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UNITED STATES DISTRICT COURT
DISTRICT OF OREGON
PORTLAND DIVISION

NATIONAL WILDLIFE FEDERATION, *et al.*,

No. 3:01-cv-00640-SI

Plaintiffs,

and

DECLARATION OF
FREDERICK E. OLNEY

STATE OF OREGON,

Intervenor-Plaintiff,

v.

NATIONAL MARINE FISHERIES SERVICE, U.S.
ARMY CORPS OF ENGINEERS, and U.S. BUREAU
OF RECLAMATION,

Defendants,

and

NORTHWEST RIVERPARTNERS, INLAND PORTS
AND NAVIGATION GROUP, STATE OF IDAHO,
STATE OF MONTANA, STATE OF WASHINGTON,
KOOTENAI TRIBE OF IDAHO, CONFEDERATED
SALISH AND KOOTENAI TRIBES, and
NORTHWEST POWER AND CONSERVATION
COUNCIL,

Intervenor-Defendants.

I, FREDERICK E. OLNEY, state and declare as follows:

INTRODUCTION

1. I worked as a fishery biologist for the U.S. Fish and Wildlife Service for 35 years, retiring on July 2, 2004, as Senior Scientist-Fisheries, Pacific Regional Office, Portland, Oregon. During the course of my career I have dealt extensively with the effects of Columbia River hydropower development on the Basin's salmonid resources and management of Columbia River salmon runs in general.

2. In December 1979, I was appointed Fisheries Technical Advisor and Chairman of the Fisheries Advisory Board for the U.S. District Court for the Western District of Washington. In that position I served as the Technical Advisor to Judge Walter E. Craig on fisheries conservation and management disputes under *U.S. v Washington* and *U.S. v. Oregon* until May 1982. Since 1982, my primary work has addressed Columbia River fisheries issues, including matters concerning the passage of salmon at the dams of the Federal Columbia River Power System ("FCRPS"). I have addressed such passage matters while serving as the Columbia River Coordinator for the U.S. Fish and Wildlife Service, as the Project Leader of the Columbia River Fisheries Program Office, and most recently as a Fish and Wildlife Administrator and Senior Scientist in the Pacific Regional Office. While serving as Project Leader of the Columbia River Fisheries Program Office from 1994-1999, I supervised a staff of about 20 full time biologists and directed their studies and activities, including work related to fish passage issues throughout

the Basin. I have B.S. and M.S. degrees in Fisheries from the University of Washington.

3. I have served as the Fish and Wildlife Service's primary representative in inter-agency fish passage forums in the Columbia River Basin. These include the National Marine Fisheries Service's Regional Forum (Technical Management Team and Implementation Team), Columbia Basin Fish and Wildlife Authority (Members Management Group, Anadromous Fish Committee, Fish Passage Advisory Committee, and Fish Passage Center Operations Committee), Northwest Power and Conservation Council/CBFWA Spill Subcommittee, and various fish passage committees advisory to the U.S. Army Corps of Engineers.

4. I previously have testified by written declaration in this proceeding on behalf of the National Wildlife Federation ("NWF") plaintiffs, including declarations in support of their motions for summary judgment regarding the 2008 BiOp, as well as in support of their motion for injunctive relief regarding that BiOp. *See* Declaration of Frederick E. Olney in Support of Plaintiffs' Motion for Summary Judgment (filed Sept. 19, 2008) ("Olney 2008 SJ Dec."); Reply Declaration in Support of Plaintiffs' Motion for Summary Judgment (filed Nov. 18, 2008) ("Olney 2008 SJ Reply Dec."); Declaration of Frederick E. Olney in Support of Plaintiffs' Motion for Injunctive Relief (filed Nov. 25, 2008).

5. I have also testified by written declaration on behalf of the four lower Columbia River Treaty Tribes (the Yakama, Warm Springs, Umatilla, and Nez Perce) concerning the biological effects of summer spill and comments on the 2004 Biological Opinion. *See* Declaration of Frederick E. Olney (July 16, 2004) (summer spill injunction 2004); Declaration of Frederick E. Olney (Mar. 21, 2005) (injunction motion summer 2005); Second Declaration of Frederick E. Olney (same) (May 16, 2005); and Declaration of Frederick E. Olney in Support of Motion for Further Injunctive Relief (Dec. 7, 2005) (spring and summer 2006 operations injunction).

6. I am currently self-employed as a consultant on fisheries issues and have been retained by the NWF plaintiffs in these proceedings. I have not been involved in any way as a government employee in the preparation of the biological opinion that is the subject of this declaration because work on it did not commence until after I retired.

7. I have reviewed the 2014 Biological Opinion on Operation of the Federal Columbia River Power System issued by NOAA Fisheries on January 17, 2014 (the “2014 BiOp”), including technical appendices and other related documents. I have also reviewed the 2008 and 2010 BiOps including the Supplemental Comprehensive Analysis and earlier Comprehensive Analysis and Biological Assessment prepared by the Corps of Engineers, Bureau of Reclamation, and Bonneville Power Administration and related documents. I am further familiar with and have reviewed previous biological opinions and related technical appendices and memoranda regarding the FCRPS and its operation following the listings of Columbia and Snake River stocks of salmon and steelhead.¹

DISCUSSION

8. In this declaration, I start by summarizing the approach and structure of the 2014 BiOp. I then address a series of issues that I have described in my prior declarations regarding the 2008 BiOp and explain whether and how these issues are addressed in the 2014 BiOp.

I. OVERVIEW OF THE 2014 BIOP AND ITS UPDATED ANALYSIS

9. NOAA explains that the 2014 BiOp is a “supplement” to the 2008 BiOp. 2014 BiOp at 31-32 (“this reinitiated consultation analyzes the revised RPA with continued reliance on the determinations of the 2008 BiOp in the context of current information regarding the species, environmental baseline, any cumulative effects, and past and prospective

¹ The papers and reports that I refer to in this Declaration that do not appear to be in the administrative record for the 2008, 2010 or 2014 BiOps, or the action agency Records of Decision are attached as exhibits to this declaration. There are only two such documents.

implementation of RPA actions”). The agency also explains that its supplemental opinion “was prepared to comply with the 2011 Court Remand Order.” *Id.* at 33. And NOAA “concludes that the section 7(a)(2) analysis of the 2008 BiOp remains valid, as supplemented in 2010, and further by the additional project definition, analysis, and revised RPA actions contained in this [2014] Supplemental Opinion.” *Id.* at 34.² The updated analysis for the revised RPA in the 2014 BiOp largely consists of two parts, an updated presentation of information about the status of the listed species that is set forth in chapter 2, *id.* at 43-224, and an updated discussion of RPA implementation that is set forth in chapter 3, *id.* at 225-458.

10. The 2014 BiOp does not apply a new jeopardy standard and does not alter the jeopardy analysis in the 2008 BiOp. For example, NOAA does not update its population-by-population quantitative prediction of the effects of the RPA, which it included as a central feature of the 2008 BiOp, by offering an updated base-to-current survival adjustment or an updated current-to-prospective survival adjustment for the revised RPA. *See Olney 2008 SJ Dec.* at 17-22 (describing these aspects of the 2008 BiOp jeopardy analysis). Rather NOAA’s logic in the 2014 BiOp is that if its assessment of the status of the species for the Base Period in the 2008 BiOp has not changed significantly in light of its updated analysis of an extended Base Period in the 2014 BiOp, if implementation of the 2008 BiOp RPA as modified is, in NOAA’s view, on track, and if, in the agency’s view, there are no reasons to re-assess the effects of specific RPA actions, then its prediction of the effects of the RPA on the listed species from 2008 remains valid and continuing to implement the RPA will be sufficient to avoid jeopardy. 2014 BiOp at 34.

² There are seven changes to the prior RPA addressed in the 2014 BiOp. Four of the changes affect spill operations at the FCRPS dams and transportation of juvenile salmon and steelhead during their out migration. One of the changes eliminates a mitigation action in the estuary, the pile dike removal program, and two others address actions to control predation (by northern pike minnows and cormorants, respectively). 2014 BiOp at 37-38 (listing changes).

II. THE UPDATED ANALYSIS OF THE STATUS OF THE SPECIES

11. The updated analysis of the status of the species in chapter 2 of the 2014 BiOp consists of a lengthy discussion of the “rangewide status of salmon and steelhead and designated critical habitat,” 2014 BiOp at 43-182, a shorter discussion of the environmental baseline, *id.* at 183-220, and a very brief discussion of cumulative impacts, *id.* at 221. As NOAA explains, its examination of the rangewide status of salmon and steelhead “reviews new information to determine if the updated status of interior Columbia basin salmonids differs from our understanding in the 2008 BiOp. If there is a change in the species status, a second step would be to determine if that change reveals effects of the action that may affect the listed species in a manner or to an extent not previously considered.” 2014 BiOp at 45.

12. The updated analysis of the species’ status addresses the same extinction risk metrics, population metrics, and jeopardy thresholds (less than a 5% risk of extinction in 24 years and population growth rates of at least 1.0 for the three recovery metrics) that NOAA reported in its quantitative jeopardy analysis in the 2008 BiOp. NOAA describes how it references these metrics and new information to update its analysis on pages 48 through 69 of the 2014 BiOp. This discussion includes pages 66-69 where NOAA explains how it evaluates whether the extended Base Period estimates of its various population status metrics, using new information, have changed from the 2008 BiOp’s Base Period estimates. As NOAA explains, “the primary method [it] uses to evaluate the Base Period versus the Extended Base Period indicator metric estimates is to determine whether point estimates for the various metrics have changed.” 2014 BiOp at 66. The agency goes on to explain that “[w]hile the comparison of point estimates is important, it does not provide a complete picture of the current status [of a population] relative to the estimates in the 2008 BiOp. Two factors that also must be considered are uncertainty in parameter estimates [i.e., in the point estimates for each metric] and the process of density dependence” *Id.* NOAA then discusses each of these factors. I briefly

describe NOAA's discussion of uncertainty below to provide background on this issue. I do not address the issue of density dependence.

13. With respect to uncertainty, NOAA notes that "the point estimates calculated for the 2008 BiOp Base Period indicator metrics tended to have fairly wide statistical confidence intervals, reflecting . . . uncertainty, as do the new extended Base Period estimates." *Id.* NOAA then explains that, "[i]f confidence intervals [for two point estimates of the same metric for the same population] overlap, particularly if the second point estimate falls within the confidence interval of the first estimate, [a statistical test] would not indicate that the metric has changed." *Id.* at 67 (emphasis added). NOAA further states that while the approach of determining whether a new point estimate falls within the confidence intervals for a prior point estimate "is a useful way of describing if a statistically significant change in a BiOp indicator metric *has* occurred, it may be of limited utility in determining that a change has *not* occurred." *Id.* (emphasis in original). NOAA does not describe or discuss in the 2014 BiOp other aspects or implications of the wide confidence intervals for its calculation of the Base Period (or the Extended Base Period) point estimates of the various population indicator metrics. Other relevant implications of these wide confidence intervals have been described and explained in the Declarations of Ed Bowles filed in support of Oregon's challenge to the 2008 BiOp and in the Declarations of Dr. Steven Orzack filed in support of NWF's challenge to the same BiOp. *See* Declaration of Edward Bowles in Support of State of Oregon's Motion for Summary Judgment at ¶¶ 51-64 ("Bowles 2008 SJ Dec."); Declaration of Steven Orzack, Ph.D., in Support of NWF's Motion for Summary Judgment at ¶¶ 8-16 ("Orzack 2008 SJ Dec.").

14. NOAA concludes that because of the uncertainty indicated by these wide confidence intervals, it does not "rely solely on results based on the relation of new mean (i.e. point estimates) to the confidence intervals of the previous estimates. . . . but rel[ies] on a

combination of all of the information described in this section in [its] determination.” 2014 BiOp at 67. NOAA summarizes the information it combines from section 2.1 on pages 129 to 134 for the interior Columbia basin salmon and steelhead populations. First, it notes that its updated analysis of metrics “provide[s] support for NOAA Fisheries’ continued reliance on the 2008 BiOp’s description of the rangewide status of these species and the Base Period metrics.” 2014 BiOp at 129. In other words, where NOAA’s calculation of Base Period metrics in 2008 produced point estimates for a metric that showed populations that were not replacing themselves, i.e., populations that were declining, the updated or Extended Base Period metrics cannot be statistically distinguished from a continuation of that trend.

15. Next NOAA summarizes the results of several reports it has made and a separate five-year status review of the listed species. As NOAA explains, these reports generally conclude that the status of the listed species has not changed: “the status of species and their constituent populations relative to those recovery goals is nearly identical to the recovery status in the 2008 BiOp as updated by the 2010 Supplemental BiOp.” *Id.* at 130 (for example, according to the five-year status review, “[o]verall risk ratings continued to be ‘high’ for all populations of [upper Columbia Chinook, upper Columbia steelhead, and Snake River spring/summer Chinook]”). NOAA then summarizes again the results of its Extended Base Period analysis of the 2008 BiOp population metrics as set out in the 2014 BiOp. 2014 BiOp at 131-32. It notes, as it had previously, that “[v]irtually all of the new extended Base Period estimates fall within the statistical confidence limits of the 2008 BiOp Base Period metric estimates” but it goes on to summarize, discuss, and draw conclusions about the populations based on a comparison of the updated Extended Base Period point estimates for these metrics to the original Base Period point estimates. *Id.* at 131-133.

16. The main conclusion it draws from this discussion of the new point estimates is

that they provide “‘strong support for the hypothesis that density-dependent recruitment is occurring in these populations’ and ‘strong support for the hypothesis that productivity has not decreased for these populations when comparing base to recent time periods” *Id.* at 132. Finally, NOAA summarizes the results of “[m]ore recent aggregate dam counts and predictions from factors influencing earlier ages of some cohorts” *Id.* at 133. NOAA does not draw any specific conclusions from these aggregate dam counts. *Id.* Earlier in the 2014 BiOp, NOAA also states that “[n]o changes resulting from RPA implementation are expected to be reflected in available BiOp indicator metrics.” *Id.* at 68.

III. THE UPDATED ANALYSIS OF THE EFFECTS OF THE RPA

17. As noted above, NOAA sets out in Chapter 3 of the 2014 BiOp its updated analysis of RPA implementation and its effects. It summarizes the conclusion of this review as follows:

As described in sections 3.1 through 3.9 and summarized above, the effects of the RPA action are anticipated to be within expectations of the 2008 BiOp. In reaching this determination, NOAA Fisheries considered apparent reductions in juvenile system survival and adult survival through the hydropower system, but determined that these factors remain within the BiOp’s expectations for the reasons described above. Additionally, survival is expected to improve to match 2008 BiOp expectations for all interior Columbia species and populations as a result of the modification to RPA action 46, which requires a reduction in the number of cormorants on East Sand Island, and survival is expected to be above expectations for specific species and populations as a result of tributary habitat improvement actions, hatchery improvements, and tern management in the upper Columbia area.

2014 BiOp at 455. In my prior declarations filed in support of NWF’s summary judgment motion against the 2008 BiOp, I addressed specific aspects of NOAA’s evaluation of the effects of the original 2008 BiOp RPA. These included the predicted effects of estuary habitat restoration actions, *see* Olney 2008 SJ Dec. at ¶¶ 61-74, measures to address avian predation (both terns and cormorants), *see id.* at ¶¶ 75-80, and the proposed kelt reconditioning program

for some steelhead populations, *id.* at ¶¶ 86-92. I will explain below whether and how the 2014 BiOp addresses each of these issues.

A. Estuary Habitat Actions

18. In my two prior declarations addressing the 2008 BiOp, I discussed a number of aspects of NOAA's assessment of the survival benefits it predicted would occur from habitat actions in the Columbia River estuary that are part of the 2008 RPA. Olney 2008 SJ Dec. at ¶¶ 61-74; Olney 2008 SJ Reply Dec. at ¶¶ 37-57. In these paragraphs, I described the survival benefits NOAA said estuary habitat actions would provide, a 6% increase for Snake River steelhead and Snake River spring/summer Chinook (called "stream-type" fish), and a 9% increase for Snake River fall Chinook (called "ocean-type fish"), with comparable increases for other ocean- and stream-type species in the Columbia basin. I also discussed aspects of the tools NOAA relied on to make these predictions. In my reply declaration, I addressed a number of statements from NOAA that appeared to misapprehend my statements and further explained the points I had raised. Olney 2008 SJ Reply at ¶¶ 38-57. Finally, I described some features of the estuary habitat projects that were relevant to the ability of the projects to provide the survival increases predicted in the RPA and the 2008 BiOp. *Id.* at ¶¶ 48-57. In the paragraphs that follow, I focus primarily on the estuary habitat actions that have occurred so far and NOAA's discussion in the 2014 BiOp of the factors it considered in evaluating whether estuary habitat actions in the revised 2014 RPA would provide the survival benefits predicted for them in the 2008 BiOp.

19. NOAA discusses the RPA's estuary habitat actions, RPA actions 36 through 38, on pages 319 through 344 of the 2014 BiOp. In this discussion, NOAA confirms that:

The particular 9% and 6% relative survival improvement performance standards [] for this program were set in the 2008 BiOp based on estimates of survival increases reasonably achievable through implementation of the Columbia River estuary management actions described in the Columbia River Estuary ESA

Recovery Plan Module for Salmon and Steelhead (NMFS 2011h, hereinafter Estuary Module). These figures, 9% relative survival increase for ocean-type fish and 6% for stream-type fish, were factored into the [2008] BiOp's quantitative analysis [Snake River and Upper Columbia River salmon and steelhead] as well as into the qualitative analysis for other affected listed salmonids, demonstrating how implementation of the RPA . . . would likely avoid[] jeopardy . . . and adverse[] modification of critical habitat.

2014 BiOp at 319-320 (footnote omitted). In my 2008 summary judgment declaration, I offered a number of observations about the action agencies' and NOAA's employment of the Estuary Module to predict survival improvements from estuary actions. Olney 2008 SJ Dec. at ¶¶ 62-74. Since NOAA continues to rely on the specific prediction of survival benefits from estuary actions developed in the 2008 BiOp, my observations about the role of the Estuary Module in developing these predictions remain relevant.

20. My observations included noting that the Module offered only a target level of survival improvement for salmon and steelhead from all types of action in the estuary of up to 20 percent. The authors of the Module state that this 20 percent total figure was for "planning purposes only," and not an actual prediction of the level of survival improvement that could be achieved from estuary actions, hence they described it as a "target." They also noted that their 20% target level of the total potential survival improvement that could be achieved in the estuary was based on a number of other critical assumptions, including the assumption that all 23 elements of the Module, in addition to estuary habitat restoration which covers only a subset of the 23 elements, were implemented to a reasonable degree. Olney 2008 SJ Dec. at ¶¶ 62-63. I also described an Independent Scientific Advisory Board ("ISAB") review of the Estuary Module that noted the Module was of limited scientific value and that its assumptions about survival improvements in the estuary were questionable. *Id.* at ¶ 64.

21. I then explained how a consultant for the action agencies developed the specific 9% and 6% survival improvement predictions for the estuary habitat restoration program

described in RPA actions 36-38 using the Estuary Module, *id.* at ¶ 65, and how the consultant's approach to developing these predictions omitted and/or did not address a number of factors that would be relevant to assessing the use of the Estuary Module to make specific survival predictions, *id.* at ¶¶ 66, 67-69 (e.g., relying on actions that protect existing estuary habitat to provide a portion of the predicted survival improvement when protecting existing habitat may prevent degradation but does not increase available habitat), 70 (not accounting for all of the Module assumptions in the survival predictions), and 71-74 (not actually following the methods described in the consultant's report or addressing the gap between the Module's assumptions about funding needs and the action agencies' planned funding at that time). I subsequently addressed and further explained these and similar points in my summary judgment reply declaration regarding the 2008 BiOp. Olney 2008 SJ Reply at ¶¶ 37-57.

22. While, as noted above, NOAA and the action agencies continue to rely on the 9% and 6% survival increases developed for the 2008 BiOp and the analysis that produced these predictions, they have changed the methodology they employ for survival benefit scoring for specific estuary habitat actions (essentially the method for predicting the amount of survival improvement a particular estuary habitat restoration action is expected to provide). NOAA describes this new approach in the 2014 BiOp. *See* 2014 BiOp at 325–328. The new approach, developed by a new group called the Expert Regional Technical Group (“ERTG”), consists of a process for calculating the number of “survival benefit units” or SBUs a particular estuary habitat restoration action is predicted to provide. As NOAA explains, as part of the ERTG scoring process, each percentage point of the 9% and 6% survival improvement requirement under the RPA was converted into 5 SBUs so that the total SBUs needed to implement the estuary RPAs for ocean-type salmonids is 45 and for stream-type is 30. 2014 BiOp at 326. As I explain in more detail in the following paragraph, even for completed projects in the estuary, the

survival benefits the ERTG process calculates remain predictions because the action agencies and NOAA cannot determine whether a specific estuary habitat action that has been completed actually has produced a survival improvement.

23. The ISAB has reviewed this new ERTG scoring process for estuary habitat actions. Their conclusions confirm many of the points I describe below. The ISAB's major conclusions are:

1. *Are the ERTG Scoring Criteria used to assign survival benefits for habitat restoration based on sound science?*

The ERTG Scoring Criteria are partially based on sound science. The Scoring Criteria were developed by the highly qualified ERTG team, which has considerable experience with estuarine and salmonid ecology. . . . The results and conclusions based on the ERTG Scoring Criteria are only partially supported by available scientific information. The Criteria have not been applied to comprehensive management elsewhere and are based largely on professional opinion. Thus, the ERTG's findings should be viewed as informed hypotheses that require research, monitoring, and evaluation to verify results and conclusions.

2. *Do the ERTG Scoring Criteria have the ability to differentiate and/or prioritize those projects that will succeed in increasing the survival of salmonids through their residence and migration in the Columbia River estuary?*

The ERTG Scoring Criteria are being used by Action Agencies (BPA/Corps) to differentiate and/or prioritize habitat restoration projects in the Columbia River estuary. However, the ability of projects to actually succeed in increasing the survival of salmon through their residence and migration in the Columbia River estuary cannot be determined from the Scoring Criteria. The ERTG Scoring Criteria can differentiate and/or prioritize the potential effectiveness of a project to increase survival of salmonids, assuming the accuracy of the score is reasonable. The statistical accuracy and precision of scoring of restoration projects are not estimated and are probably low in terms of the actual survival benefit expected from a specific project, but the accuracy and precision are likely greater for comparing the relative benefits of one project ranked against another. . . . [T]heir ability to differentiate and/or prioritize is only as good as the science behind SBUs in the primary planning document (Estuary Module). The main disadvantage of the scoring process is its subjectivity and variability of assigned scores, especially if there is a change in ERTG personnel. Whether or not the selected projects will actually succeed in increasing the estuarine survival of salmonids will remain uncertain until quantitative estimates of improvements in estuarine survival of salmonids become available.

