted the existence of guidelines for allocating costs of multioutput water projects in benefit–cost analysis but not in cost-effectiveness analysis. We then posed several brief approaches to this cost-allocation problem. We were transparent in discussing this limitation and, moreover, conveyed the idea that solving this limitation is a challenging research problem in itself.

Conclusion

We have enumerated several ways in which the comments of Wilson et al. on our paper are misleading or incorrect when taken in context of the original paper. Yet we reiterate our paper's main point: integrating biological and economic models for cost-effectiveness analysis is a useful decision support tool. We look forward to research that adopts this framework, applies newer models and data, and produces results that can directly inform decisions on Snake River salmon.

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