3. *Do the processes identified in the ERTG Scoring Criteria reflect a landscape approach to restoring estuarine habitat through landscape ecology, resilience, and adaptive capacity?*

The processes identified in the ERTG Scoring Criteria reflect a landscape approach to restoring estuarine habitat through landscape ecology, resilience, and adaptive capacity, but only in a limited way. Ecological processes acting at the landscape scale, such as connectivity of habitats along salmonid migratory pathways, are recognized by the ERTG when subjectively scoring individual projects rather than by explicit criteria that guide scoring. Feedback processes due to connections among habitats are particularly important to resilience, but they are not explicitly quantified by the ERTG Scoring Criteria. Major socioeconomic processes such as salmon harvest, hatchery salmon production, hydrosystem operation, and urbanization also affect the diversity of salmon populations and habitats, and hence resilience, but do not seem to be considered in the scoring process. At present, the ERTG is operating under a high level of scientific uncertainty to qualitatively evaluate the identified processes. Quantitative estimates of processes are needed to develop adaptive capacity. Indeed, the limited purpose and scope of the ERTG Scoring Criteria and Terms of Reference for the ERTG do not promote a comprehensive landscape approach.

4. *Are there systematic and repeatable methods for quantitatively assessing the net changes in the Columbia estuary ecosystem that would produce data and analysis to validate the ERTG's survival benefit estimates?*

The review materials provided to the ISAB did not include systematic and repeatable methods for quantitatively assessing the net changes in the Columbia estuary ecosystem that would produce data and analysis to validate the ERTG's Survival Benefit estimates. Previous ISAB advice from the CEERP review . . . is still relevant: "A highly focused RME approach that estimates stock-specific survival rates in all major habitat types in the estuary and identifies habitats/locations where there are survival bottlenecks for species and stocks that migrate through Federal Columbia River Power System (FCRPS) is needed. Once these estuary bottlenecks are identified, it will be much easier to determine the most cost-effective approaches to habitat restoration that will be of benefit to Columbia River fish and wildlife."

5. *Are there other data available to complement the ERTG's approach or additional analysis that would make better use of available information to prioritize habitat restoration?*

The information from ERTG reports, meetings, and Action Agency documents specific to the ISAB's review suggests that other data are available to complement the ERTG's approach and additional analyses could make better use of available information to prioritize habitat restoration. . . .

Review of the Expert Regional Technical Group (ERTG) Process for Columbia River Estuary

Habitat Restoration at 2-3 (Feb. 12, 2014) (hereinafter “ISAB 2014-1”) (2014 Corps AR at 3671).³

24. The 2014 BiOp reports that the ERTG scoring process the ISAB addressed in its review quoted above was used to score all of the estuary projects completed, or expected to be completed, from 2007 through 2013, although it appears from the record that more than one third of these projects were actually scored by the action agencies or were scored (possibly using a different method) in the original action agency biological assessment. *See* 2014 NOAA AR at 271375 (estuary action spreadsheet dated May 21, 2013) (“May Spreadsheet”). Based on the reported scores for these projects, all of the projects completed from 2007 through 2012 provided less than 4 of the required 45 SBUs for ocean-type fish and less than 2 of the required 30 SBUs for stream-type fish. 2014 BiOp at 332-333 (Table 3.2-2). By adding projects the action agencies expected to complete through 2013, *id.* at 330, this total increased to 8.2 SBUs and 3.4 SBUs for ocean and stream-type fish, respectively, *id.* at 333 (Table 3.2-2). This is less than one-fifth of the survival improvement the RPA requires for ocean-type fish and just over one-tenth of the improvement for stream-type fish. As NOAA acknowledges, “this means that the program still must achieve the bulk of the SBUs . . . needed to satisfy the estuary performance standard.” *Id.* at 331.

25. The estuary habitat work in the RPA has consistently been behind in implementation and so in the survival improvements it is predicted to provide. At each step since the 2008 BiOp, the action agencies have said that the estuary habitat program is “maturing,” or becoming better organized, or ramping up, and will soon catch up with level of survival improvements it is required to provide. *See, e.g.*, 2009 Annual ESA Project Report:

³ Citations to the administrative records for the 2014 BiOp are in the form “2014 [agency] AR at [document or page number]” where the AR number is the document or page number in the AR index. If necessary for clarity, the citation includes the specific Bates stamp page number as well.

Section 2, Summary Table: Actions and Accomplishments for 2009 at 40 (2014 Corps AR 27 at 4551) (“Some projects scheduled for completion in 2007-2009 were delayed or proved infeasible. The Action Agencies are constructing projects in the 2010-2013 implementation period to replace the survival benefits those projects would have provided”); FCRPS 2010-2013 Implementation Plan at 6 (June 2010) (2014 Corps AR 29 at 5133) (“Estuary actions are behind schedule, but a catch-up plan has been formulated, with many new estuary projects under development for completion in 2010-2013”); 2014-2018 Implementation Plan at 61-62 (2014 Corps AR 9 at 1347) (“During 2007-2009 implementation period, some projects scheduled for completion were delayed and carried forward to the 2010-2018 period. The benefits associated with those projects are included in the 2010-2013 [Comprehensive Analysis, section 3, 2014 Corps AR 12 at 2466-2480], and the 2014-2018 [Implementation Plan at Appendix A, 2014 Corps AR 9 at 1468-1510] implementation cycles. During the 2007-2009 period some projects also proved infeasible. The Action Agencies are implementing additional projects through 2018 to provide survival benefits equivalent to those of the infeasible projects. These additional projects are being selected and implemented in accordance with RPA Action 37”). As with these prior statements, the 2014 BiOp also says that the estuary habitat program has become better organized and more effective and will soon catch up and provide the predicted survival improvements. 2014 BiOp at 331 (discussion and NOAA conclusion that the action agencies “are likely to make up this sizable difference”).

26. As the 2014 BiOp explains, the action agencies expect to make up the large shortfall in predicted SBUs from estuary habitat actions by relying on a few very large habitat projects. 2014 BiOp at 334-35 (Table 3.2-3) (listing many individual projects but estimating only SBU totals for projects initiated in 2012 that are to be completed by 2018 and separate totals for projects to be initiated in 2013 and beyond and completed by 2018). The 2014-2018

Implementation Plan (Jan. 10, 2014), (2014 Corps AR at 9), Appendix A lists the individual projects for 2014 and beyond with the SBU estimates and identifies whether they were given preliminary scores by the action agencies, preliminary scores by the ERTG, or final scores by the ERTG. The ERTG provided preliminary scores for four large dike breach projects. The ERTG provided a final score for only one small project: Oaks Bottom Tidal Reconnection (0.16 and 0.08 SBU's for ocean and stream-type fish, respectively). One other large dike breach project and the remainder of the projects were given preliminary scores by the action agencies. NOAA says these groups of projects combined are expected to provide 74.6 SBUs for ocean-type fish and 26.6 SBUs for stream-type fish. *Id.* The SBUs for these future projects, when added to the much smaller number of SBUs predicted to be produced by all estuary habitat actions that have been implemented or are expected to be implemented from 2007 through 2013 (8.2 SBUs for ocean-type fish and 3.4 SBUs for stream-type fish), produce the total number of SBUs on which NOAA relies. 2014 BiOp at 336 (last paragraph).

27. In the 2014 draft BiOp, NOAA discusses estuary habitat projects it expects will be implemented between 2014 and 2018. *See* 2014 Draft BiOp at 317-318. There it explains that some of these projects are still in the feasibility phase and none are in construction-ready status hence they were given only preliminary scores by the ERTG. By summing the scores from Appendix A of the action agencies 2014-2018 Implementation Plan, it appears that the ERTG preliminary scores, based on project concepts, for the four large projects would provide 41.82 ocean SBUs and 14.8 stream-type SBUs. One of these large projects, Large Dike Breach-Reach E alone has a preliminary ERTG score of 31.0 ocean-type SBUs and 11.08 stream-type SBUs. These large projects all involve levee breaching, 2014 BiOp at 337-338, which requires significant investment. The 2014 BiOp does not discuss in any detail the feasibility of these projects or any potential funding issues but the 2014 BiOp does say that if any of these projects

prove infeasible, the action agencies “will implement others that collectively contribute an equivalent number of SBUs.” *Id.* at 336. NOAA does not actually describe any potential substitute projects or explain where they would occur or its basis for concluding that they are available and can be implemented.

28. As NOAA explained more clearly in the draft BiOp, because the estuary projects given preliminary scores by the ERTG did not provide the SBUs for stream-type fish required by the RPA, the action agencies also relied on additional projects to provide another 24.4 SBUs for ocean- and 8.06 SBUs for stream-type fish. These projects had not yet been initiated and were not scored even preliminarily by the ERTG. 2014 Draft BiOp at 316-317. These projects, with slightly different SBU numbers, apparently are listed in the final 2014 BiOp on page 335 but the feasibility and individual scores for these projects are not described or listed. The 2014-2018 Implementation Plan does not make a separate designation for these projects.

29. NOAA and the action agencies expect to achieve totals of 74.6 ocean- and 24.6 stream-type SBUs from the two groups of estuary habitat actions discussed above between 2014-2018. When added to the much smaller number of SBUs estimated for estuary projects from 2007-2013, NOAA expects to achieve 82.7 and 30.0 SBUs in total for ocean- and stream-type fish, respectively, which exceeds the 45 SBUs (9% relative survival improvement for ocean-type fish) and just meets the 30 SBUs (6% relative survival improvement for stream-type fish). The projects whose final feasibility have not been assessed by the ERTG make up about 95% of the total ocean- and total stream-type SBU’s for the entire RPA from 2007 through 2018. Preliminary ERTG scores given for the four large dike breach projects make up about 51% of the total ocean and 49% of the total stream-type SBUs.

30. Although not discussed in the 2014 BiOp, apparently the original 2008 BiOp requirements of a 9% and 6% survival improvement assumed that estuary habitat projects would

deliver 0.66 stream SBUs for every ocean SBU. The 2014-2018 Implementation Plan at pages 61-62, however, states that, “[t]he original targets based on the [2008] BiOp’s relative percent survival improvement targets (9% for ocean and 6% for stream type) assume that habitat projects will deliver roughly 0.66 stream SBUs for every ocean SBU (30/45). The actual results from the ERTG scores to date correspond to 0.33 stream SBUs for every ocean SBU, roughly half the ratio found in the BiOp targets.” It is more difficult to achieve benefits for stream-type fish from estuary habitat projects because they spend so little time in the estuary in their migration to the ocean. In order to meet the survival improvement of 6% or 30 SBUs for stream-type fish, the action agencies must achieve about 90 SBUs for ocean-type fish rather than the 45 SBUs required by the 2008 RPA. This change in the ratio of SBUs between ocean- and stream-type fish accounts for the sharp increase in predicted SBUs and survival improvements for ocean-type fish in the 2014 BiOp. Thus NOAA now says that estuary habitat actions will increase the survival of ocean-type fish by nearly 17% rather than 9%, while just meeting the required 6% survival improvement for stream-type fish.

31. NOAA and the action agencies have now eliminated from the RPA action 38—the Piling and Piling Dike Removal Program. 2014 BiOp at 341. This action was depicted in the 2008 BiOp as a key component of the estuary work. The project was intended to help increase connectivity and reduce avian predation by removing perches for double-crested cormorants. According to the 2008 BiOp, Comprehensive Analysis, Appendix D, the piling and piling dike removal program would provide about 1.2% of the 9% survival benefit target for ocean-type and 1.2% of the 6% target for stream-type fish, or 15% and 20% of the overall survival benefit from estuary work, respectively. These numbers can also be derived from NOAA’s Columbia River Estuary ESA Recovery Plan Module for Salmon and Steelhead at 5-39 (Table 5-5) (Jan. 2011) (2014 NOAA AR B296 at 31691). Removing this program creates a need to make up for these

predicted estuary survival benefits and NOAA says as much: “[a]ll SBUs attributed to this program in [the analysis supporting the 2008 BiOp] will now be acquired by implementing other projects under RPA action 37 [estuary habitat projects].” 2014 BiOp at 341.

32. NOAA does not explain how this can be accomplished consistent with the framework of the Estuary Model, the ERTG scoring process, and the ISAB reviews. As these documents explain, the action agencies cannot create or take credit for more SBUs for an Estuary Module component action than the Module’s structure allows. They cannot, for example, take assumed potential SBUs from the Module’s flow management element/action and shift them to estuary habitat work because it is inconsistent with the assumptions of the Module. The same is true for the Piling and Piling Dike Removal action which is a separate Module action from habitat restoration. NOAA’s response to comments on this issue that accompany the 2014 BiOp does not address this point when it discusses the issue of ERTG “weighting.” *See* Response to Comments (“RTC”) at 44 (comment/response D-8) (2014 NOAA AR 288216 at 288259). The weighting using the fish density estimates discussed in this response to comments only affects how the potential SBU’s *within* an Estuary Module action are allocated among projects and does not change the number of SBUs possible for that action element as noted by the ERTG. For example, in the ERTG’s 2011 Feedback on Inputs to the Calculator to Assign Survival Benefit Units, the ERTG states with respect to the SBU calculator, “[w]eighting does not change the number of SBU possible. It only reallocates SBU among subactions.” ERTG 2011-01 at 4 (2014 NOAA AR B108 at 9152). They state the same in their Meeting Notes 2011, 2012, and 2013. ERTG 2013-03 at 6 (2014 Corps AR 42 at 5705).

33. The ISAB makes this same point in their review of the ERTG SBU scoring methods: the process cannot assign more SBUs for a restoration action than the Module estimates:

The 2011 Estuary Module developed by NOAA constrains the quantity of SBU's that the ERTG can assign to restoration projects. The Module lists 22 habitat restoration actions and associated subbasin goals, and provides each restoration action with a set number of SBU's. The ERTG cannot assign more SBU's for a restoration action than the Module delineates.

ISAB 2014-1 at 1 (discussing the Estuary Module). As in other aspects of the 2014 BiOp, NOAA states that “[i]f any of [the estuary habitat restoration actions] prove infeasible, the Action Agencies will ensure that the total sum of projects implemented, including any replacement projects, will collectively reach the BiOp’s estuary habitat survival benefits performance standards” 2014 BiOp at 338.

34. NOAA’s statement about replacing the survival benefits from the piling and piling dike removal program also does not address the Estuary Module’s assumption of a total 20% overall survival improvement target for the estuary if each of the 23 management actions in the Module are implemented to “a reasonable degree” (22 actions to improve juvenile survival and one to improve adult survival). The piling and piling dike removal program was one such action and was a different element from the habitat restoration elements. Not addressing factors that limit survival in some areas could reduce or negate survival benefits from improvements in estuary habitat in other areas. As NOAA explained in the 2008 BiOp, the 9% and 6% survival improvement requirements for the RPA from estuary habitat actions were derived from the Estuary Module and its underlying assumptions. The Module assumed a total potential survival improvement of 20% as a target for salmonids passing through the estuary if all 22 of the actions for juvenile salmonids and the one action for adult salmonids were implemented to a reasonable extent. In addition to the estuary habitat improvement actions, these actions include improvements in flow regulation, reducing entrapment of sediments in reservoirs, reducing impacts from dredging, fertilizer and pesticides upstream, limiting industrial, commercial and public sources of pollution, reducing the effects of ship wakes and reservoir related water

temperature changes, and removing piling and pile dike structures. NOAA does not address whether these actions have been implemented to a reasonable extent or whether there are negative effects such as adverse flow effects, increased ship traffic, or increased agricultural runoff as a result of some of the elements not being implemented at all or to a reasonable degree, or whether such shortcomings (if any) could affect the survival improvements from estuary habitat actions, although NOAA has concluded that the piling and piling dike removal program will not be implemented at all.

35. NOAA also says in the 2014 BiOp that it “continues to assume that these habitat improvement projects are mitigating for the negative effects of RPA flow management operations on estuarine habitat used by these species for rearing and recovery.” 2014 BiOp at 475. In making this assumption (that estuary habitat improvement projects can mitigate for negative effects of RPA flow management), NOAA does not address several relevant factors. First, as discussed above, the Estuary Module assumed that all 22 actions, including flow improvements, would be implemented to a reasonable extent in order to achieve the 20% potential survival improvement target for the estuary, which includes the potential SBUs from the subset of estuary habitat actions. Second, the ISAB states, “the ERTG scoring criteria do not include key processes such as operations of spill and water releases at the dams, precipitation and timing of volume of flows that likely affect estuarine conditions.” ISAB 2014-1 at 14. Finally, the ERTG also has identified several key uncertainties including whether historical functions of floodplains can be restored because of modern flow regulation and invasion by non-native warm water fishes, uncertainty about juvenile salmon use of riparian habitats depending on water level and vegetation type, uncertainty about how rearing capacity varies seasonally with changes in temperature and flow, and uncertainty about how the “peaking” cycle at the dams influences rearing opportunities and capacities at upper estuary restoration sites. ERTG 2012-02 at 4-7

(2014 Corps AR 39 at 5628-5631). These latter points indicate that allowing the negative effects of RPA flow operations to continue could reduce the potential survival benefits from estuary habitat actions and that these negative effects of flow are not mitigated by habitat actions.

36. In my summary judgment reply declaration regarding the 2008 BiOp, I explained that one assumption of the Estuary Module was that restoration actions would be balanced and distributed throughout all segments of the lower river in order to ensure their connectivity, rather than concentrated in one or a few segments. Olney 2008 SJ Reply Dec. at ¶ 51 (summarizing a memorandum from a NOAA scientist). In their May Spreadsheet and 2014-2018 Implementation Plan, the action agencies list the completed and proposed estuary habitat projects located in the eight reaches of the estuary from A to H. Only a few small projects have been completed and none are proposed for reaches D and H for implementation in 2014-2018. Three of the four large projects that NOAA discusses in the 2014 BiOp, and relies on to produce the SBUs still needed to improve estuary survival, are located in the upper three of the six reaches that do have proposed estuary projects. These three projects alone contribute nearly half of the total ocean- and stream-type SBUs for all of the 2007-2018 estuary habitat improvement projects. One of these projects, Large Dike Breach E, contributes about one-third of the total expected benefits. NOAA says the estuary habitat projects, including these, are adequately distributed in the estuary, 2014 BiOp at 338, but it does not provide any supporting analysis. The completed and predicted SBUs for estuary habitat actions, according to the action agency's May Spreadsheet and the 2014-2018 Implementation Plan, are substantially concentrated in a few projects in three reaches from the mid- to upper estuary. About 66% of all of the SBUs listed in the May Spreadsheet and 2014-2018 Implementation Plan are concentrated in 3 of the 8 reaches in the mid- to upper estuary, less than 0.5% of the SBUs are located in 2 of the 8 reaches, and the remaining 34% are located in the three lower reaches of the estuary. Of course, this

analysis assumes all of the projects listed in the 2014-2018 Implementation Plan, most of which are only conceptual, are actually implemented. It also assumes that final SBU scores by the ERTG do not change the scores developed by the action agencies for a number of projects that only the action agencies have scored.

37. NOAA also does not address the relationship between the Estuary Module's constraint of a total survival improvement of 20% in the estuary—as a target, not a prediction—assuming all 22 action categories from the Module are implemented to a reasonable degree (on the one hand), and its conclusion in the 2014 BiOp that the action agencies can achieve almost a 17% increase in survival for ocean-type Chinook just from estuary habitat projects implemented between 2007-2018 which is nearly double the 9% potential survival improvement identified in the 2008 BiOp and over 80% of the total survival improvement assumed to be possible under the Estuary Recovery Module (on the other hand). Likewise, NOAA does not discuss the relationship between the Estuary Module assumptions and its conclusion that almost half the required stream-type survival improvements can be achieved from three habitat projects in the mid- to upper estuary.

B. Avian Predation

38. In my 2008 summary judgment declarations, I described a number of issues related to avian predation because the RPA in the 2008 BiOp included several measures to increase salmon survival (and thereby help avoid jeopardy) by reducing avian predation of juvenile salmon during their migration to the ocean. Olney 2008 SJ Dec. at ¶¶ 75-80; Olney 2008 SJ Reply Dec. at ¶¶ 16-28. These issues included the RPA actions to address predation by Caspian terns, the treatment of predation by double-crested cormorants in NOAA's assessment of the effects of the RPA, and NOAA's treatment of the issue of compensatory mortality. I again address these issues below as they have evolved since the 2008 BiOp and as they are addressed

in the 2014 BiOp.

1. Caspian terns

39. In the 2008 BiOp, NOAA concluded that RPA action 45 to reduce predation on juvenile salmon by Caspian terns in the estuary would provide a 3.4% survival increase for all listed steelhead populations, a 2% survival increase for all listed spring/summer Chinook populations, and a .8% survival increase for listed fall Chinook. *See, e.g.*, 2008 BiOp at 8.3-54 (Table 8.3.5-1) (indicating a 2% survival improvement for all Snake River spring/summer Chinook populations from measures to address “bird predation”). RPA action 45 called for reducing Caspian tern nesting habitat on East Sand Island in the Columbia River estuary to less than one-third of its pre-2008 BiOp size and simultaneously creating alternative nesting sites away from the River in order to reduce the number of nesting pairs of terns by more than half (from about 9,000 pairs before implementing RPA 45 to 3,500 to 4,000 pairs following implementation). 2014 BiOp at 411. The 2008 BiOp assumed that reducing the area of the East Sand Island colony would reduce the number of nesting pairs of terns, which would then reduce predation by terns and provide the survival improvements noted above and attributed to this RPA action.

40. Since 2008, the action agencies have reduced the area of the East Sand Island tern colony from about 6 acres in 2008 to 1.5 acres in 2012, 2014 BiOp at 411, which is at least the amount of reduction the 2008 BiOp assumed would be required to shrink the number of nesting pairs to the desired level. 2013 Comprehensive Evaluation at 83 (2014 Corps AR 12 at 1786). The agencies also have created 8.3 acres of alternative nesting habitat at nine locations elsewhere but “no coastal sites have been developed [and] [p]redation [on terns at alternative sites], lack of sufficient water, and limited food resources have plagued tern nesting success at several of these interior sites to the degree that a significant portion of the alternative nesting habitat has not been

available for nesting terns in any single year.” 2014 BiOp at 411.

41. Even though the area of the East Sand Island tern colony has been reduced to the extent planned by the action agencies, as the 2014 BiOp also reports, the number of nesting pairs of terns has not been reduced to the 3,500 to 4,000 pair level but remains at 6,000 to 6,500 pairs, at best half the total reduction in tern pairs that NOAA thought would be necessary to achieve the predicted survival improvements for juvenile salmon. Apparently the density of tern nesting has increased to offset the loss of total nesting habitat. NOAA also notes that action agency efforts to establish tern colonies elsewhere have been considerably less successful than anticipated. *Id.* NOAA does not indicate whether or the extent to which it believes the terns now nesting at other sites are from the East Sand Island colony or represent an expansion of the tern population. NOAA also acknowledges that it will be difficult to reduce the area of the East Sand Island nesting site further. *Id.* at 411-412. For example, the Corps is pursuing construction of an additional island in San Francisco Bay. That island, if construction becomes possible, will allow a further reduction on East Sand Island to 1.0 acres, the minimum area considered in the management plan. 2013 Comprehensive Evaluation at 83 (2014 Corps AR 12 at 1786).

42. NOAA says in the 2014 BiOp the reduction in tern numbers by 2,500 to 3,000 pairs at East Sand Island that has been achieved so far through reducing nesting habitat there, “has not translated to a similar reduction in salmonid smolt consumption [by Caspian terns] which remains similar to pre-implementation levels.” 2014 BiOp at 411. In other words, even though nesting habitat has been reduced as planned, and even though the number of nesting pairs of terns has declined to some extent, tern predation on juvenile salmon has not declined and so the salmon and steelhead survival improvements predicted for this RPA action have not actually accrued.

43. With respect to the survival improvements anticipated from reducing tern

predation in the estuary, NOAA concludes, “[i]t remains likely that suitable [alternative nesting] habitat will be found, allowing for full implementation of the management plan to occur, and for the reduction of Caspian terns (and associated losses of steelhead and Chinook smolts) to levels anticipated in the 2008 BiOp.” 2014 BiOp at 413. In this regard, NOAA notes that “additional suitable habitat is being sought,” that “only about one acre of suitable habitat is needed,” and that there are “currently likely candidate locations.” *Id.* NOAA does not explain why it expects the acquisition of one additional acre of alternative nesting habitat somewhere in the West, and the corresponding small additional reduction in the area of nesting habitat at East Sand Island it may then undertake, to produce the remainder of the reduction in terns at East Sand Island that has not yet occurred—as well as, more importantly, the corresponding reduction in smolt predation from 2008 BiOp levels that also has not yet begun to occur.

2. *Cormorants*

44. In my 2008 summary judgment declarations I explained that the analysis in the 2008 BiOp did not address the rapid growth of cormorant colonies in the estuary and the large increase in predation on juvenile salmonids by these birds. Olney 2008 SJ Dec. at ¶¶ 76-80; Olney 2008 SJ Reply Dec. at ¶¶ 22-28. In Appendix E to the 2014 BiOp (the “Cormorant Appendix”), NOAA states:

The primary goal for addressing double-crested cormorant (DCCO) smolt consumption in the 2013 [sic] BiOp is to determine the smolt survival “gap” that has resulted from the dramatic increase in cormorant population and smolt consumption between the base [1981 to 2000] and current [2001-2006] years that was not captured in the 2008 BiOp analysis.

2014 BiOp, App. E at E-3. In the Cormorant Appendix, NOAA calculates that this survival gap is 3.6% for steelhead populations, 1.1% for yearling Chinook, and less than 1% for sockeye. *Id.* at E-5 to E-6. In other words, steelhead survival during the current period was 3.6% lower and spring/summer Chinook survival was 1.1% lower, as compared to the Base Period, than the

analysis in the 2008 BiOp assumed. In order to address this survival “gap,” NOAA proposes to reduce cormorant predation from today’s level by an amount sufficient to return cormorant predation levels to those of the Base Period, thereby bringing current period survival for salmon and steelhead in line with the assumptions in the 2008 BiOp analysis. The cormorant action in the RPA is thus not designed to increase salmon survival as compared to the Base Period but to address an overlooked source of increased mortality that arose after the Base Period and thereby restore cormorant-caused mortality to the Base Period level.

45. NOAA’s proposal for addressing the survival gap described in the Cormorant Appendix is the removal of more than 50% of the breeding pairs of cormorants from the largest colony in the estuary (and the largest colony in the western United States), also on East Sand Island. 2014 BiOp at 410 (“[t]he FCRPS action agencies will develop a cormorant management plan . . . and implement warranted actions to reduce cormorant predation in the estuary to Base period levels”). This would require the elimination of between 6,500 and 7,000 pairs of cormorants. In fact, since the colony size continued to increase to almost 15,000 pairs in 2013, a higher number of cormorants presumably would need to be eliminated. Multiplying the number of pairs that need to be eliminated by two to produce the number of individual birds that would need to be removed is likely to under-estimate the magnitude of the removal effort because of re-pairing, juvenile maturation and other factors.

46. NOAA explains that “[t]he Corps is the lead agency on a draft EIS that will use NOAA Fisheries’ survival gap and colony per capita analysis to develop objectives for double-crested cormorant management on East Sand Island. . . . The range of alternatives will cover lethal methods (shooting of individual birds, egg collection/nest destruction, etc.) and non-lethal methods (hazing, habitat modification, etc.) to reduce double-crested cormorant predation impacts to juvenile salmonids in the estuary.” 2014 BiOp at 410. NOAA goes on to say that

“[m]anaging natural resource damage by cormorants and associated conflicts on a local scale has been successfully implemented in the U.S. A recent example of a successful cormorant-damage management action includes a 2005 implementation at Leech Lake, Minnesota [that] was considered a success in helping to curb declining populations of walleye and contribute to record 2008-2009 walleye harvest rates.” *Id.* at 411 (citing Schultz 2011 and 2012 which report on a program that involved pass-shooting of cormorants with shotguns as they returned to the nest island in order to remove 3,000 birds per year from a colony of about 2,500 pairs). Although NOAA acknowledges “that any similar management actions in the Columbia River basin will require that the Action Agencies first obtain the appropriate permits,” *id.*, it concludes that “[s]imilar double-crested cormorant management actions in other parts of the U.S. have recently been implemented in a timely manner and have proven successful,” *id.* at 412, apparently referring to the Leech Lake program.

47. NOAA did not refer to the Schulz et al. (2013) studies of the Leech Lake cormorant control program which reported that, “increases in walleye harvest reflected increasing walleye abundance . . . concurrent with cormorant control and Walleye fry stocking, indicating that the effects of cormorant management on the Walleye population and its fishery are thoroughly confounded with other management actions.” Schultz et al. 2013 at 1296 (copy attached as Exhibit A). While the 2013 Schultz study recognizes evidence that “suggests cormorant management has positively affected the [Walleye] fishery,” data for two other fish species “were not explained by cormorant predation pressure,” leading the study authors to state that while “[c]hanges in all Walleye population metrics were associated with changes in cormorant feeding pressure, . . . we suspect that Walleye fry stocking has confounded interpretation of Walleye abundance, recruitment and fishery statistics.” *Id.* Varying stocking densities of Walleye fry during 2005-2011, as well as the implementation of a 454-660 mm

protected slot limit and a bag limit reduction from six to four Walleye, also confounded interpretation of the Walleye population and fishery response to the concurrent efforts at cormorant control according to this 2013 study. *Id.* The authors note further that during the period of cormorant removal at Leech Lake, “it is no surprise that new colonies have established and expanded in northern Minnesota, some of which are less than 100 kilometers from the [Leech Lake] study site. It has been hypothesized that some of these new colonies may be the direct result of control efforts on Leech Lake, and public pressure is mounting for cormorant management to begin at these locations.” *Id.* at 1298. NOAA does not describe or discuss any of these findings from the Schultz et al. 2013 studies of the Leech Lake cormorant control program or address the differences in scale between the program at Leech Lake and the program that would be required at East Sand Island to reduce the cormorant colony there by 6,500 to 7,000 pairs or more.

3. *Compensatory Mortality*

48. In my 2008 summary judgment declarations, I described an ecological process called “compensatory mortality” and explained how it would affect the evaluation of the benefits to salmon and steelhead survival from reductions in avian predation by terns or cormorants. Olney 2008 SJ Dec. at ¶¶ 77-78; Olney 2008 SJ Reply Dec. at ¶¶ 16-21. NOAA identifies and discusses the concept of compensatory mortality in the 2008 BiOp at page 7-48, explaining that “[t]he projected benefits identified [for reducing Caspian tern predation] assume complete additivity (no compensatory mortality), i.e., every salmonid not consumed by terns survives all other sources of mortality.” NOAA then indicates it will apply “a hypothetical compensatory mortality of 50%” to its estimates of the survival improvements from reducing tern predation. This 50% factor would reduce by half the survival improvement from reduced tern predation for each of the listed species. Olney 2008 SJ Reply Dec. at ¶ 17 (citing calculation from the 2008

BiOp at page 8.3-26). As it turns out, NOAA did not apply this adjustment for compensatory mortality to its prediction of survival benefits from reducing tern predation because it concluded it was not “significant.” *Id.* at ¶ 16 (discussing response of Mr. Graves). As I explained in my reply declaration, NOAA has not explained what it considers a “significant” survival adjustment, either positive or negative, but it has included in its jeopardy analysis *positive* survival adjustments much smaller than the *negative* adjustment that would occur from using a 50% compensatory mortality assumption for the effects of reducing tern predation. *Id.* at ¶¶ 17-21.

49. In the 2014 BiOp and its Response to Comments on the draft 2014 BiOp, NOAA makes two observations about compensatory mortality, one related to terns and one related to cormorants. First, both the State of Idaho and NWF commented on the assumption in NOAA’s analysis that tern predation is not affected by compensatory mortality. RTC at 60 (2014 NOAA AR 288216) (comment and response G-4) (Idaho comment that “[a]ssuming there is no compensatory mortality . . . is contrary to the ecological principal [sic] of minimizing energy expenditures to capture prey” and referring to a study of compensatory mortality in avian predation in the Columbia basin); *id.* at 63 (comment by NWF and NOAA’s response G-11). NOAA’s response to both comments was either that the cited studies did not “offer a specific compensation level for predation by the estuary tern population,” RTC at 63 (2014 NOAA AR 288216), or that the studies were “in the mid-Columbia and Snake Rivers, not in the estuary,” *id.* at 60. The absence of a study identifying a specific level of compensatory mortality for avian predation in the estuary does not make the issue of compensatory mortality irrelevant. Not addressing the effects of a recognized ecological principle where NOAA has previously described information that would allow it to do so, and has said it would do so, just because no study provides a specific compensation level for predation by the estuary tern population, is actually a failure to consider a relevant factor where information is available to do so. For

example, in the 2008 BiOp at 7-48 NOAA says, “[s]ince current literature and empirical data do not identify more specific estimates or ranges, NOAA Fisheries assumes tern predation likely falls between being completely additive or completely compensatory. Consequently, in estimating the effect of reducing tern predation NOAA Fisheries assumed a hypothetical compensatory mortality of 50%.” As noted above, however, NOAA did not apply that adjustment in the 2008 BiOp, apparently because it did not find it “significant.” It also did not apply this adjustment in the 2014 BiOp and did not explain further its decision to omit this factor.

50. NOAA’s second comment regarding compensatory mortality explains that its analysis of cormorant predation compares two time periods during which compensatory mortality for cormorants was presumably the same. RTC at 60, 63 (2014 NOAA AR 288216). This may well be the case for comparing two periods of cormorant mortality, but it does not address the points discussed above regarding the effects of compensatory mortality on the survival improvements predicted from reduced Caspian tern predation.

C. Kelt Reconditioning

51. In my first summary judgment declaration regarding the 2008 BiOp, I addressed RPA action 33, the steelhead kelt reconditioning program. As I explained there, the jeopardy analysis in the 2008 BiOp relies on this action to increase the survival of each Snake River B-run steelhead population by 6%. Olney 2008 SJ Dec. at ¶¶ 86-92. I pointed out a number of factors relevant to this analysis that NOAA had not addressed. *Id.* In my summary judgment reply declaration I again addressed this issue and responded to comments by Mr. Graves of NOAA. Olney 2008 SJ Reply Dec. at ¶¶ 29-36. Except as noted in my reply declaration, my observations about NOAA’s analysis of the effects of the proposed kelt reconditioning program have not changed.

52. I summarize below a series of Independent Scientific Review Panel (ISRP) reviews of kelt reconditioning projects in the Columbia River basin. These reviews have consistently expressed skepticism about whether kelt reconditioning is a viable steelhead survival improvement and recovery strategy. *See, e.g.*, ISRP 2009-39 at 2 (Sept. 28, 2009) (reviewing the Yakama Nation’s Upper Columbia River Kelt Reconditioning Program - 2008-458-00) (*available at* <https://www.nwcouncil.org/fw/isrp/isrp2009-39/>). I also address a number of statements about kelt reconditioning from the 2014 BiOp.

53. The 2009 ISRP review of project 2008-458-00 cited in the preceding paragraph states that evidence kelt reconditioning is effective at improving the survival of steelhead populations remains to be demonstrated and the results of the Yakama Nation work to date are discouraging. ISRP 2009-39 at 2. The ISRP also indicated that, “Simply putting more adult steelhead on the spawning grounds does not ensure enhanced natural recruitment and, in fact, may do the opposite. Artificial reconditioning may alter maturity and spawning dates (as seen when smolts, parr, or sub-adults have been used for supplementation) thus adding little, or negatively, to recruitment.” ISRP 2009-39 at 3.

54. Likewise, the ISRP’s Retrospective Report 2011 from December of 2011 looked at all of the kelt work in the Columbia River basin to date. *See* ISRP 2011-25 (*available at* <https://www.nwcouncil.org/fw/isrp/isrp2011-25/>). In their conclusions and recommendations in this Report the ISRP states: “Kelt reconditioning (either transportation, short-term, or long-term) as a recovery tool as envisioned by the agencies is in an early stage of development. It remains to be seen whether reconditioning can contribute meaningfully as a recovery strategy. Efforts from transportation and short-term reconditioning have not yielded substantial gains compared to in-river migration. Long-term reconditioning has demonstrated some promise. An adequate comparison of reproductive performance between natural and reconditioned kelts has not been

accomplished. It remains uncertain whether nutrition and gametogenesis in reconditioned kelts is sufficient. In any case, it should be recognized that successful reconditioning—survival and subsequent reproduction—is a necessary, but not sufficient condition for kelt reconditioning to provide benefits for recovery.” ISRP 2011-25 at 28.⁴

55. The Northwest Power and Conservation Council (NPCC) has approved funding of the kelt reconditioning proposals the ISRP has reviewed as part of implementing the Columbia Basin Fish Accords, despite the concerns the ISRP has expressed. In reaching the recommendation to fund one of these projects (2008-458-00), the NPCC explained, in a January 13, 2010 letter from Tony Grover to William Maslen, Bonneville Power Administration, why it recommended proceeding with the project despite an ISRP recommendation that the proposal did not meet scientific review criteria, *see* ISRP 2009-39 at 2 (*available at* <https://www.nwcouncil.org/fw/isrp/isrp2009-39/>). The NPCC recommended that the project proceed conditioned on the understanding that the project would have a performance check in 2014. In its 2014 review (ISRP 2014-9) of project 2008-458-00 the ISRP gave it a rating of Meets Scientific Review (Qualified). They said the project has the potential to make important contributions to kelt reconditioning research if it can be modified to address several qualifications. To address these, they encouraged the project proponents to expand its future work objectives, “[o]therwise, there is a real risk that essential questions will remain unanswered

⁴ In their findings on page 27-28 of the Retrospective Report, the ISRP refers to long-term reconditioning survival for steelhead kelts ranging from 5% in the Deschutes River subbasin to 38% for fish from the Yakima River subbasin. This is presumably the basis for their statement that long-term reconditioning shows “some promise.” But as the ISRP quote above makes clear, survival rates for reconditioned kelt are only the first step towards increased reproduction from these kelt and increased steelhead population survival. As the ISRP explains, any subsequent increase in steelhead populations is contingent on much more than kelt survival because increased survival of reconditioned kelts must also lead to increased reproductive success from these kelts which must, in turn, lead to increased juvenile production, their survival, and increased adult returns. ISRP 2011-25 (*available at* <https://www.nwcouncil.org/fw/isrp/isrp2011-25/>).

and that the project will make a minimal contribution to answering the questions surrounding kelt reconditioning.” As they had noted in prior reports, the ISRP also stated a continuing concern, “Ultimately the efficacy of reconditioning and releasing kelts to spawn in nature will depend on the demographic and genetic effects the strategy has on targeted populations, MPG’s and ESU’s. At present it remains to be seen if reconditioning is a viable recovery strategy.” ISRP 2014-9 at 3 (*available at* <https://www.nwcouncil.org/fw/isrp/isrp2014-9/>).

56. The original Yakima River kelt reconditioning project became project 2007-401-00 in July 2007. Project 2007-401-00 includes kelt reconditioning studies in the Columbia and Snake Rivers, not including the upper Columbia River. The kelt studies in the upper Columbia River are included in project 2008-458-00 mentioned above. In the ISRP’s final assessment of this project (2007-401-00), *see* 2007-401-00-ISRP-20101015 (*available at* www.cbfish.org) (Columbia Basin Fish and Wildlife Program Projects and Priorities), the ISRP concluded that it did not meet scientific review criteria and recommended, “Before proceeding with additional kelt reconditioning feasibility and physiology research the Basin co-managers need to establish a well-defined kelt management master plan.” The ISRP stated in their first round review of this project that, “Kelt reconditioning is mushrooming into a very large effort with little quantitative justification for anticipated benefits to steelhead status. The potential research seems endless. It seems that some numerical and life history benefit and cost analysis should have been done by now.” Despite the ISRP’s concerns, this project was also approved by the NPCC conditioned on a performance check in 2014 (which I have not been able to locate).

57. In the 2014 BiOp, NOAA identified a number of problems the action agencies face in implementing a kelt reconditioning program and achieving the 6% steelhead survival improvement from the program. *See* 2014 BiOp at 383-387. For example, NOAA says reconditioning success rates by holding kelts for extended periods continue to be inconsistent,

ranging from 20% to 62% with a 10-year mean of 38% for kelts from the Yakima Basin. *Id.* at 385. One problem I identified in my 2008 declarations was that NOAA's estimate of the number of kelts potentially collected for reconditioning failed to address a number of relevant factors or explain why they did not need to be addressed. *See* Olney 2008 SJ Reply Dec. at ¶ 35. NOAA has now noted in the 2014 BiOp that only 5.6% of the kelts passing Lower Granite Dam entered the juvenile bypass system. 2014 BiOp at 385. This is substantially lower than the 33% assumed in the 2008 BiOp in the analysis of the number of kelts that could be collected for reconditioning and which was used to estimate the potential survival improvements that could be achieved from a reconditioning program. *Id.* NOAA describes a number of steps it expects to take over the next several years to increase collection of kelts, *id.* at 386, but again offers no estimates of the likely number of kelts collected through any of these measures, or any analyses to explain why these measures would allow the kelt reconditioning program to achieve the goal of increasing steelhead survival by 6%. These collection measures also only address the initial capture and reconditioning of kelts, not their subsequent spawning success or contribution to increased population productivity.

58. In my 2008 BiOp summary judgment reply declaration, I noted that Mr. Graves in his declaration indicated NOAA's assignment of benefits for kelt survival from in-river kelt migrants, which he placed at less than .1%, was not an important consideration in the overall analysis of survival benefits from kelt reconditioning. Olney 2008 SJ Reply Dec. at ¶ 32 (quoting Graves declaration). NOAA's 2008 BiOp analysis also suggests that a substantial number of female kelts would need to be reconditioned (not left to migrate in-river) in order to achieve the 6% benefit. 2008 BiOp at 3.5 30 to 8.5-31. The 2014 BiOp, however, says, "[i]ncreasing the survival of inriver migrating kelts . . . appears to have longterm potential for increasing the productivity of B-run SR steelhead populations" 2014 BiOp at 383. NOAA

then describes a number of projects to improve in-river survival for these fish. *Id.* at 383-384. The average return rate for kelts migrating in-river back to Lower Granite Dam is only 0.68%. Transport of kelts from Lower Granite Dam to below Bonneville Dam provided a relatively small benefit in comparison (1.17%). 2014 BiOp at 384. Even if the action agencies collected and transported all of the kelts downstream, the increase would amount to a very small number of fish. For example, if they collected and transported 1,000 fish, they would gain only 5 more fish from the transported group than the 7 fish that would have returned if all of the fish were left to migrate in-river. Because of these low return rates for both in-river and transported kelts, NOAA also says: “the Action Agencies proposed to prioritize strategies which yield a higher rate of reconditioned kelts, such as long-term reconditioning.” 2014 BiOp at 384. NOAA does not explain its basis for concluding that prioritizing these other strategies will consistently produce the kelt numbers, spawning success and other steps that must occur to achieve the 6% survival improvement for steelhead.

59. NOAA also states on page 387 of the 2014 BiOp that installation of surface passage routes and kelt-specific operations at The Dalles Dam have likely increased the survival of in-river migrating kelts (and adult steelhead falling back at the dams), but the limited number of reach survival estimates are not definitive. In 2012, nine (9) natural origin B-run steelhead kelts that had been successfully reconditioned were released into the Snake River and the action agencies claimed a 0.5% survival improvement toward achieving the 6% survival improvement goal from kelt reconditioning. *See* 2012 Kelt Management Plan at 9-10 (July 9, 2013) (2014 NOAA AR B39 at 2658-2659). The action agencies also say in the 2012 Kelt Management Plan that the goal of the Plan is to increase B-run female *spawners* by 6% or 180 females above the 3000 female base. The release of nine fish, even if they all spawn, equals 5% of the 180 kelt goal and even these fish will contribute to reaching the 6% steelhead population survival increase

of the RPA only if all of the questions and uncertainty the ISRP identifies turn out to be immaterial, e.g., if it turns out, for example, that kelt released back to the river are 100% as effective in spawning and reproducing as a wild female and in increasing adult returns. It also is not clear how the action agencies arrived at the 0.5% credit for the release of these 9 fish. Even if 9 fish are 5% of an overall 180 fish goal, 5% is only 0.3% of the 6% survival improvement the RPA requires.

60. NOAA does not discuss the ISRP's concerns from their reviews of kelt reconditioning projects underway in the Columbia River basin and from the ISRP's 2011 Retrospective Report. It also appears that there is currently no final Snake River Kelt Master Plan that the ISRP or others could review and evaluate. NOAA also describes several other areas of uncertainty in the 2014 BiOp, including several that I had identified earlier and some of those discussed above (e.g., the numbers of potential kelts collected has fallen well below the assumption used in the 2008 BiOp calculations, 2014 BiOp at 385). *See also* Olney 2008 SJ Dec. at ¶¶ 87-92; Olney 2008 SJ Reply Dec. at ¶¶ 30-36.

61. NOAA concludes its discussion of the "potential for long-term [kelt] reconditioning as a tool to increase the number of viable females" by noting:

One of the uncertainties surrounding the survival benefits of long-term reconditioning is the actual spawning success of reconditioned kelts. There are also questions relating to the nutrition and proper maturation of kelts being held in the long-term reconditioning program. Research is currently underway to assess these issues.

2014 BiOp at 386. These are also issues I raised in my summary judgment declarations. Olney 2008 SJ Dec. at ¶¶ 89-92. NOAA does not explain how these particular factors affect its analysis of the ability of RPA 33 to achieve a 6% survival increase for Snake River steelhead populations although it does conclude that this RPA will achieve the predicted survival increase. 2014 BiOp at 387.

D. NOAA's Base Period and Extended Base Period Analysis

62. Since the 2008 BiOp, three additional points relevant to NOAA's updated analysis for Snake River steelhead—and, for two of these points, other species as well—have become apparent. First, in the 2008 BiOp, NOAA used aggregate dam count estimates for Snake River steelhead to derive its Base Period values for the extinction risk metric and the three recovery metrics for the individual steelhead populations. *See, e.g.*, 2008 BiOp at 8.5-32 (discussing A-run population estimates), 8.5-50 (Table 8.5.2-1). I offered a number of observations about the uncertainty inherent in this approach for calculating individual steelhead population level metrics and individual population responses to RPA actions. *See Olney 2008 SJ Dec.* at ¶ 26 (explaining that using aggregate data in this fashion “introduces an element of uncertainty about the individual B-run population performance because the status of a few strong populations can mask the status of weak populations”). NOAA responded, through the Declaration of Dr. Christopher Toole, that it had acknowledged this uncertainty. *See Declaration of Christopher L. Toole* at ¶¶ 60-62 (dated Oct. 24, 2008).

63. As NOAA now recognizes, in light of subsequent research, its use of aggregate dam count data for this species is not valid because of recent studies that indicate a more complex structure for Snake River steelhead populations than is indicated by the previous A- and B-run classifications. *See 2014 BiOp* at 74-75 (noting this problem in the 2008 jeopardy analysis and stating that an alternative method to the use of aggregate dam counts will not be reliably available for two or three more years). Because NOAA recognizes that its aggregate method is now outdated, it does not attempt to calculate extended Base Period metric estimates for individual Snake River steelhead populations using the average A-run and average B-run data, so there are no extended Base Period estimates for the various jeopardy metrics for these populations. *See, e.g., id.* at 92 (Table 2.1-10) (no updated information for Snake River steelhead populations for the R/S metric) (yet stating that “all new estimates are within the 2008

BiOp's 95% confidence limits").

64. The analysis in the 2008 BiOp also used the average A-run and B-run estimates to calculate the prospective effects of population-specific tributary habitat RPA actions for the individual Snake River steelhead populations. *See* 2014 BiOp at 73-75 (identifying this issue). In light of the research discussed in the preceding paragraph, this is an approach that NOAA acknowledges is no longer valid. *Id.* The 2014 BiOp does not discuss how this limitation affects projected survival benefits assigned to individual Snake River steelhead populations from tributary habitat actions. *Id.* NOAA continues to assign the same habitat quality improvements from tributary habitat actions to the individual steelhead populations that it developed in the 2008 BiOp based on what is now recognized as an invalid method, 2014 BiOp at 73 (Table 3.1-1), and continues to report the extent to which these individual populations have met these habitat quality improvement standards (*e.g.* Lolo Creek, South Fork Clearwater, Lochsa, Secesh, etc.), *id.*; *see also id.* at 278-279.

65. NOAA Fisheries does not address the implications of continuing to rely on the 2008 BiOp's use of aggregate dam count data for the individual Snake River steelhead populations in the 2014 BiOp, or describe any risks that may be associated with continued use of this data, or consider any alternative approaches, other than to note that it "continue[s] to rely on the . . . average A- and B- run method, for lack of an alternative method . . ." 2014 BiOp at 75. The risks of continuing to rely on the aggregate dam count data as the basis for predicting the effects of the RPA on individual Snake River steelhead populations would include, for example, difficulty in determining the individual population response to tributary habitat measures and other RPA actions affecting steelhead. To the extent NOAA expects to rely on monitoring to make adjustments to the tributary habitat program for steelhead populations if actions are not as effective as assumed, the lack of any valid population data will make assessing the effectiveness

of habitat actions much more difficult and less reliable. Of course, NOAA also says elsewhere that it is unrealistic to expect empirical validation of habitat quality or survival improvements by 2018 or even much later. *See* RTC at 30-31 (Comment and Response C-11) (2014 NOAA AR 288216 at 288245-246).

66. The second new issue relevant to NOAA's 2008 BiOp analysis for Snake River steelhead—and for other ESUs/populations—is the acknowledgement in the 2014 BiOp that the currently available data indicate adult survival through the FCRPS has been lower during the recent period than during the approximately 20-year Base Period for Snake River spring/summer Chinook, Snake River sockeye, and Snake River steelhead. 2014 BiOp at 351-355 (discussing this issue and providing details on these lower survival rates). The 2008 BiOp assumed that these adult survival rates would be the same for both the Base Period and the time since then. I explain below how this issue is relevant to NOAA's 2014 BiOp's updated analysis.

67. NOAA based adult survival assumptions in the 2008 BiOp on new, stock-specific detection methods using Passive Integrated Transponders or "PIT" tags to identify the origin of adults passing Bonneville, McNary and Lower Granite dams (for 2002 and 2006-2007). Because they had no PIT tag data for the Base Period before 2002, the 2008 BiOp's assumption was that Base Period survival was the same as that estimated from PIT tags in 2002 and 2006-2007. In the 2008 BiOp, NOAA also used PIT tag detections from upper Columbia River sockeye stocks as surrogates to establish assumed survival rates in the lower Columbia River reach and extrapolated these to assess likely survival rates for the entire Bonneville to Lower Granite Dam migration corridor. *See* 2014 BiOp at 351-355 (discussing this issue). Based on this approach, NOAA reported in the 2008 BiOp that it would use an adult survival rate for Snake River sockeye of 81.1%, although it considered this estimate too uncertain to use as an actual adult survival performance standard for this species. *Id.* at 353-354. NOAA now has 2010-2012 PIT

tag-based data for adult survival, a direct measure of this survival rate, rather than an extrapolation. This direct estimate of adult survival is 70.9% for Snake River sockeye which is more than 10% lower than the 2008 BiOp assumption. *Id.* at 354.

68. Based on similar recent data for other ESU's and DPS's, adult survivals are also lower than assumed in the 2008 BiOp for Snake River spring/summer Chinook and Snake River steelhead, and it is unclear if they have declined or not for mid-Columbia River steelhead.⁵ They were higher for Snake River fall Chinook, and upper Columbia River spring Chinook and steelhead. 2014 BiOp at 352 (Table 3.3-1). For these and other species, NOAA used its 2008 adult survival assumptions as adult performance standards. *Id.* at 351 (citing 2008 BiOp RPA actions 52 through 54). NOAA does not discuss why these recent survival rates, based on actual data rather than extrapolations, are lower (or higher) than the assumptions it used in the 2008 BiOp. *Id.* at 351-355. It also does not describe or discuss the implications of these new survival rates for its analysis of the effects of the RPA. *Id.* Regardless of the factor or factors causing the lower estimates of adult survival, and regardless of whether they eventually prove to be accurate, the implication of the lower survival rates through the hydrosystem is that positive expectations of future population improvements for most of the Snake River populations are at least more uncertain than anticipated, and may also prove to be too optimistic.

69. NOAA does explain that it is not certain whether the new estimates represent a true difference from the Base Period adult survival rates it assumed in the 2008 BiOp. Consequently it says it does not yet consider this new information as indicative of an RPA

⁵ Adult passage was blocked at Lower Granite Dam on two separate occasions in 2013 which resulted in adult losses (substantial for sockeye (about 30%) and significant for summer Chinook (about 15%) and less for Snake River fall Chinook (about 7%) and steelhead (about 12%)), based on PIT tag based conversion rate estimates. NOAA has required the action agencies to implement operational changes and physical structures to address the blockage that occurred at Lower Granite Dam in 2013. This problem, however, illustrates that serious adult passage problems can still occur.

implementation problem because it cannot identify the factor that is responsible for the lower than expected survival rates. 2014 BiOp at 354. NOAA also says in its response to comments on the draft 2014 BiOp that, “[t]he validity of the PIT tag method of estimating adult upstream survival has been generally confirmed [but it is pursuing] further analyses to identify potential sources of bias” RTC at 51 (comment/response E-12) (2014 NOAA AR 288216 at 288266). In other words, even with generally confirmed empirical methods for estimating adult upstream survival, and the updated empirically based adult survival estimates that are lower than those on which the 2008 BiOp relies, NOAA says it will not discuss or include in its analysis any potential negative implications of the new data until it has identified potential sources of bias. *See* 2014 BiOp at 355 (describing additional studies).

70. The third issue relevant to the analysis in the 2014 BiOp is not actually new but the actions in the RPA have changed and the change affects this issue. In my 2008 declarations I addressed how transportation may impair homing ability and increase straying and mortality of adult salmon and steelhead. Olney 2008 SJ Dec. at ¶¶ 125-129. In the 2008 BiOp NOAA presented adult conversion rate data for adult fish that had been collected and transported as juveniles and for fish migrating as juveniles in the river. In Table 14.1 of the 2008 BiOp’s Incidental Take Statement, the reduction in adult escapement due to transportation was 6.1% for Snake River fall Chinook, 6.9% for Snake River spring/summer Chinook, and 6.8% for Snake River steelhead. 2008 BiOp at 14-21 to 14-23 (Table 14.1). It was listed as unknown for Snake River sockeye. *Id.* In the 2010 Supplemental BiOp, NOAA acknowledged that transported Snake River spring/summer Chinook salmon and steelhead stray at higher rates than fish that migrated in the river as juveniles. 2010 BiOp at 70. NOAA explained that “[c]ompared to assumptions in the 2008 BiOp, recent spill operations at the Snake River collector projects have resulted in substantially lowered transportation rates (compared to either the Base or Current

conditions). This should substantially reduce the number of Snake River steelhead adults straying into affected MCR steelhead populations (primarily those in the Deschutes and John Day rivers) as a result of juvenile transportation operations, and thus reduce negative genetic impacts to these MCR populations.” *Id.* at 78 (emphasis added).

71. NOAA did not include adult conversion rate estimates for transported fish in the 2014 BiOp. They also did not discuss the effects of the proposed increase in transportation under the revised RPA on homing impairment and straying in updating their analysis. *See* 2014 BiOp at 367-376 (discussing RPA transportation actions); 375 (“modifications to RPA action 30 should result in somewhat higher transportation rates, compared to recent operations for both SR spring/summer Chinook salmon and steelhead smolts”). They only generally state that “previously described effects of recent operations on SR spring-summer Chinook salmon, sockeye salmon, and steelhead are generally expected to continue through the remainder of the BiOp.” *Id.* at 376. While NOAA concluded in the 2010 BiOp that the reduced transportation rates under recent spill operations would “substantially” reduce straying, and consequently improve adult conversion rates, it did not address in the 2014 BiOp the negative effects that increased transportation under the revised RPA could have on straying. For example, achieving a goal of transporting 50% of juvenile steelhead is a significant change from the 28-49% of juvenile steelhead that were transported between 2007 and 2013. The Fish Passage Center addressed this issue in their comments on the draft 2014 BiOp and noted that NOAA has not updated its analysis of the effects of increased transportation on straying or adult conversion rates. Fish Passage Center, Review Comments: 2013 Draft FCRPS Supplemental Biological Opinion at 16-18 (Oct. 7, 2013) (Exhibit B). The impact of transportation on straying has been shown to be significant and the effect of increased transportation would be expected to increase straying and reduce adult conversion rates to some extent. NOAA’s response is that “overall

transport rates will remain substantially lower than those expected in the 2008 FCRPS BiOp for all spring-migrating species (though somewhat higher than observed recently) and the previously described effects of recent operations . . . are generally expected to continue through the remainder of this BiOp.” 2014 BiOp at 375-376. NOAA does not explain how a revised RPA that will limit an action which for the last eight years NOAA says has “substantially” reduced straying will allow the “effects of recent operations . . . to continue through the remainder of the BiOp.”

72. In addition, when a higher percentage of juvenile fish are left to migrate in-river it reduces predation rates because the total number of fish available to predators increases which has a swamping effect on predation. *See* Olney 2008 SJ Dec. at ¶¶ 116-121 (discussing this issue). The 2014 BiOp acknowledges that uncertainty about the degree to which removing juveniles from the river for transportation would have affected predation rates on the juvenile fish remaining in the river is a complicating factor (along with configuration changes) in how the 2008 BiOp transport operations would have performed relative to the actual operation under the Court Order. 2014 BiOp at 369. While the degree of predation rate change from transporting more fish under the 2014 BiOp compared to the recent operations may be uncertain, the direction of that change is not uncertain: it would significantly reduce the population migrating in-river which would reduce the effects of swamping and consequently increase predation rates on juvenile fish migrating in-river. The ISAB in their 2008-5 review of spill and transport on pages 23-24 also addressed this issue and reached a similar conclusion. NOAA does not explain how the effect of predator swamping under the current operations . . . “are generally expected to continue through the remainder of this BiOp”. . . when a much higher percentage of the fish will be removed from the river for transport under the 2014 BiOp’s proposed transport operations. Indeed, it appears that with recent (2010-2012) levels of in-river migration for steelhead, any

benefits from transportation have all but disappeared, suggesting that any benefits of removing more fish from the river for transportation, as the revised RPA proposes, may not be supported by the available data.

Pursuant to 28 U.S.C. § 1746, I declare under penalty of perjury that the foregoing is true and correct to the best of my knowledge. Executed this 4th day of December, 2014, at Nittenau, Germany.

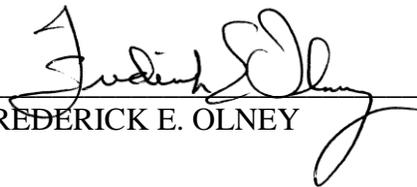

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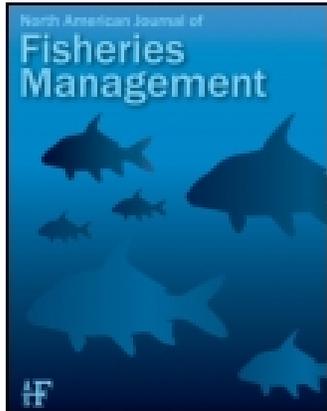
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Modeling Population Dynamics and Fish Consumption of a Managed Double-Crested Cormorant Colony in Minnesota

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ARTICLE

Modeling Population Dynamics and Fish Consumption of a Managed Double-Crested Cormorant Colony in Minnesota

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Abstract

The recovery of the double-crested cormorant *Phalacrocorax auritus* throughout North America has led to increased human–cormorant competition over fishery resources and has forced managers to evaluate cormorant effects and consider management alternatives at local and broader scales. We present a method for modeling local double-crested cormorant populations under varying levels of culling intensity based on a colony managed at Leech Lake, Minnesota, during 2005–2011. In this case study, the cormorant colony was evaluated under no, moderate, and intensive control rates and compared with the observed population response. Cormorant diets for fledged (adults and subadults) and nestling cormorants were described during 2004–2007 and 2010. Annual fish consumption and 95% confidence intervals were estimated from 1992 to 2011 using a bioenergetics model and Monte Carlo methods. Total feeding effort and fish consumption has been reduced by nearly 90% since cormorant control began in 2005 (consumption reduced from 20.01 kg/ha in 2004 to 1.98 kg/ha in 2011) and by 46–73% annually, depending on the number of birds arriving each spring and the applied culling intensity. Averaged across all years and periods, fledged cormorants consumed 0.75 kg·bird⁻¹·d⁻¹ and nestlings consumed 0.45 kg·bird⁻¹·d⁻¹. Respectively, average fledged and nestling diets were comprised of Yellow Perch *Perca flavescens* (61.0% and 77.4%), *Coregonus* spp. (12.3% and 9.4%), minnows *Notropis* spp. (9.9% and 2.2%), Trout-perch *Percopsis omiscomaycus* (4.1% and 0.4%), and Walleye *Sander vitreus* (4.6% and 3.6%), though considerable seasonal and temporal variability was observed. The bioenergetics model used to estimate total fish consumption was sensitive to this variability, which was strongly associated with the dynamics of the population of Cisco *Coregonus artedii*. Some fish population and fishery statistics were described by changes in cormorant predation pressure, but these relationships were compromised by other concurrent management activities.

Historically, double-crested cormorants *Phalacrocorax auritus* (hereafter termed “cormorant”) were widely distributed throughout North America. Wires and Cuthbert (2006) reviewed the decline of continental cormorant populations throughout the

European settlement era of the late 19th and early 20th centuries until federal protection under the U.S. Migratory Bird Treaty Act in 1972, the banning of DDT in the same year, and the development of large-scale aquaculture facilities afforded an

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opportunity for marked recovery. Currently, the species is widespread with five major breeding areas: Alaska, Pacific coast, Canadian and U.S. interior, Gulf Coast, and Atlantic coast (Wires and Cuthbert 2006).

The improvements to continental cormorant populations have not gone unnoticed, as debates over cormorant impacts on fisheries resources have increased (Muter et al. 2009). Now more frequently viewed as a nuisance wildlife species, cormorant issues can polarize among interest groups in both definition and remedial management (Warburton and Norton 2009). Even resource professionals have strongly disagreed on the interpretation of data implicating cormorants as a causative factor in fishery declines and associated management solutions (Belyea et al. 1999; Diana et al. 2006; Fielder 2008; Diana 2010; Fielder 2010). In an effort to deal with the controversy, the U.S. Fish and Wildlife Service (USFWS), in conjunction with the U.S. Department of Agriculture Wildlife Services Division, prepared an Environmental Impact Statement and Final Rule establishing a Public Resource Depredation Order for controlling cormorants in 24 states (Wires and Cuthbert 2006), one of which is Minnesota.

As continental cormorant populations continue to recover in number, investigations describing their role as a significant piscivore in aquatic ecosystems and the potential effects on fish communities and the fisheries they support are increasing. Cormorants can negatively impact aquaculture in their winter range (Glahn et al. 2000). Furthermore, in some cases cormorants have been implicated as a causative factor in sport fish population declines and associated reduced recreational harvest (VanDeValk et al. 2002; Rudstam et al. 2004; Fielder 2010), as well as adversely impacting local economies dependent upon the affected fishery (Shwiff et al. 2009). Most investigations, however, have concluded that cormorant impacts are negligible to fish populations and their fisheries (Engström 2001; Seefelt and Gillingham 2006; Barks et al. 2010).

Given the shift in public perception of cormorants from that of a victim of anthropogenic disturbance to a perpetrator now threatening the quality of resources (Muter et al. 2009), investigations describing the effects of cormorants on fisheries are required. Previous studies have described considerable variability in *Phalacrocorax* spp. diets (Johnson et al. 2006; Čech et al. 2008; DeBruyne et al. 2013); consequently, study design may have a strong influence on findings. Many studies have employed a bioenergetics approach to evaluating cormorant consumption, where caloric content of the prey has a substantial influence on total consumption estimates (Madenjian and Gabrey 1995). Thus, sampling cormorant diets during seasons or years when a system sustains relatively abundant high-calorie prey (e.g., Alewife *Alosa pseudoharengus* or Cisco *Coregonus artedii*) of preferred size, which can be driven by recruitment variability and growth, could lead the researcher to incorrectly conclude that cormorant diets are consistently dominated by these species.

The number and size of double-crested cormorant nesting colonies in Minnesota have increased considerably since 1970

(Wires and Cuthbert 2006). On Leech Lake, cormorants were prominent during the early 1800s but disappeared as a nesting species for over 100 years (Mortensen and Ringle 2007). They recolonized the lake in the early 1990s and quickly became one of the single largest breeding colonies in the state as the population expanded from 73 nesting pairs in 1998 to 2,524 nesting pairs in 2004. As in other continental locales where cormorants have shown marked improvements, public concern over the risk cormorants pose to stocks of sport fish in Minnesota has also heightened. Standardized fish population assessments conducted annually by the Minnesota Department of Natural Resources (MN DNR) since 1983 documented concurrent declines in percid stocks as the cormorant population expanded during the early 2000s. Gill-net catch rates of Wall-eye *Sander vitreus* and Yellow Perch *Perca flavescens* declined as the cormorant colony expanded at an exponential rate during 2001–2004 (Figure 1). Associated fishing pressure, catch, and harvest within the recreational fishery also declined during this period (Figure 2). Furthermore, the expanding cormorant population indirectly threatened some of the five other species of colonial waterbirds nesting on Leech Lake, in particular the common tern *Sterna hirundo*, which is listed as a threatened species in Minnesota and a species of conservation concern for the Leech Lake Band of Ojibwe (USDA 2005).

In response to both social and biological concerns, federal, state, and tribal resource agencies prepared an Environmental Assessment outlining the resource conflict issue and management alternatives (USDA 2005). In 2005, the Leech Lake Band of Ojibwe's Division of Resource Management (DRM) sought and received approval from the USFWS to take more than 10% of the cormorant population under the Public Resource Depredation Order to a population target of 500 nesting pairs with an anticipated total annual consumption goal below 3.85 kg/ha. The consumption goal was adapted from then recently completed cormorant research on Oneida Lake, New York, suggesting consumption above this level negatively impacted percid populations (Rudstam et al. 2004). Oneida Lake and the main basin of Leech Lake are morphometrically similar and both fish communities are dominated by percids (USDA 2005), though Oneida Lake is smaller and more productive. Fish population trends would continue to be monitored by the MN DNR and the cormorant management goal would be refined as additional information, such as cormorant diet, was collected and evaluated.

The Leech Lake cormorant colony has been managed annually since 2005 under varying levels of culling intensity. Most culling consisted of adult and subadult birds pass-shot by shotgun as they returned to the nesting island from feeding. As part of the approval to remove over 10% of the colony, the DRM also received funding from the USFWS to conduct a cormorant diet study. Therefore, our objectives were to model the cormorant population under pre- and postmanagement conditions and contrast differing cormorant management scenarios, to describe the associated changes in total fish consumption, and to evaluate

DOUBLE-CRESTED CORMORANT POPULATION DYNAMICS AND FISH CONSUMPTION

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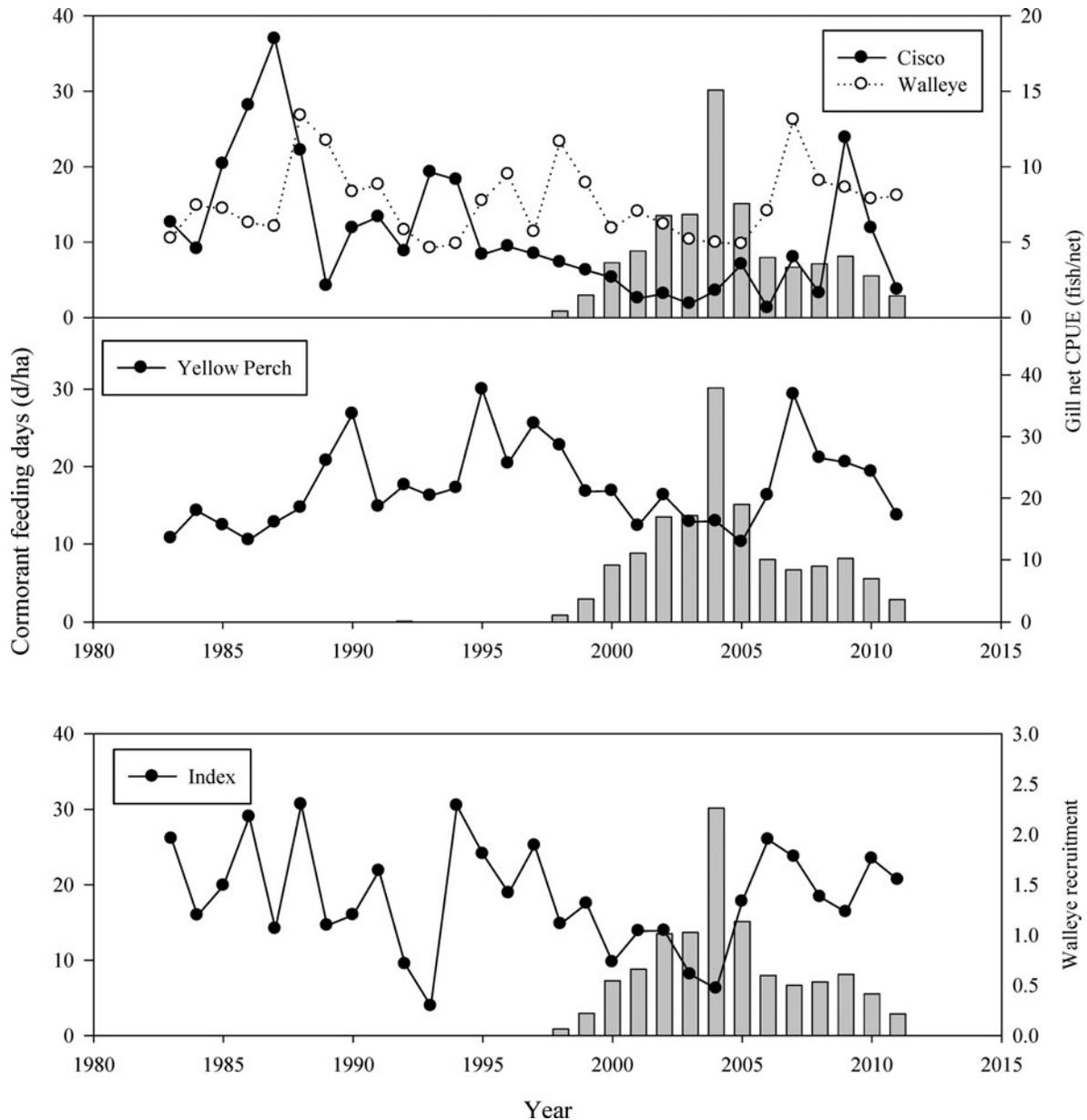


FIGURE 1. Double-crested cormorant feeding days (bars; d/ha), gill-net catch rates (fish/net) of Cisco, Walleye, and Yellow Perch (top two panels), and Walleye recruitment index (bottom panel) at Leech Lake, Minnesota, 1983–2011. The cormorant population has been managed annually since 2005.

changes, if any, in populations of fish species of interest and the associated fishery.

STUDY AREA

Leech Lake, located in north-central Minnesota, is the third largest inland lake in the state (45,158 ha; Figure 3). The lake has an irregular shape with many large and small bays and varies considerably from a morphological perspective. Some large bays are shallow and heavily vegetated with native aquatic

plant species, whereas others are deep and vegetated only on the perimeter, having properties more consistent with oligotrophic lakes. The fish community is dominated by percids and esocids. Walleye, Northern Pike *Esox lucius*, and Muskellunge *E. masquinongy* are the primary predators, while Yellow Perch, Cisco, a low-density population of Lake Whitefish *Coregonus clupeaformis*, Trout-perch *Percopsis omiscomaycus*, and several minnows *Notropis* spp. are the principal prey species. Cormorants nested on Gull (0.25 ha) and Little Pelican (1.1 ha) islands in the main basin (38,006 ha). The substrate of both

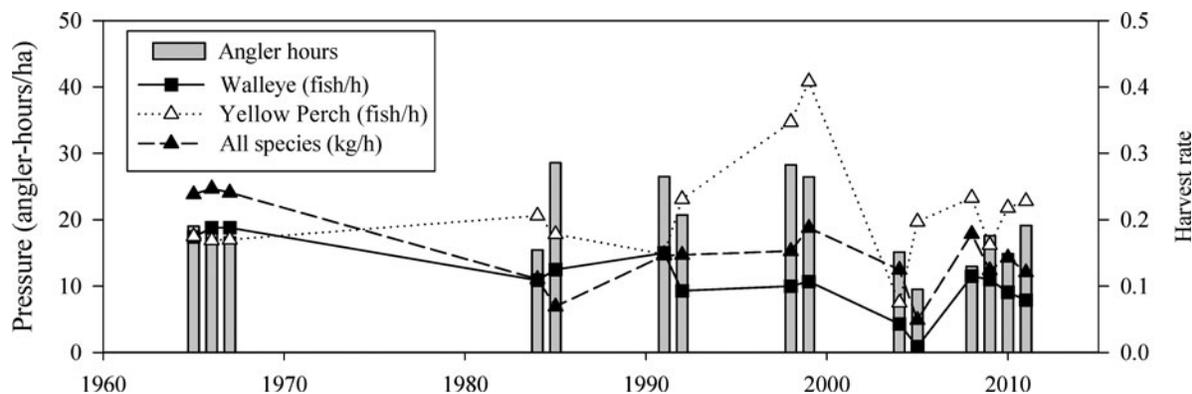


FIGURE 2. Trends in recreational fishing pressure (bars; h/ha), harvest rate of all species (kg/h), and harvest rates (fish/h) of Walleyes and Yellow Perch at Leech Lake, Minnesota.

islands is composed of a mix of large boulders, gravel, and sand, and both islands are essentially treeless. Nearly all birds are ground nesters and, due to the openness of the nesting site, aerial or ground counts are easily accomplished. Cormorants fed almost exclusively within the main lake basin (USDA 2005); however, all analyses in this manuscript refer to the entire lake.

METHODS

Population modeling.—Active cormorant nests and fledglings have been counted each year since recolonization occurred in 1992 (Mortensen and Ringle 2007; S. Mortensen, unpublished), and this is the most consistent record of temporal

changes in cormorant density (Table 1). Control activities at the nesting site during 2005–2011 compromised counts of the peak number of nests observed. Consequently, we estimated the maximum nesting potential by dividing the total number of estimated adults by two and used this statistic when estimating foraging effort and consumption of the population in the absence of control during these years.

Weekly estimates of adult, subadult (sexually immature but fledged birds, as defined by white plumage on the breast), and nestling (hatchling to fledge stages) age-classes have been maintained since control efforts were initiated in 2005. Using high-resolution aerial digital photos and GIS software to mark and count each bird, the DRM was able to enumerate adult and subadult cormorants either sitting on nests or loafing at the colony during 2005–2011. Loafing adult and subadult cormorants were readily distinguishable from those on nests. The proportion of subadults was calculated from ground counts conducted either from a distance or from blinds. The number of nests counted from aerial photographs was verified during the first few years of the study using ground counts of isolated groups of cormorant nests and comparing them with aerial counts. Photographs were taken three or four times throughout the nesting season each year. Flights were made around noon after feeding cormorants had returned to the rookery and were not conducted on days when control activities were occurring. Flights conducted during 2008–2011 also included the entire main basin of the lake to confirm that there were no significant numbers of feeding birds excluded from photographic counts. In the few cases when foraging groups were observed, they were also photographed and the count was added to the total numbers for the day.

Colony counts after the control work ceased were added to culling totals to estimate the peak spring population of cormorants preceding management activities. Weekly cormorant numbers were determined by subtracting the culling total at the end of the week from the estimated peak spring abundance. Similar to estimating peak abundance, estimating the rate of fall migration was determined by subtracting the differences

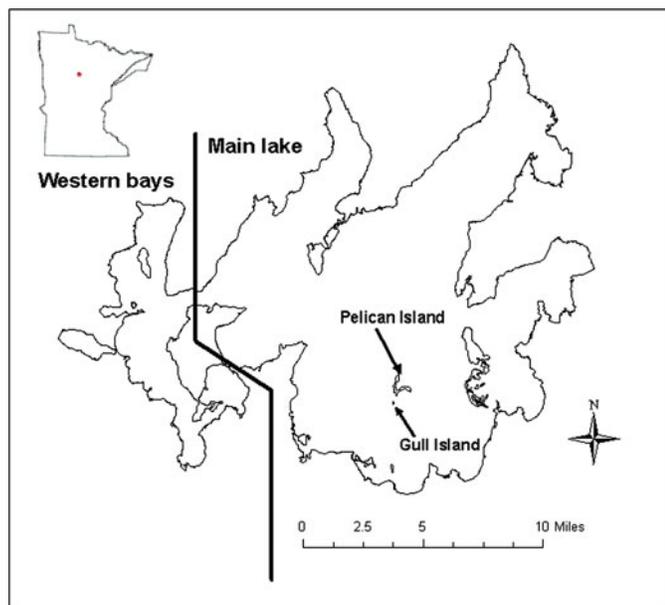


FIGURE 3. Leech Lake, located in north-central Minnesota, has been the primary location of cormorant management in the state since 2005. Nearly all feeding activity by cormorants occurs in the main lake basin.

TABLE 1. Size, fledge rate (fledglings/nest), and feeding effort (days/ha) of the double-crested cormorant colony at Leech Lake, Minnesota, 1992–2011. Feeding effort was modeled for each cormorant age-group under no (DCCO_n), log-linear (DCCO_l; 2005–2009), and exponential (DCCO_e; 2010–2011) population control rates and directly estimated using observed counts (DCCO_o) at the nesting site.

Year	Observed peak nests	Potential peak nests	Postcontrol nests	Fledge rate	Total feeding days (d/ha)								
					DCCO _n		DCCO _l		DCCO _e		DCCO _o		
					Fledged birds	Nestlings	Fledged birds	Nestlings	Fledged birds	Nestlings	Fledged birds	Nestlings	
1992	5			1.25	0.04	0.02							
1993	1			0	0.01	<0.00							
1994	0			0									
1995	0			0									
1996	0			0									
1997	0			0									
1998	73			1.37	0.64	0.23							
1999	249			2.33	2.18	0.79							
2000	608			2.71	5.33	1.93							
2001	737			2.50	6.47	2.34							
2002	1,130			1.99	9.92	3.59							
2003	1,144			2.50	10.04	3.63							
2004	2,524			2.00	22.15	8.01							
2005	922	2,353	700	0.30	20.65	7.47	7.78	2.57	6.05	1.63	14.06	1.06	
2006	857	1,962	532	1.59	17.22	6.23	6.95	2.36	5.58	1.62	6.94	1.04	
2007	705	1,518	563	2.12	13.32	4.82	6.01	2.13	5.06	1.61	5.07	1.59	
2008	1,244	1,728	487	2.00	15.16	5.48	6.46	2.24	5.31	1.61	5.73	1.40	
2009	949	1,420	473	2.30	12.46	4.51	5.80	2.08	4.94	1.61	5.95	1.52	
2010	862	1,745	348	2.20	15.32	5.54	6.49	2.25	5.33	1.61	5.00	0.54	
2011	566	780	357	2.01	6.84	2.48	4.45	1.74	4.18	1.59	2.37	0.51	

between the postfledge estimated number of birds and counts conducted during later dates.

We approximated the summer season of cormorant residency on Leech Lake as beginning the week of April 15 and ending the week of October 7, thus consisting of 27 weekly time steps (Table 2). The adult component of the modeled population was linearly increased during the first 4 weeks (mid-April to early May), after which we assumed all actively nesting adult birds were present; this mimicked spring immigration to the peak number of active nests in the colony. During weeks 4 (early May) to 19 (mid-August), we assumed zero immigration or emigration of cormorants under all scenarios modeled. The beginning of fall migration varied from year to year but typically began around week 20 (mid-August) and ended at week 26 (early October). The fall migration was fitted with a negative exponential function, and we assumed zero cormorants were present from week 27 (mid-October) to the following spring (mid-April).

Historical changes in cormorant nesting density and fledging success have been recorded at this site from the 1800s to 2007 (Mortensen and Ringle 2007), and the DRM has continued to maintain fledgling records (Table 1). Ground counts were employed periodically throughout the nesting season to estimate nesting success. Because control activities resulted in the fail-

ure of most initial nesting attempts early in the nesting season, counts during the years when control occurred were conducted after most control work was completed for the year. Hatch rates were estimated by examining a subset of the colony and counting the number of live nestlings present. Efforts were made to minimize disturbance to the nests of cormorants and other colonial nesting species when counts were conducted. We observed an average hatch rate of 3.00 nestlings/nest (Mortensen, unpublished), and we used a fledge rate of 2.20 fledglings/nest averaged from observations (1998–2004) reported by Mortensen and Ringle (2007); both statistics were similar to ranges reported for cormorants in other locations (Price and Weseloh 1986; Hatch and Weseloh 1999; Wires et al. 2001). Nestlings were maintained independently within the model from the time of hatching until fledged. Within the cormorant population model, nestlings hatched at the start of week 9 (early June) and fledged 8 weeks later. We multiplied weekly estimates of active nests by weekly numbers of nestlings per nest to avoid overestimating nestling abundances under modeled management scenarios. We assumed 100% recruitment of fledglings to the subadult age-class and emigration similar to the rest of the colony. Fledglings were treated as subadults when modeling consumption.

Subadult cormorants were estimated as the observed proportion relative to the number of nesting adults. This was done

TABLE 2. Weekly time steps of the model used to describe the cormorant population at Leech Lake, Minnesota. Proportions are relative to 100 total nests (base breeding pairs). Subgroups were independently varied by the percentages indicated to test sensitivity to total feeding effort.

Time step	Approximate date	Proportional abundances				Nestlings (N/nest)	Total feeding days			
		Breeding pairs	Nonbreeding adults	Subadults	Migrants		Base	Nonbreeding adults (+10%)	Subadults (+10%)	Migrants (+100%)
1	15 Apr	0.002	0.035	0.150	0.010		4	4	4	4
2	22 Apr	0.252	0.035	0.150	0.058		494	496	505	519
3	29 Apr	0.501	0.035	0.150	0.002		938	941	959	940
4	6 May	0.751	0.035	0.150	0.000		1,403	1,406	1,434	1,403
5	13 May	1.000	0.035	0.150	0.000		1,869	1,874	1,911	1,869
6	20 May	1.000	0.035	0.150	0.000		1,869	1,874	1,911	1,869
7	27 May	1.000	0.035	0.150	0.000		1,869	1,874	1,911	1,869
8	3 Jun	1.000	0.035	0.150	0.000		1,869	1,874	1,911	1,869
9	10 Jun	1.000	0.035	0.150	0.000	3.00	3,969	3,974	4,011	3,969
10	17 Jun	1.000	0.035	0.150	0.000	2.87	3,872	3,877	3,914	3,872
11	24 Jun	1.000	0.035	0.150	0.000	2.74	3,780	3,785	3,822	3,780
12	1 Jul	1.000	0.035	0.150	0.000	2.63	3,692	3,697	3,734	3,692
13	8 Jul	1.000	0.035	0.150	0.000	2.51	3,608	3,613	3,650	3,608
14	15 Jul	1.000	0.035	0.150	0.000	2.40	3,528	3,533	3,570	3,528
15	22 Jul	1.000	0.035	0.150	0.000	2.30	3,452	3,457	3,494	3,452
16	29 Jul	1.000	0.035	0.150	0.000	2.20	3,379	3,384	3,421	3,379
17	5 Aug	1.000	0.035	0.150	0.000		3,379	3,384	3,421	3,379
18	12 Aug	1.000	0.035	0.150	0.000		3,379	3,384	3,421	3,379
19	19 Aug	1.000	0.035	0.150	0.000		3,379	3,384	3,421	3,379
20	26 Aug	0.610	0.035	0.150	0.003		2,065	2,068	2,090	2,068
21	2 Sep	0.372	0.035	0.150	0.047		1,067	1,069	1,083	1,096
22	9 Sep	0.227	0.035	0.150	0.004		504	505	513	505
23	16 Sep	0.138	0.035	0.150	0.000		270	270	275	270
24	23 Sep	0.084	0.035	0.150	0.000		159	159	162	159
25	30 Sep	0.052	0.035	0.150	0.000		96	97	99	96
26	7 Oct	0.031	0.035	0.150	0.000		59	59	60	59
27	15 Oct	0.000	0.000	0.075	0.000		0	0	0	0
Total										
							53,954	54,043	54,712	54,014
% Change										
								+4.86	+4.86	+0.12

by counting large numbers of loafing cormorants either from a boat or from a blind on the islands and recording the number of birds with and without juvenile plumage. We also accounted for nonnesting mature adults, who either unsuccessfully paired or lost a mate during culling activities, and migrants. Direct enumeration of these groups was difficult because these birds were not obligated to reside at the colony. Consequently, we assumed abundances of these subgroups were directly proportional to the total abundance of nesting adult birds based on approximate observations. Model sensitivity to these assumptions was tested by independently varying nonnesting adults and subadults by +10% and migrants by +100% from their nominal value to examine the effect estimation error could have on total feeding days (Table 1). This analysis determined that most feeding ef-

fort at this location was exerted by the nesting colony proper, provided proportions of subgroups remained consistent with our approximations.

Weekly cormorant nest estimates varied considerably since 1992 due to population expansion and eventual control activities initiated in 2005. Cormorant population scenarios fitted with piecewise regression (Figure 4) consisted of the following criteria: (1) no control (DCCO_n), by which the population was assumed to remain constant with a slope of zero during weeks 5–19; (2) moderate population reduction described with a log-linear curve (DCCO_l) during weeks 5–14; and (3) intensive population reduction described with an exponential curve (DCCO_e) during weeks 5–12, meaning control efforts were disproportionately allocated towards the beginning

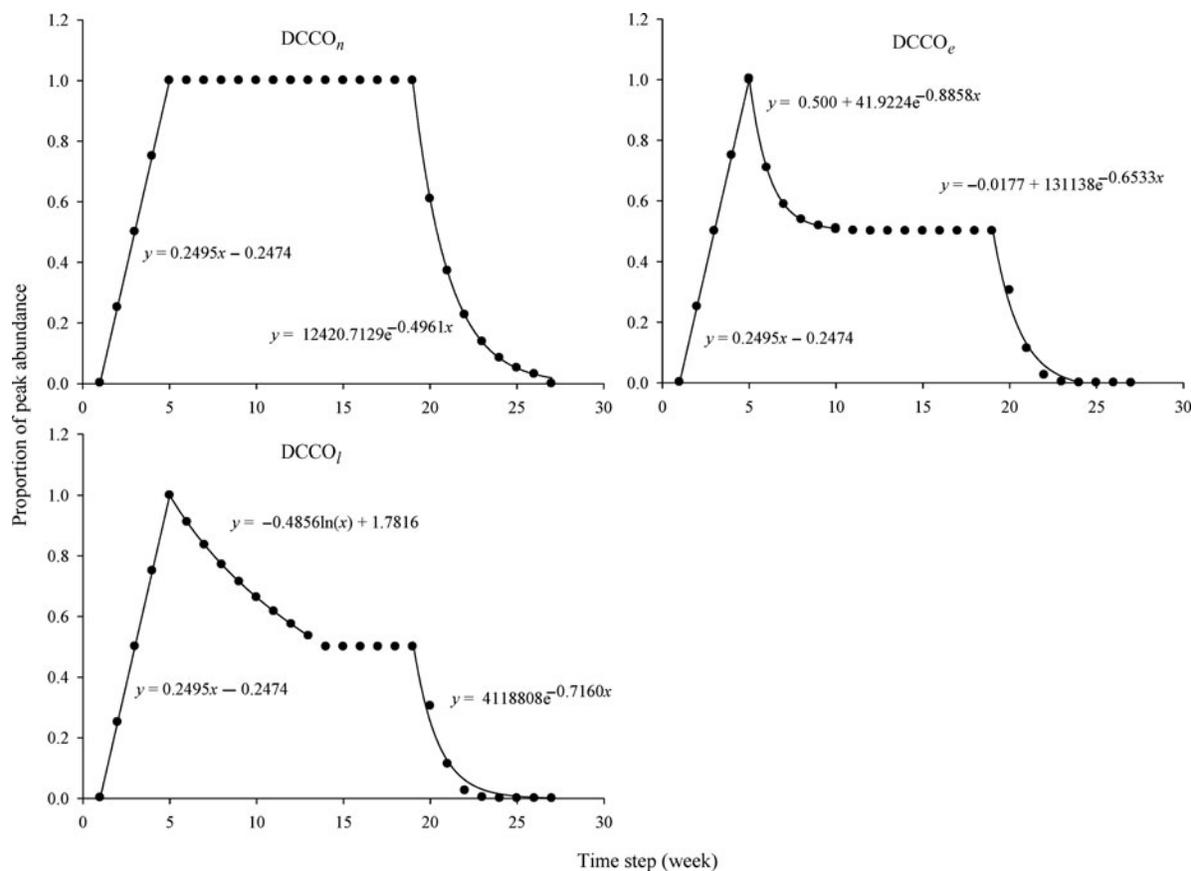


FIGURE 4. Piecewise regressions used to model nesting adult cormorants at Leech Lake, Minnesota, for a population peak of 1,000 nesting pairs controlled to 500 nesting pairs. Population control scenarios included no control ($DCCO_n$), a population reduced at a logarithmic rate during weeks 5–14 ($DCCO_l$), and a population reduced at an exponential rate during weeks 5–12 ($DCCO_e$). The weekly abundance of modeled populations was assumed to have a slope of zero when no control was occurring.

of the control season, thus having the potential to further reduce total cormorant feeding days within the limitations of the existing depredation permit. Actual control scenarios were modest ($DCCO_l$) during 2005–2009 and intensive ($DCCO_e$) during 2010–2011. None of the control models accounted for what was presumed to be a dispersal response to culling activities, evidenced by the continued decline of adult numbers and possible nest abandonment after control activities ceased for the year; this occurred to some extent during 2005–2009 but was most pronounced during 2010 and 2011 (Figure 5). Moreover, predictive models did not account for variability in nesting and fledging success. For these reasons, we also estimated the annual total feeding days for nestling and nonnestling age-classes for the observed population during 2005–2011 ($DCCO_o$) in addition to the modeled population scenarios to evaluate model assumptions by contrasting predicted and observed fish consumption estimates.

Cormorant diet.—Adult and subadult cormorant stomach contents were collected as cormorants returned from feeding events during 2004–2007 and 2010. Diets collected in 2004 were taken during late summer only. As detailed by Hundt (2009), a 10% buffered formalin solution was injected down

the esophagus of the collected adult and subadult cormorants to preserve stomach contents, the esophagus was sealed with a plastic band, and the samples were frozen until dissection, at which time cormorants were thawed for 24 h. The thawed cormorants were weighed and age was estimated by breast feather coloration (mature or subadult), sex was determined by gonad examination, and stomach contents were removed and stored in 10% formalin until identified. Regurgitated diet samples from nestlings were collected during the 2005–2007 and 2010 nesting seasons per Blackwell et al. (1995) and were also preserved in 10% formalin.

Most fish were identified to species; in some cases, well-digested fish were identified to genus or family, and unidentifiable fish were recorded as such. All minnows and coregonids were grouped to genus to expedite processing because of similar features. Ciscoes comprised the majority of coregonid abundance in Leech Lake and, although Ciscoes and Lake Whitefish were grouped for analysis, Ciscoes also comprised nearly all of the identified coregonids observed in diets. To reduce processing time, whole identifiable fish were measured to standard length (SL; mm) but were not weighed, and partially

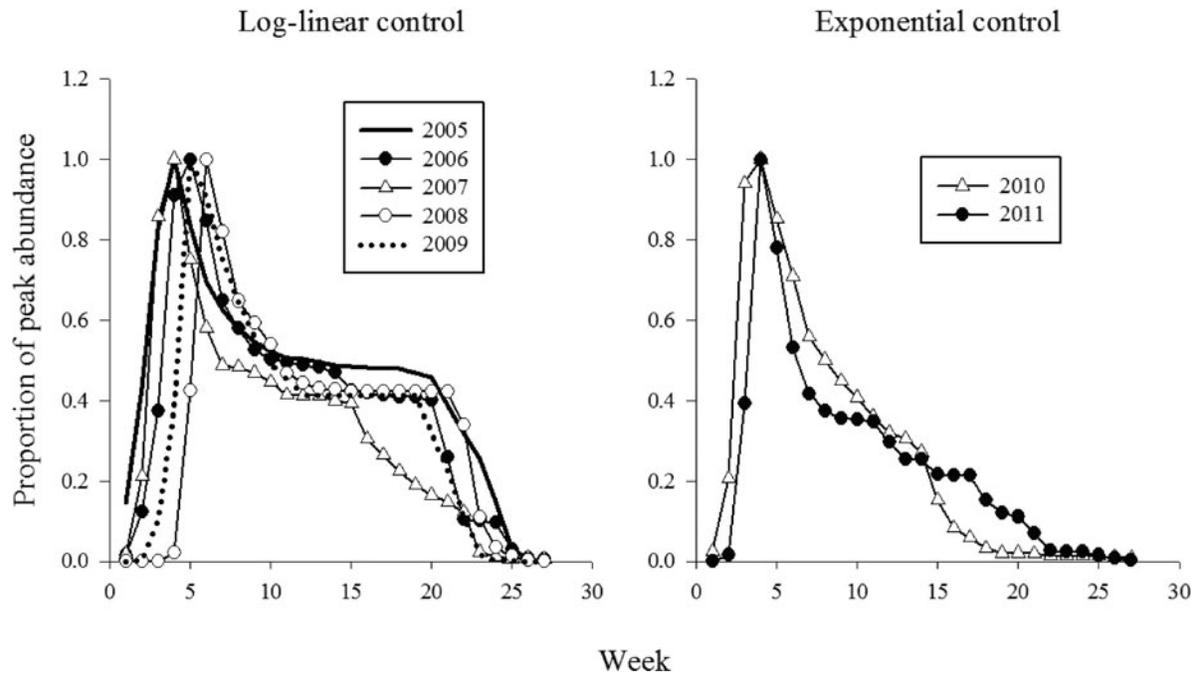


FIGURE 5. Observed cormorant population levels in response to different control rates at Leech Lake, Minnesota, 2005–2011. A behavioral response to increased control efforts during 2010–2011 in the form of continued dispersal after control efforts ceased (approximately week 11) was not taken into account in the predictive models. Week 1 approximates April 15.

digested (e.g., headless torso) and unidentifiable fish were enumerated. In some cases, a subsample of an abundant identifiable group (species and size-group) was measured and remaining similar fish were counted. This practice was most common with minnows and age-0 Yellow Perch, and lengths were assigned to unmeasured fish proportionately based on the measured subsample. Irregularities in the lowest identifiable taxonomic level of partially digested fish led us to assign individual fish lengths as the observed mean SL within sample month and family of identification. Whole unidentifiable fish were assigned the mean SL of the total measured sample specific to the collection date, under the assumption cormorants collected on the same day fed in the same general location and consumed similar prey. Weights of fishes observed in the diet were estimated with weight-length relationships summarized by Hundt (2009) using survey data collected at the study site by MN DNR, from the literature (Carlander 1969, 1977, 1997), and from specimens preserved at the Bell Museum of Natural History. The mass of unidentifiable fish was estimated using the Yellow Perch weight-length equation since this was the predominant prey item and best described the median size of prey observed in diets.

Modeling fish consumption.—Daily fish consumption rates ($\text{g}\cdot\text{bird}^{-1}\cdot\text{d}^{-1}$) were estimated using Madenjian and Gabrey's (1995) waterbird bioenergetics model. Sensitive parameters in the model identified by its authors included daily energy expenditure, proportion of total fish in the diet, assimilation efficiency, and caloric density of prey. Originally presented by Birt-Friesen et al. (1989), the equation used for estimating daily energy ex-

penditure (DEE; kilojoules) of a fledged bird is as follows:

$$DEE = 1737.8W^{0.727},$$

where W = mass (kg); DEE was then converted to kilocalories (1 kilojoule = 0.23892 kilocalories). The equation used to estimate DEE (kilocalories) of a nestling bird was estimated using the equation presented by Madenjian and Gabrey (1995):

$$DEEN = 1.230W^{0.7749},$$

where W = mass (kg). In both equations, cormorant weight is the independent variable; consequently, cormorant weight is the sensitive parameter (Göktepe 2012). For this reason, we adjusted individual observations of cormorant weight by subtracting the total mass of the observed diet. The mass of adult and subadult cormorants was tested for annual and seasonal differences using a fixed-effects analysis of variance (ANOVA) to provide insight on the levels of stratification among cormorant age-classes that should be considered when modeling consumption.

Nestlings were not weighed, and we assumed fledglings weighed the same as subadult cormorants (2.12 kg) observed during this study. We used the mass of nestling cormorants age 0–28 d reported by Dunn (1975a), which corresponded to nestlings up to 5 weeks old in our population model, to describe early nestling growth. The remaining difference in mass between nestlings 5 weeks old and fledglings was fitted linearly (Figure 6), and a weekly average nestling mass was calculated

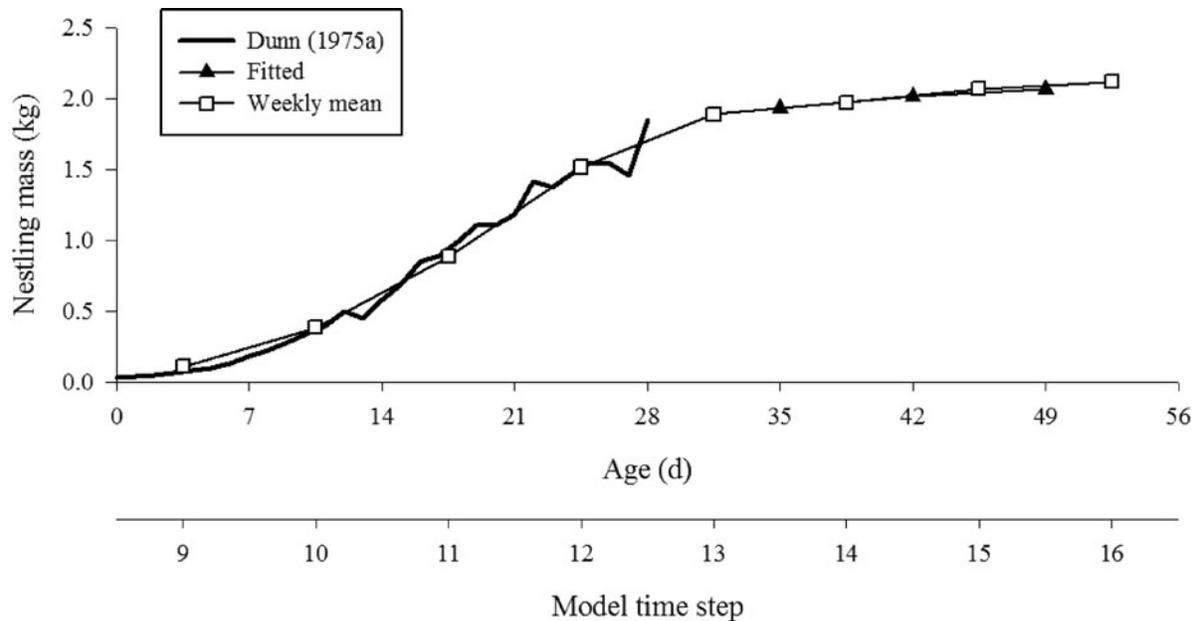


FIGURE 6. Weekly mass (kg) used to model nestling cormorant consumption.

for use in consumption models. We used a fixed SE = 0.03 kg around weekly estimated nestling mass in consumption models; this was the average of values for nestlings 0–28 d old calculated from Dunn (1975a).

In contrast to Madenjian and Gabrey (1995), who used an assimilation efficiency of 80% reported by Furness (1978) for seabirds, we used a value of 75.68% (SE = 1.51) (Brugger 1993). Brugger (1993) estimated the digestibility of Bluegill *Lepomis macrochirus* and other species by captive double-crested cormorants; the morphological structure of Bluegill is most similar to the species commonly observed in Leech Lake diets. The caloric densities of fish consumed were gathered from

the literature (Table 3). The caloric density of unidentifiable diet items was assigned as the weighted average of the observed caloric density of identified food items within the same period because of the influence caloric density can have on consumption estimates (Madenjian and Gabrey 1995).

The composition of cormorant diets collected during 2004–2006 varied significantly among years and biweekly periods within years (Hundt 2009). In contrast to this initial study, we partitioned the sampling year into three periods representative of nesting chronology (incubation, nestling, and postfledge) to mitigate some of this variability. We examined the differences in the caloric content (cal/g) among cormorant age-classes (adult,

TABLE 3. Caloric densities used to estimate total consumption of aquatic animals identified in cormorant diets on Leech Lake, Minnesota.

Family or genus	Caloric density (cal/g)	Source
Cyprinidae ^a	1,196	Hartman and Margraf 1992; Bryan et al. 1996; Madenjian et al. 1998
Catostomidae	1,019	Bryan et al. 1996
Ictaluridae	1,870	Hebert and Morrison 2003
Esocidae	955	Jonas et al. 1996
<i>Coregonus</i> ^a	2,826	Rottiers and Tucker 1982; Negus 1992
Gadidae	900	Wiens and Scott 1975
Percopsidae ^a	950	Hartman and Margraf 1992; Madenjian et al. 1998
Cottidae	1,493	Cummins and Wuycheck 1971
Gasterosteidae	1,493	Cummins and Wuycheck 1971
Centrarchidae	1,423	Hebert and Morrison 2003
Percidae ^a	1,100	Hartman and Margraf 1992; Hanson et al. 1997; Hebert and Morrison 2003
Cambaridae ^a	900	Wiens and Scott 1975; Hein et al. 2006
Planorbidae	50	Eggleton and Schramm 2004

^aAveraged across references.

TABLE 4. Observed mean bird weight (SE in parentheses), average caloric density (ACD) of diet items, and proportion of total diet composed of fish (PROP_f) used in Monte Carlo simulations to estimate total fish consumption by cormorants at Leech Lake, Minnesota.

Year	Period	Adults		Subadults		Fledged bird diet		Nestling diet		
		N	Weight (kg)	N	Weight (kg)	ACD (cal/g)	PROP _f	N	ACD (cal/g)	PROP _f
2004	Incubation									
	Nestling									
	Postfledge	89	2.31 (0.04)	78	2.09 (0.03)	1,157.05 (7.53)	0.999 (<0.001)			
2005	Incubation	68	2.31 (0.02)	18	2.22 (0.05)	1,295.28 (51.05)	1.000 (0.000)			
	Nestling	101	2.35 (0.02)	71	2.20 (0.02)	1,185.30 (22.92)	0.999 (0.001)	151	1,186.05 (26.57)	0.999 (<0.001)
	Postfledge	12	2.28 (0.06)	26	2.13 (0.03)	1,132.07 (13.87)	1.000 (<0.001)			
2006	Incubation	110	2.21 (0.02)	7	2.29 (0.12)	1,476.42 (60.72)	0.993 (0.007)			
	Nestling	104	2.27 (0.03)	27	2.08 (0.07)	2,013.11 (68.62)	1.000 (0.000)	114	1,282.54 (42.13)	0.999 (<0.001)
	Postfledge	23	2.25 (0.05)	15	2.07 (0.04)	1,170.73 (14.16)	1.000 (<0.001)			
2007	Incubation	57	2.34 (0.03)	14	2.16 (0.05)	1,138.85 (34.37)	1.000 (0.000)			
	Nestling	96	2.35 (0.02)	17	2.13 (0.05)	1,173.23 (19.07)	1.000 (0.000)	107	1,119.51 (7.53)	1.000 (0.000)
	Postfledge	33	2.30 (0.04)	13	1.94 (0.05)	1,192.61 (30.01)	1.000 (0.000)			
2010	Incubation	67	2.20 (0.03)	17	2.13 (0.06)	1,173.86 (30.90)	0.998 (0.001)			
	Nestling	136	2.13 (0.03)	36	2.03 (0.04)	1,596.03 (49.94)	0.999 (<0.001)	160	1,496.43 (51.47)	1.000 (0.000)
	Postfledge	11	2.10 (0.09)	16	2.09 (0.05)	1,455.95 (113.53)	0.998 (0.002)			
All years pooled	Incubation	302	2.25 (0.01)	56	2.19 (0.03)	1,294.97 (26.30)	0.997 (0.002)			
	Nestling	437	2.26 (0.01)	151	2.13 (0.02)	1,487.55 (26.30)	0.999 (<0.001)	532	1,286.69 (20.48)	1.000 (<0.001)
	Postfledge	138	2.28 (0.02)	148	2.08 (0.02)	1,186.41 (12.38)	0.999 (<0.001)			
All years pooled	All periods pooled	907	2.26 (0.01)	355	2.12 (0.01)	1,357.51 (15.10)	0.999 (0.001)	532	1,286.69 (20.48)	1.000 (<0.001)

subadult, and nestling), years, and periods using a fixed-effects ANOVA to guide the application of diet data to consumption models, specifically the assumptions that might be made when pooling diets among years or cormorant age-groups.

A 20,000-iteration stratified Monte Carlo simulation model was used to generate point estimates of consumption (kg) with 95% confidence intervals (95% CIs). Stratification levels included of adult and subadult cormorants (fledged birds), nestlings, year, and period within years. Model inputs included the observed means and standard errors of cormorant mass, the average caloric content of prey items, and the proportion of total fish in the diet (Table 4). Brugger's (1993) assimilation efficiency was a fixed parameter in all consumption models. Inputs into general consumption models were separated by nestling and fledged cormorant classes only, meaning other model inputs were pooled across years and periods within these classes, and these models were used to estimate total consumption from 1992 to 2011. Cormorant mass and feeding days were specific to all three levels of cormorant age-class (adult, subadult, and nestling), year, and period in stratified models (Table 4). Stratified models were run only for the observed cormorant population during 2005–2007 and 2010, years when the diet was described throughout the year, and compared to consumption estimates of the observed population generated with the general model. This was done to evaluate assumptions made when applying the general model, such as grouping adult and subadult cormorants and using overall pooled diet data, against a more specific approach with increased complexity. In all cases, total fish consumption estimates and their confidence intervals were summed across all stratification levels for an annual point estimate.

Fish population and fishery trends.—Creel surveys of the open-water recreational fishery have been intermittently con-

ducted during 1965–2011 (Figure 2), and winter creel surveys have been conducted less frequently. Approximately 80–90% of the observed total annual pressure and harvest occurs during the open-water season, and the fishery is tourism based; median distance traveled by angling parties approximates 250 km (Ward and Schultz 2012). We regressed fishing pressure (h/ha), angler harvest rate (HPUE) of all species (kg/h), Walleyes (fish/h), and Yellow Perch (fish/h) from open-water creel surveys conducted during 1998–2011 onto observed cormorant feeding days to test for effects of changing cormorant abundance on the fishery. Creel statistics were \log_e transformed to linearize the data. We lagged creel statistics by 1 year because we expected anglers to exhibit a behavioral response to changes in cormorant abundance, and cormorant predation on juvenile fish could impact harvest of older age-classes preferred by anglers the following year.

Standardized population assessments using multifilament experimental gill nets have been conducted annually since 1983. Similar to the creel time series, this data set was reduced to 1998–2011, the time period cormorants had completely established at the study site. We regressed \log_{10} transformed gill-net catch rates of Ciscoes, Walleyes, and Yellow Perch onto cumulative total cormorant feeding days (d/ha) accrued during years y , $y-1$, and $y-2$ under the premise that sustained predation on juvenile fish through age 2 would eventually be reflected as declining catch rates at age 3 and older. Walleye recruitment (Figure 1) was indexed using the gill-net catch rates of fish ages 1–3; growth data were available for both Walleyes and Yellow Perch (Figure 7), and maturity data were available for Walleyes only (Figure 7). We used a Kruskal–Wallis test (Zar 1999) to test for differences in 1992–1997, 1998–2004, and 2005–2011 groups of data for Walleye recruitment, growth,

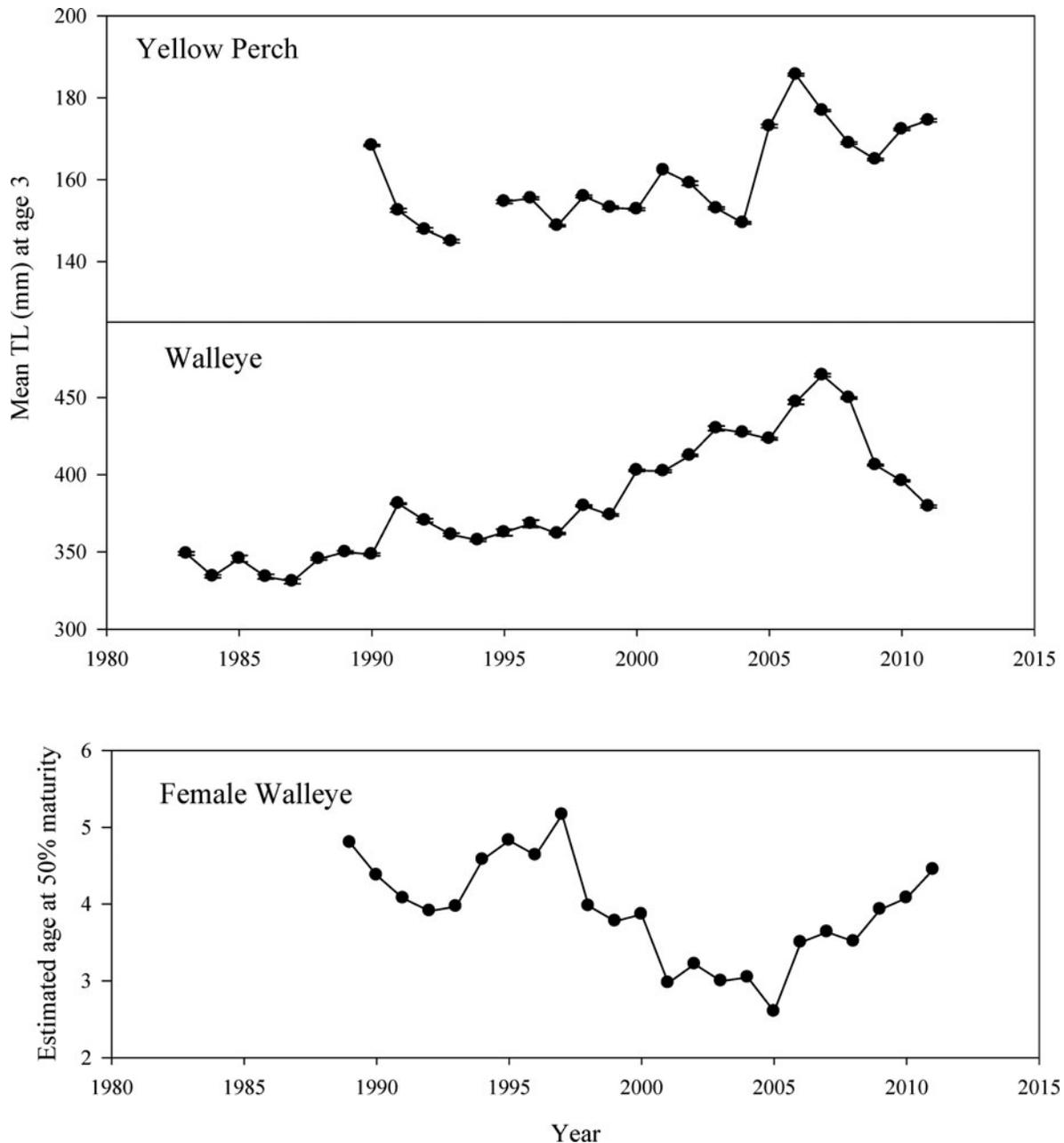


FIGURE 7. Growth rate of Yellow Perch and Walleye (top panels) and Walleye maturity rate (bottom panel) in Leech Lake, Minnesota, 1983–2011. Growth rate was indexed by mean TL (mm) of age-3 fish sampled, and maturity rate was the calculated age at 50% female maturity.

and maturity and Yellow Perch growth; these corresponded to time periods of relatively no cormorant presence, sustained cormorant reestablishment and expansion, and cormorant management at the study site. Recruitment, growth, and maturity data were tested against indices of cormorant predation pressure using a cohort-specific approach. Instead of lagging metrics by 2 years per Fielder (2010), we regressed these metrics onto the cumulative cormorant feeding days (d/ha) experienced by the cohort during ages 0–2. These metrics have been shown

to exhibit a density-dependent, population-level response to increased mortality in Minnesota Walleye populations (Gangl and Pereira 2003), though the authors presented them for monitoring exploitation. Fielder (2010) reported a positive relationship between Yellow Perch growth and cormorant predation, suggesting a similar population-level response to increased mortality regardless of the source. We used a more conservative $\alpha = 0.025$ for all regressions to reduce spurious assignment of significance that could result from multiple comparisons.

TABLE 5. Results of ANOVA model testing adjusted mass of fledged cormorants among years, periods, and their interactions; SS = sum of squares.

Age-group	Effect	SS	F-value	df	P-value
Adult	Model	67.87	6.34	12, 894	<0.0001
	Year	2.11	7.53	4	<0.0001
	Period	0.12	0.82	2	0.4389
	Year × Period	0.54	1.29	6	0.2575
Subadult	Model	20.41	2.69	12, 342	0.0017
	Year	0.52	2.39	4	0.504
	Period	0.56	5.18	2	0.0061
	Year × Period	0.42	1.3	6	0.2583

RESULTS

Sensitive Bioenergetic Model Parameters

Cormorant mass (mean ± SE) varied significantly among adult (2.26 ± 0.01 kg) and subadult (2.12 ± 0.01 kg) age-groups (ANOVA: $F = 76.48$; $df = 1, 1,260$; $P < 0.0001$). Independent testing of adult and subadult data for year, period, and year × period interaction effects suggested consumption model inputs should be stratified by age-class, year, and period within year for fledged cormorants when estimating consumption (Table 5).

Adult and subadult cormorant diet mass (Table 6) was comprised primarily of Yellow Perch (61.0%) and Ciscoes (12.3%), though other species were prominent (10.0% or higher) depending on the season and year. Yellow Perch comprised a greater overall proportion of nestling diets (77.4%; Table 6). Cisco

TABLE 6. Mean (SE in parentheses) composition (proportion of total mass) of fish species most frequently observed in cormorant diets at Leech Lake, Minnesota, 2004–2007 and 2010.

Year	Fish	Fledged birds				
		Incubation	Nestling	Postfledge	Pooled	Nestlings
2004	<i>Coregonus</i> spp.			0.004 (0.002)	0.004 (0.002)	0.004 (0.002)
	<i>Notropis</i> spp.			0.202 (0.012)	0.202 (0.012)	0.202 (0.012)
	Trout-perch			0.000	0.000	0.000
	Yellow Perch			0.639 (0.021)	0.639 (0.021)	0.639 (0.021)
	Walleye			0.029 (0.012)	0.029 (0.012)	0.029 (0.012)
2005	<i>Coregonus</i> spp.	0.053 (0.024)	0.041 (0.013)	0.012 (0.008)	0.041 (0.010)	0.027 (0.012)
	<i>Notropis</i> spp.	0.094 (0.019)	0.073 (0.008)	0.127 (0.025)	0.086 (0.008)	0.044 (0.009)
	Trout-perch	0.039 (0.017)	0.001 (0.001)	0.001 (0.001)	0.012 (0.005)	0.000
	Yellow Perch	0.600 (0.043)	0.845 (0.016)	0.755 (0.043)	0.762 (0.018)	0.835 (0.022)
	Walleye	0.013 (0.012)	0.006 (0.002)	0.054 (0.022)	0.014 (0.005)	0.018 (0.008)
2006	<i>Coregonus</i> spp.	0.230 (0.035)	0.529 (0.040)	0.015 (0.007)	0.339 (0.026)	0.105 (0.024)
	<i>Notropis</i> spp.	0.023 (0.005)	0.024 (0.009)	0.411 (0.045)	0.075 (0.011)	0.016 (0.005)
	Trout-perch	0.177 (0.028)	0.017 (0.008)	0.002 (0.001)	0.080 (0.013)	0.015 (0.005)
	Yellow Perch	0.510 (0.039)	0.400 (0.038)	0.442 (0.039)	0.451 (0.024)	0.791 (0.027)
	Walleye	0.018 (0.008)	0.023 (0.009)	0.102 (0.044)	0.031 (0.008)	0.049 (0.012)
2007	<i>Coregonus</i> spp.	0.000	0.018 (0.010)	0.024 (0.017)	0.014 (0.006)	0.000 (0.000)
	<i>Notropis</i> spp.	0.103 (0.025)	0.070 (0.020)	0.231 (0.054)	0.113 (0.017)	0.012 (0.006)
	Trout-perch	0.170 (0.030)	0.015 (0.009)	0.003 (0.001)	0.061 (0.011)	0.002 (0.002)
	Yellow Perch	0.584 (0.046)	0.615 (0.037)	0.466 (0.057)	0.575 (0.026)	0.854 (0.032)
	Walleye	0.049 (0.020)	0.154 (0.028)	0.101 (0.033)	0.111 (0.017)	0.047 (0.020)
2010	<i>Coregonus</i> spp.	0.020 (0.012)	0.285 (0.029)	0.195 (0.067)	0.198 (0.020)	0.213 (0.030)
	<i>Notropis</i> spp.	0.032 (0.011)	0.017 (0.003)	0.002 (0.001)	0.020 (0.004)	0.011 (0.005)
	Trout-perch	0.129 (0.030)	0.034 (0.008)	0.061 (0.038)	0.065 (0.011)	0.002 (0.001)
	Yellow Perch	0.625 (0.039)	0.569 (0.028)	0.502 (0.085)	0.579 (0.022)	0.651 (0.031)
	Walleye	0.063 (0.018)	0.051 (0.010)	0.147 (0.059)	0.064 (0.010)	0.036 (0.011)
All years pooled	<i>Coregonus</i> spp.				0.123 (0.013)	0.094 (0.012)
	<i>Notropis</i> spp.				0.099 (0.008)	0.022 (0.003)
	Trout-perch				0.041 (0.006)	0.004 (0.001)
	Yellow Perch				0.610 (0.016)	0.774 (0.014)
	Walleye				0.046 (0.007)	0.036 (0.006)

contribution to diets ranged from 0.5% to 52.9% and 0% to 24% of fledged and nestling diets, respectively. Walleye composition in diets ranged from near 0% to 10% for fledged cormorants and from 0% to 5% for nestlings.

Although differences among adult and subadult cormorants were observed, we expected similar caloric content among their diets because these age-groups were typically observed feeding together. Subadult cormorants contributed only an additional 7.5% of total feeding days for fledged birds within the population model. Observing similar caloric density among adult and subadult cormorants would facilitate simplified consumption models stratified only by nestling and fledged age-classes if differences in weights among fledged birds were ignored. The $\log_e(x + 1)$ transformed caloric content (calories/g) was similar among fledged cormorant age-groups (ANOVA: $F = 2.16$; $df = 1, 1,093$; $P = 0.1418$). We also observed strong year and period effects as well as a year \times period interaction ($F = 31.19$; $df = 11, 1,083$; $P < 0.0001$) in the caloric content of fledged cormorant diets. Conversely, nestling diets were of significantly lower caloric content than that of fledged birds (ANOVA: $F = 13.20$; $df = 1, 1,625$; $P = 0.0003$) and varied among years ($F = 20.20$; $df = 3, 528$; $P < 0.0001$). In all cases, differences in caloric content were suspected to be caused by changes in Cisco consumption. We tested this hypothesis by regressing the $\log_e(x + 1)$ transformed total caloric density of observed cormorant diets (years and seasons pooled) onto the $\log_e(x + 1)$ transformed mass of Ciscos observed in diets. Cisco mass explained most of the caloric variability in both nestling ($F = 3,023$; $df = 1, 530$; $R^2 = 0.85$; $P < 0.0001$) and fledged ($F = 5,974$; $df = 1, 1,260$; $R^2 = 0.83$; $P < 0.0001$) cormorant diets. The proportion of total fish in diets did not vary within or among years for fledged birds (ANOVA: $F = 0.70$; $df = 12, 1,249$; $P > 0.05$) or among years for nestlings (ANOVA: $F = 1.88$; $df = 3, 528$; $P > 0.05$) despite nonfish items, such as crayfish (family Cambaridae) or snails (family Planorbidae), occasionally observed in the diet.

Consumption Models

During 1983–2011, cormorant feeding days ranged from 0.00 to 30.16 d/ha (Figure 1) and fish consumption ranged from 0.00 to 20.10 kg/ha (95% CI = 18.09–22.08) (Figure 8). Relative to the observed population response to control activities, predictive models underestimated total fish consumption in most cases; these reflected differences between predicted and observed total feeding effort. Within the context of the observed variability in cormorant diet, a comparison of generalized (stratified by nonnestling and nestling age-groups only) and stratified consumption models using the observed population abundances and diets collected during 2005–2007 and 2010 showed the general model underestimated annual consumption in some cases (Figure 9). The differences between the two approaches were associated with overrepresentation of Ciscos in the average diet during the years Cisco consumption was observed to be lower. Seasonal consumption rates ranged from 0.465 (95% CI = 0.382–0.557) to 0.895 $\text{kg}\cdot\text{bird}^{-1}\cdot\text{d}^{-1}$ (95%

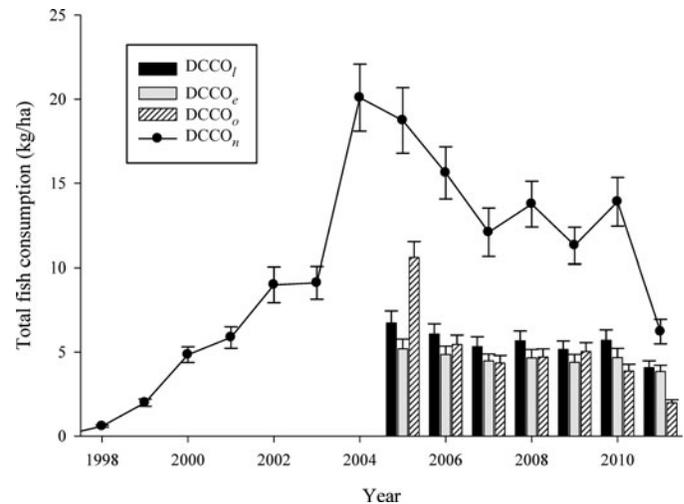


FIGURE 8. Total fish consumption (kg/ha; error bars show 95% CI) by the Leech Lake, Minnesota, cormorant colony in the absence of culling activities ($DCCO_n$), 1998–2011. The cormorant population was managed at control rates described by log-linear ($DCCO_t$; 2005–2009) and exponential ($DCCO_e$; 2010–2011) functions during 2005–2011, and total consumption was estimated for predicted and observed ($DCCO_o$) populations. Models were stratified only by cormorant age-groups (fledged birds and nestlings), and point estimates and their confidence intervals were summed across age-classes.

CI = 0.780–1.057) for fledged cormorants and from 0.409 (95% CI = 0.342–0.486) to 0.490 $\text{kg}\cdot\text{bird}^{-1}\cdot\text{d}^{-1}$ (95% CI = 0.428–0.547) for nestlings (Table 7). Daily fish consumption rates and total consumption by cormorants was lower when Ciscos comprised a greater portion of the diet.

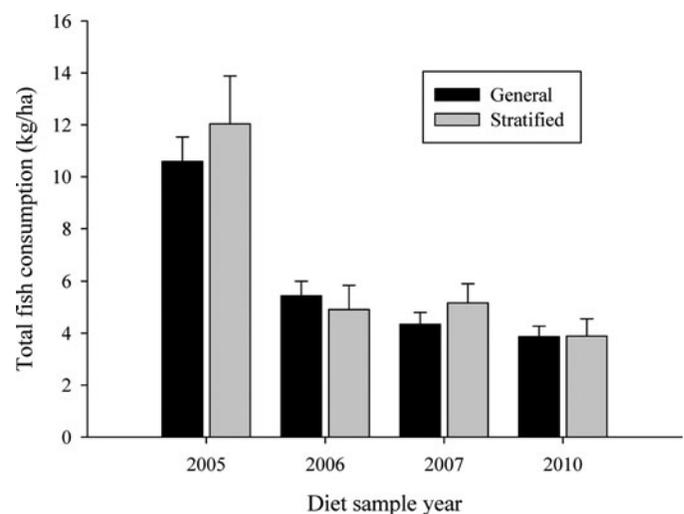


FIGURE 9. Estimated total fish consumption (kg/ha; error bars show 95% CI) of the observed cormorant population using general (stratified by fledged and nestling cormorants only) and stratified (adult, subadult, nestling, year, and period within year) models. The two approaches yielded similar estimates during the years when Ciscos were a significant diet item (2006 and 2010).

TABLE 7. Stratified Monte Carlo estimates and 95% confidence limits of daily fish consumption rates ($\text{kg}\cdot\text{bird}^{-1}\cdot\text{d}^{-1}$) from models stratified by year, period, and cormorant age-class for the years when cormorant diets were examined on Leech Lake, Minnesota.

Year	Period	Adult			Subadult			Nestling		
		Mean	95% Lower limit	95% Upper limit	Mean	95% Lower limit	95% Upper limit	Mean	95% Lower limit	95% Upper limit
2005	Incubation	0.780	0.668	0.929	0.758	0.643	0.908	0.458	0.398	0.532
	Nestling	0.861	0.762	0.985	0.821	0.731	0.928			
	Postfledge	0.883	0.785	0.990	0.840	0.757	0.943			
	Mean	0.841	0.738	0.968	0.806	0.710	0.926			
2006	Incubation	0.658	0.548	0.791	0.675	0.518	0.846	0.424	0.354	0.504
	Nestling	0.495	0.410	0.579	0.465	0.382	0.557			
	Postfledge	0.845	0.761	0.938	0.796	0.705	0.889			
	Mean	0.666	0.573	0.769	0.645	0.535	0.764			
2007	Incubation	0.895	0.780	1.057	0.845	0.729	1.008	0.490	0.428	0.547
	Nestling	0.871	0.785	0.978	0.811	0.716	0.918			
	Postfledge	0.884	0.736	0.980	0.745	0.639	0.871			
	Mean	0.883	0.767	1.005	0.800	0.695	0.932			
2010	Incubation	0.828	0.733	0.968	0.809	0.694	0.928	0.409	0.342	0.486
	Nestling	0.596	0.521	0.687	0.575	0.486	0.681			
	Postfledge	0.649	0.486	0.939	0.648	0.477	0.959			
	Mean	0.691	0.580	0.865	0.677	0.552	0.856			
All years pooled	All periods pooled	0.770	0.665	0.902	0.732	0.623	0.870	0.445	0.381	0.517

Fish Population and Fishery Trends

Fish abundance, fishing pressure, and harvest have all increased following the implementation of cormorant control. Most of the variability in creel statistics from 1998 to 2011 was explained by cormorant feeding effort (Table 8), except for the angler harvest rate of Yellow Perch; this relationship was strongly influenced by one outlier. The negative relationship between cormorants and most statistics of the fishery suggests cormorant management has positively affected the fishery. Statistical differences were observed in Walleye recruitment ($\chi^2 = 6.78$, $df = 2$, $P = 0.0270$) (Figure 1), Walleye growth ($\chi^2 = 11.15$, $df = 2$, $P < 0.0001$) and maturity ($\chi^2 = 8.99$, $df = 2$, $P = 0.0059$) (Figure 7), and Yellow Perch growth ($\chi^2 = 12.65$, $df = 2$, $P < 0.0001$) (Figure 7) among the 1992–1997, 1998–2004, and 2005–2011 time periods. Despite these differences and the observed increases in Yellow Perch and Cisco abundance following the onset of cormorant management activities, variation in gill-net catch rates of these species as well as changes in Yellow Perch growth rates were not explained by cormorant predation pressure.

Changes in all Walleye population metrics were associated with changes in cormorant feeding pressure. However, we suspect Walleye fry stocking has confounded interpretation of Walleye abundance, recruitment, and fishery statistics. Walleye fry were stocked at varying densities during 2005–2011, and these were also positively associated with recruitment during 1998–

2011 ($F = 7.83$; $df = 1, 12$; $R^2 = 0.40$; $P = 0.0161$) but not during 1992–2011 ($F = 1.65$; $df = 1, 20$; $R^2 = 0.08$; $P = 0.2132$), meaning stocked cohorts have remained within the range of observed historical recruitment variability. Furthermore, increases in Walleye harvest rates reflected increasing Walleye abundance ($F = 10.94$; $df = 1, 5$; $R^2 = 0.69$; $P = 0.0213$) concurrent with cormorant control and Walleye fry stocking, indicating that the effects of cormorant management on the Walleye population and its fishery are thoroughly confounded with other management actions. Walleye growth and maturity rates differed with changes in cormorant abundance and management, and trends in these metrics reversed for 2005 and later year-classes for both percid species as abundance increased.

DISCUSSION

Our estimated average daily fish consumption rate of $0.75 \text{ kg}\cdot\text{bird}^{-1}\cdot\text{d}^{-1}$ (95% CI = 0.64–0.89) for fledged cormorants was considerably higher than other studies (Dunn 1975b; Schramm et al. 1984; Glahn and Brugger 1995; Fowle 1997; Rudstam et al. 2004; Seefelt and Gillingham 2008). Given the similarities of percid-dominated fish communities and cormorant diets on both Leech and Oneida lakes, we were surprised our observed consumption rate was nearly double that reported for Oneida Lake ($0.456 \text{ kg}\cdot\text{bird}^{-1}\cdot\text{d}^{-1}$; Rudstam et al. 2004). We offer several reasons for this difference.

TABLE 8. Linear regression results of cormorant feeding effort (d/ha) effects on fishery and fish population statistics at Leech Lake, Minnesota, during 1998–2011. Statistical significance was set at $\alpha = 0.025$.

Independent variable	Dependent variable	Transformation	Direction	R^2	df	F -value	Intercept	Slope	P -value
Feeding days (d/ha)	Fishery metric, lagged 1 year								
	Pressure (h/ha)	Log _e	Negative	0.71	6	12.00	3.0480	-0.0282	0.0180
	HPUE of all species (kg/h)	Log _e	Negative	0.88	6	36.72	-1.6461	-0.0439	0.0018
	Walleye HPUE (fish/h)	Log _e	Negative	0.80	6	20.51	0.1170	-0.0037	0.0062
	Yellow Perch HPUE (fish/h)	Log _e	Negative	0.16	6	0.93	-1.4098	-0.0194	0.3795
Cumulative feeding days (d/ha), $y + (y-1) + (y-2)$	Gill-net CPUE								
	Cisco	Log ₁₀	Negative	0.12	13	1.64	0.5457	-0.0006	0.2241
	Walleye	Log ₁₀	Negative	0.37	13	6.91	0.9860	-0.0043	0.0221
	Yellow Perch	Log ₁₀	Negative	0.22	13	3.37	1.4031	-0.0031	0.0913
Cumulative feeding days (d/ha) experienced by the cohort, ages 0–2	Population statistic								
	Walleye mean TL at age 3	None	Positive	0.36	13	8.35	388.0244	0.9608	0.0136
	Yellow Perch mean TL at age 3	None	Positive	0.09	13	1.15	159.6548	0.1763	0.3038
	Walleye TL at 50% female maturity	None	Negative	0.46	13	10.27	4.0519	-0.0190	0.0076
	Walleye recruitment	None	Negative	0.47	12	9.63	1.7540	-0.0183	0.0100

Our observed overall mean (SE) mass of adult and subadult cormorants of 2.22 kg (0.01) was similar to those reported by Fowle (1997) and Rudstam et al. (2004), suggesting that cormorant weight was not driving these differences. Rudstam et al. (2004) assumed cormorants consumed 20% of their body mass per day, as this was similar to Dunn (1975b) and Glahn and Brugger (1995); conversely, we observed cormorants to consume approximately 34% of their body weight per day. Where other bioenergetics investigations have relied on an assimilation efficiency of 80.0% recommended by Ridgway (2010), we used an assimilation efficiency of 75.68% (Brugger 1993); this alone increased the average daily consumption rate by approximately 0.04 kg·bird⁻¹·d⁻¹. Taking differences in assimilation efficiency into account, we still observed fledged cormorants to consume approximately 32% of their body weight per day.

Our results corroborate other studies documenting considerable variability in cormorant diet (Neuman et al. 1997; Belyea et al. 1999; Johnson et al. 2006; Hobson 2009; DeBruyne et al. 2013), and this was largely influenced by recruitment variability of Ciscoes <254 mm TL and Walleyes <400 mm TL and potentially by a Cisco summer-kill event during 2006. Regarding predator–prey dynamics, Type III predators demonstrate increasing prey consumption rates as prey density increases until the predator becomes saturated by prey abundance and the rate of prey consumption has reached its maximum. Prior to the onset of predator saturation, consumption rates are believed to accelerate relative to prey density as the predator learns to recognize the more abundant prey item (Holling 1959). If cormorants are a Type III predator, this may explain the recruitment-driven vari-

ability in consumption rates of Ciscoes and Walleyes observed in this study and similar prey-switching behavior observed in other systems as fish communities change (DeBruyne et al. 2013). Specifically, if this behavior follows an initially large cohort until it outgrows the size range susceptible to cormorants, then cumulative predation sustained on a single cohort could lead to increased total mortality and eventual suppression of recruitment to older age-classes, particularly those recruiting to a fishery. Fielder (2010) documented increased growth and annual mortality rates of Yellow Perch cohorts as cormorant abundance increased in the Les Cheneaux Islands region of Lake Huron. Similarly, the density-dependent response of Walleye cohorts to differing levels of cormorant predation, specifically increasing growth and maturity rates, despite presumed decreases in angling pressure and harvest at this location suggests this condition may have existed.

It is possible that the 7-year duration of cormorant recolonization and expansion prior to the onset of management activities at Leech Lake was too brief of a time period to describe cause-and-effect relationships between cormorants and fish populations in the face of natural variability, particularly with the Yellow Perch population. The cormorant colonies described by Fielder (2010) and Rudstam et al. (2004) had been established longer than 10 years before management activities were implemented. While some population metrics, such as growth and maturity rates, showed indications of a population-level response to increased mortality at this location, these trends were not clear when examining abundance alone. Furthermore, fish population responses to cormorant control were complicated by other

management activities and natural perturbations. Creel surveys suggest that pressure and harvest both declined from 1998 to 2005 and should not have contributed to any potential increases in total mortality; however, no data are available to confirm this trend during 2000–2003 as these populations and presumably the fishery declined. The Cisco population, which experienced recruitment during 2005, suffered a summer kill in 2006 and this event may have negated a population-level response to reduced cormorant predation if predation on this species was a significant source of mortality. Varying stocking densities of Walleye fry during 2005–2011, as well as the implementation of a 454–660-mm protected slot limit and a bag limit reduction from six to four fish, has confounded interpretation of the Walleye population and fishery response to cormorant control. At a minimum, managers should consider discontinuation of Walleye stocking activities, which had not occurred during 1988–2004 and were conducted to a much lesser extent prior to 1988. This would facilitate a post hoc test to show that natural reproduction and juvenile survival are sufficient to maintain recruitment similar to historical levels. This test would support the theory that cormorant predation on juvenile fish, not the reproductive capacity of the population, was limiting recruitment to older age-classes; a different outcome would indicate other factors are influencing the population.

Seasonal and annual trends in the population dynamics of the fish community (Hebert et al. 2008; DeBruyne et al. 2013) should be considered when evaluating consumption. Failing to take this variability into account with an adequate sampling design will lead to under- or overestimation of average consumption. For example, the general model we used to estimate annual consumption during 1992–2011 underestimated total fish consumption relative to the stratified approach during 2005 and 2007 because Ciscos were overrepresented in the average diet. Therefore, general models can be appropriately applied when estimating annual consumption provided the source diet data are adequately distributed across seasons, years, and cormorant age-classes, thus representing the “average” condition of prey populations. However, higher levels of stratification should be considered when estimating consumption of a specific prey species because of the high variability in recruitment-driven species availability and subsequent contribution to the diet.

Resident population control models used to contrast culling alternatives did provide constructive guidance to managers. Relative to the potential maximum peak nests that could have been present in the absence of control, predictive population models indicated annual consumption could be reduced by 52–79%, and we observed reductions of up to 73%. However, it was not until intensive control measures ($DCCO_e$) were initiated in 2010 that total consumption approached the initial consumption target of 3.85 kg/ha. This was achieved via fewer adult birds arriving each year, a likely artifact of culling mortality and disturbance during previous years, as well as continued dispersal from the colony after more intensive levels of control effort had ceased. Predictive models consistently overestimated nestling production relative to the observed population during years

culling was applied. Some of this error is explained by the variability in observed fledge rates; however, it also suggests some nest abandonment may have been occurring. This was further evidenced by control rates applied at the log-linear and exponential rates resulting in observed population responses expressed as exponential and extended exponential curves, respectively. These trends infer a behavioral response to control measures and dispersal to new locations. Duerr et al. (2007) documented dispersal of a managed cormorant colony and suggested control measures that reduce dispersal away from the managed colony be considered to facilitate more effective management of the metapopulation. Similarly, it is no surprise that new colonies have established and expanded in northern Minnesota, some of which are less than 100 km from this study site (Wires et al. 2011). It has been hypothesized that some of these new colonies may be the direct result of control efforts on Leech Lake, and public pressure is mounting for cormorant management to begin at these locations. Thus, management agencies should evaluate both local and statewide cormorant management goals from a broader cost-benefit perspective when drafting local management plans. During 2005–2011, cormorant control activities at this location have averaged approximately US\$50,000–60,000 annually; this has been independent of diet work, modeling, and analysis. Subsequently, control methods that minimize disturbance to meet desired local management objectives, such as the use of high-powered air rifles equipped with silencers and egg oiling, should better mitigate concerns of dispersal to new locations and eventual expansion of control activities and management cost to the management entities.

From the perspective of a tourism-based fishery, the annual cost of cormorant control appears to have been a cost-effective investment. However, while frequently hailed as a cormorant management success story in the Glacial Lakes region, our investigation would have benefited from a better design for evaluating the responses of exploited fish populations to cormorant control. Specifically, more robust sex- and age-structure data for Ciscos and Yellow Perch would have facilitated a more detailed analysis of recruitment, growth, and maturity responses of these species to changes in cormorant predation pressure. The importance of these two species is highlighted by their frequency in cormorant diet and because no other management actions were directed at them. Creel surveys should have been conducted as the cormorant population increased to confirm that any changes in fish populations were not induced by increasing harvest. Additionally, changes in other fish management activities with the potential to benefit the populations under investigation, such as harvest restrictions or stocking, should not have been pursued concurrent with cormorant control. In this instance, sociopolitical drivers of other management actions, particularly stocking, have compromised our ability to interpret the effect of cormorant control on the Walleye population or to revise the current management target of 3.85 kg/ha of annual consumption based on Rudstam et al. (2004) to one more appropriate to this system. Consequently, continued control and investigation are recommended until this revision can be made.

The minimum required data for evaluating cormorant control activities include pre- and postcontrol estimates of nesting colony size, nonnesting cormorants also using the resource if they are deemed a significant component of the population, and fish consumption rates ($\text{kg}\cdot\text{bird}^{-1}\cdot\text{d}^{-1}$) by cormorants. The high variability in cormorant diets is strongly associated with fish population dynamics, which in turn have a significant influence on consumption rate estimation and interpretation of results. Furthermore, the effects of cormorants on fish populations can change as fish communities are altered by new aquatic species introductions, habitat modification, harvest, primary productivity changes, or other factors. Accurately describing cormorant diet composition and variability in the system in question is recommended but may not be feasible when considering control program implementation. When borrowing consumption estimates from other systems in lieu of an independent investigation, we recommend that the characteristics of the source aquatic community be carefully reviewed. Failing to select a representative surrogate could lead to poor estimation of cormorant consumption and misinterpretation of the range of potential effects, thus compounding decision-making processes and exacerbating the social complexities around a biological question. Where and when lethal control programs are implemented, we strongly encourage resource managers include funding to describe the diet of cormorants across multiple seasons and years and avoid concurrent management activities that could also influence the response of prey populations. The cumulative results of additional studies across varying aquatic ecosystem types and fish communities will enhance the overall understanding of cormorant interactions with fish populations and the fisheries they support. Future studies should consider describing cormorant consumption and total feeding effort as a function of system productivity, similar to Ryder's (1965, 1982) morphoedaphic index and harvest. In time, such efforts could serve to filter biological concerns from social anxiety and refine the role of cormorant management when managing recreational fisheries.

ACKNOWLEDGMENTS

We would like to thank many agencies, staff, and graduate students who assisted with various aspects of cormorant research and management on Leech Lake. This included the U.S. Department of Agriculture Wildlife Services Division, the U.S. Forest Service, the USFWS, the MN DNR, the University of Minnesota, and the Leech Lake Band of Ojibwe's Division of Resources Management. We would also like to thank members of the public for their assistance with cormorant research and management activities, specifically the Miles Lord family, Terry Daudt, and members of the Leech Lake Association. Finally, we would like to thank David Staples, Steve Windels, and anonymous reviewers for their constructive edits of this manuscript.

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EXHIBIT B



FISH PASSAGE CENTER

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MEMORANDUM

TO: Ed Bowles, ODFW

FROM: Michele DeHart

DATE: October 7, 2013

SUBJECT: Review Comments, 2013 Draft FCRPS Supplemental Biological Opinion

In response to your request the Fish Passage Center (FPC) staff have reviewed the hydro systems operations portion of the NOAA 2013 Draft FCRPS Supplemental Biological Opinion (herein referred to as Draft BIOP) and the three supporting documents: (1) "Federal Columbia River Power System Improvements and Operations under the Endangered Species Act — A Progress Report" by the Bonneville Power Administration based upon analyses by BioAnalysts Incorporated and Anchor QEA LLC, (2) "Limitations of Correlative Investigations of Identifying Causal Factors in Freshwater and Marine Survival of Columbia River Salmonids" by Skalski et al. (2013), and (3) "Review of *Assessing Freshwater and Marine Environmental Influences on Life-Stage-Specific Survival Rates of Snake River Spring–Summer Chinook Salmon and Steelhead*" by Manly (2012).

Although Skalski et al. (2013) and Manly (2012) are presented by NOAA as the primary foundation for elements of the Draft BIOP, or at least retaining the status quo, these documents were not available for public review until September 16, 2013, after half of the public review period for the Draft BIOP had passed, although previous requests for those documents had been submitted to NOAA

Our overall conclusion is that the hydro systems operations sections of the Draft BIOP reduces fish passage protections and does not incorporate new data, analyses, and knowledge that have been gained since the 2008 version of the Biological Opinion was completed. In this way the Draft BIOP provides less than the previous Biological Opinion in fish protection. The

Action Agencies and NOAA have contracted with consultants, Skalski et al. 2013, Manly 2012, BioAnalysts, Inc., and Anchor QED, 2013, for analyses intended to support their decision to discount and exclude new data and analyses from incorporation into the Draft BIOP, therefore maintaining, or in some cases reducing, the present status quo in fish protections in the Draft BIOP. Our review of these specific documents concludes that they do not provide a reasonable or technically sound basis for excluding new data and analyses from the Draft BIOP. These recent data and analyses clearly indicate that some of the fundamental components of the 2008 and Draft BIOP should be reconsidered, specifically the at-dam performance standards and spill for fish passage.

In the following we have organized our comments according to key issues regarding the Draft BIOP and our summary conclusions, followed by detailed discussion of each. We also provide specific comments on each of the above listed documents which NOAA has provided to support the Draft BIOP. We offer the following review comments for your consideration.

Spill for Fish Passage

- The Draft BIOP reduces spill for fish passage, reducing the time period that spill is provided by ending spill prior to August 31, and by starting lower summer spill levels at an earlier date.
- The Draft BIOP does not provide any scientific biological rationale for providing lower spill, below gas cap levels, for fall Chinook summer migrants.

Performance Standards Evaluation and Accomplishment

- The Draft BIOP does not address significant serious technical concerns that have been raised over the past several years regarding the concept and approach of performance standards. NOAA has failed to address or consider recent data and analyses that raise serious issues regarding the validity of the performance standard concept and approach, specifically that route of dam passage affects survival at later life stages and adult return rates.
- The present performance standard testing is likely generating estimates that are biased high and do not represent the run-at-large.
- Recent data and analysis indicate that freshwater passage experience affects later life stages and adult returns, which are not considered in performance standard implementation in this Draft BIOP therefore underestimating the impact of dam passage. Recent data indicate that a smolt-to-adult return rate would provide a more appropriate performance standard.
- NOAA does not offer any rationale for lower performance survival standards for fall Chinook compared to standards for spring/summer Chinook and steelhead.

Smolt Transportation

- The Draft BIOP increases the proportion of smolts transported by implementing an earlier date for the start of transportation.
- NOAA does not provide a biological scientific rationale for this action, but recognizes that this will provide no benefit to spring Chinook, which migrate earlier in the spring and will receive the majority of the impact from this action.
- Recent data and analyses indicate that overall transport SARs have improved with later transport dates and transportation of later migrating fish. Recent data and analyses indicate that powerhouse bypass passage should be avoided, indicating that increased spill at collector projects such as Lower Granite would result in higher SARs, rather than transporting earlier and increasing transportation.
- Recent data and analyses have shown that increasing transportation will increase straying and increase the negative impact of straying on other listed populations of salmon and steelhead.

Benefits of Spill for Fish Passage/Experimental Spill Management

- NOAA's rejection of consideration of Experimental Spill Management on the basis of the spring Chinook returns from the 2011 outmigration year is unfounded.
- NOAA fails to consider the high fall Chinook return from the 2011 outmigration year, which also experienced high spill and flow.
- NOAA fails to recognize, address or propose mitigation measures for hydrosystem operations under the present FCRPS configuration that took place in the 2011 outmigration year that were adverse for spring migrants.

The Draft BIOP Excludes Recent Data and Analyses and Maintains the Status Quo

- NOAA excludes recent data and analyses from consideration in the Draft BIOP on the basis of three documents: Skalski et al. 2013, Manly 2012, and BioAnalysts Incorporated and Anchor QEA 2013. These documents do not provide valid technical justification or rationale for excluding consideration of recent scientific findings from development and modification of RPAs in the Draft BIOP.
- Technical and analytical issues and methodology contained in Skalski et al. 2013, indicate that conclusions are not supportable and do not provide a valid rationale for rejecting recent data and analyses.
- Specific comments on these documents are provided in subsequent discussion sections of this review.

Detailed Discussions

Spill for Fish Passage

Reductions in Spill under RPA 29 in the Draft BIOP

Through the Draft 2014–2018 Implementation Plan (herein referred to as the Draft IP), the Draft BIOP proposes to change the start date for summer spill to begin earlier than presently implemented. At many projects summer spill volumes are less than those in the spring.

- The action agencies do not provide any scientific basis for the provision of lower spill for fish passage levels for summer migrating fall Chinook. At some projects spill could be increased to the gas cap for fall Chinook summer migrants to increase juvenile survival.
- Earlier summer spill will result in reductions in June spill volumes at Lower Granite (LGR), Lower Monumental (LMN), and Bonneville (BON) in medium and low flow years. June spill reductions at these sites are less likely in high flow years.
- Earlier summer spill will result in additional spill in June at McNary (MCN) because summer spill levels are higher than spring spill levels at MCN.
- Earlier summer spill will result in no change in June spill at LGS, Ice Harbor (IHR), John Day (JDA), and The Dalles (TDA) because spill levels do not change between seasons.

The Draft IP proposes to terminate spill at Snake River sites in August, based on subyearling Chinook collection counts.

- This change reduces spill at these sites. On average, over the last 9 years, summer spill at LGR, LGS, LMN, and IHR would have been terminated on August 7th, 16th, 18th, and 20th, respectively.
- Since flow conditions in August vary little among years and are usually below hydraulic capacity, the amount of reduced spill in August is not likely to be impacted by the flow conditions.

Spill could potentially decrease in spring and summer at IHR and JDA and in summer at BON if performance standards testing shows performance standards are met at the lower spill levels. Performance standard testing and implementation should be reconsidered based upon significant technical concerns and new scientific data.

Despite repeated objections from most of the salmon managers over the years, the Draft IP proposes to continue bulk spill at LMN during the spring, which effectively limits spill at LMN due to the fact that bulk spill generates high levels of total dissolved gas (TDG).

The Draft BIOP references RPA 29 of the Draft IP for proposed spring and summer spill operations at FCRPS projects. The Draft IP reduces spill from the current Court Order in at least two ways. In addition, there is language in the Draft IP that has the potential to further reduce spill at some sites from what is being provided under the current Court Order. Below is a

detailed explanation of these known reductions, as well as the language that allows for potential further reductions.

Known Reductions in Spill

Earlier Start to Summer Spill

At many FCRPS sites, summer spill volumes are lower than spring spill volumes. The technical, biological basis for providing less spill for summer migrating fall Chinook is not explained by NOAA fisheries and should be evaluated. RPA #29 of the Draft IP proposes to implement summer spill at an earlier time than the current Court Order. Under the current Court Order, summer spill begins on June 21st at Snake River projects and July 1st at Lower Columbia sites. There have been some exceptions to these start dates to accommodate research/performance standard studies over the years. However, in the absence of performance standards testing, June 21st and July 1st would be the summer spill start dates under the Court Order.

According to Table 2 of the Draft IP, the initiation of summer spill at Snake River sites will be based on collections of subyearling Chinook, and may occur as early as June 4th. Using the last ten years (2004–2013) of Smolt Monitoring Program (SMP) data at LGR, LGS, and LMN dams we estimated when summer spill would have begun, had the Draft BIOP been in place. Over the last 10 years, the average summer spill start dates would have been June 4th at LGR, June 6th at LGS, and June 7th at LMN (Table 1). Furthermore, all three Snake River sites had at least one year in the last ten that had a summer spill initiation date of June 4th (Table 1). Under the Draft IP summer spill at Lower Columbia sites will begin on June 16th, rather than July 1st under the current Court Order.

NOAA indicates that these earlier summer spill start dates are at least partly due to earlier run timing of subyearling fall Chinook. While it is true that subyearling Chinook timing has been earlier over the years, run timing of subyearling Chinook at SMP sites is largely influenced by the timing of hatchery releases. On average, approximately 80% (range 65%–100%) of hatchery subyearling Chinook released above LGR were released prior to the estimated summer spill initiation date over the last 10 years (2004–2013) (Table 1).

Table 1. Estimated summer spill initiation dates at Snake River sites, based on the 50% subyearling Chinook collection criteria outlined in RPA 29 of the Draft 2014–2018 Implementation Plan. Hatchery release data are only for subyearling Chinook releases above LGR.

Migration Year	LGR	LGS	LMN	Hatchery Release Total Above LGR	Percent Released Prior to LGR Summer Spill Initiation Date
2004	9-Jun	12-Jun	11-Jun	1,517,006	100%
2005	4-Jun	4-Jun	6-Jun	3,964,117	99%
2006	4-Jun	4-Jun	4-Jun	3,615,216	65%
2007	5-Jun	9-Jun	10-Jun	2,251,450	77%
2008	5-Jun	7-Jun	9-Jun	4,117,657	80%
2009	4-Jun	4-Jun	4-Jun	5,108,127	78%
2010	4-Jun	12-Jun	11-Jun	4,613,429	77%
2011	4-Jun	4-Jun	4-Jun	4,919,891	73%
2012	4-Jun	6-Jun	7-Jun	4,889,575	78%
2013	4-Jun	4-Jun	6-Jun	4,456,543	75%
Avg. ('04-'13)	4-Jun	6-Jun	7-Jun		80%

Earlier End Date for Summer Spill at Snake River Sites

Under the current Court Order, summer spill at Snake River sites occurs through August 31st. However, the Draft IP indicates that summer spill may be terminated as early as August 1st at LGR, August 4th at LGS, August 7th at LMN, and August 9th at IHR. According to Table 2 of RPA 29, the actual end date for summer spill will be based on subyearling Chinook collection counts in late July and into August. Using the criteria outlined in Table 2, we reviewed the last nine years (2005–2013) of Smolt Monitoring Program data at LGR, LGS, and LMN dams to estimate when summer spill would have ended had the Draft IP been in place (note: 2004 was not included since it was before the Court Order and there was no summer spill).

Over the last 9 years, the average summer spill end dates were August 7th at LGR, August 16th at LGS, August 18th at LMN, and August 20th at IHR (Table 2). In addition, over the last nine years, spill would have been terminated prior to August 31st in all 9 years at LGR, in 8 of the 9 years at LGS, and in 7 of the 9 years at LMN and IHR. Finally, August spill would have been terminated completely at LGR in 3 of the last 9 years (2005, 2006, and 2007).

Table 2. Estimated end dates of summer spill at Snake River projects based on criteria outlined in RPA 29 of the Draft 2014–2018 Implementation Plan over the last 9 years (2005–2013).

Migration Year	LGR	LGS	LMN	IHR
2005	1-Aug	5-Aug	8-Aug	10-Aug
2006	1-Aug	4-Aug	7-Aug	9-Aug
2007	1-Aug	6-Aug	9-Aug	11-Aug
2008	31-Aug	29-Aug	1-Sep	1-Sep
2009	3-Aug	12-Aug	15-Aug	17-Aug
2010	11-Aug	24-Aug	27-Aug	29-Aug
2011	6-Aug	15-Aug	18-Aug	20-Aug
2012	7-Aug	18-Aug	21-Aug	23-Aug
2013	10-Aug	1-Sep	1-Sep	1-Sep
Average ('05-'13)	7-Aug	16-Aug	18-Aug	20-Aug

The Draft IP also specifies a criterion for resuming spill at the Snake River sites if subyearling Chinook collections exceed 500 for two consecutive days. However, it is difficult to determine if this criterion would have been met over the last 9 years because we do not know what collections would have been, had spill not been provided.

Estimated Impact of Earlier Summer Spill Start Dates and Earlier Termination of Summer Spill

To investigate the impact of these date changes, the Court Order and Draft IP schedules were modeled using hourly flow data for 3 flow years: a high flow year (2011), a medium flow year (2009), and a low flow year (2013). Instantaneous spill volumes under both scenarios were based on Table 2 of the Draft IP (RPA 29). For the Court Order schedule, we assumed the spring spill dates of April 3 to June 20 and summer spill dates of June 21 to August 31 at Snake River sites. For Lower Columbia sites, the Court Order schedule assumed spring spill dates of April 10 to June 30 and summer spill dates of July 1 to August 31. For the Draft IP schedule, we assumed the estimated dates for the initiation of summer spill and the termination of summer spill under the Draft IP (Tables 1 and 2) for Snake River sites. The Draft IP schedule for the Lower Columbia sites included spring spill dates of April 10 to June 15 and summer spill dates of June 16 to August 31. In addition, when spill volumes alternated at IHR, JDA, and BON, we assumed the spill schedules used in 2013. Finally, under both scenarios excess spill due to lack of market was not included, as this is not predictable from year to year. However, excess spill due to hydraulic capacity was included in both scenarios.

Results from this modeling exercise can be found in Tables 3 and 4. Spill volumes for April, May, or July are not provided in these tables because these 3 months are not affected by the proposed changes in the Draft BIOP spill schedule. Furthermore, since spring and summer spill percentages are the same at JDA and TDA, there were no differences in spill volumes between the two different schedules at these sites (Table 3). Spring and summer spill operations are also the same at LGS and IHR. This is why there were no differences in spill volumes at these two sites for the month of June (Table 3). However, since the Draft IP proposes to terminate summer

spill in August, based on subyearling Chinook collections, there were reductions in the August spill volumes at both LGS and IHR in 2 of the 3 years modeled (Table 4).

Table 3. Estimated June spill volumes (MAF) at FCRPS sites under Court Order schedule versus the Draft 2014–2018 Implementation Plan schedule.

Project	High Flow Year (2011)		Medium Flow Year (2009)		Low Flow Year (2013)	
	Court Order	Draft IP	Court Order	Draft IP	Court Order	Draft IP
LGR	3.85	3.85	1.60	1.56	1.15	1.08
LGS	3.37	3.37	1.92	1.92	0.98	0.98
LMN	3.26	3.25	1.50	1.32	1.29	1.10
IHR	5.61	5.61	3.16	3.16	1.74	1.74
MCN	17.18	<i>17.18[†]</i>	7.43	8.17	5.96	6.69
JDA	10.25	10.25	5.81	5.81	5.15	5.15
TDA	11.44	11.44	6.43	6.43	5.44	5.44
BON	13.69	13.69	6.01	5.89	5.95	5.82

Bold = spill reduction under Draft IP schedule, ***Bold Italics*** = additional spill under Draft IP schedule

[†] There was a slight increase in the June spill volume under the Draft IP schedule, although not shown here due to rounding.

Table 4. Estimated August spill volume (MAF) at Snake River sites under Court Order schedule versus Draft 2014–2018 Implementation Plan schedule.

Project	High Flow Year (2011)		Medium Flow Year (2009)		Low Flow Year (2013)	
	Court Order	Draft IP	Court Order	Draft IP	Court Order	Draft IP
LGR	1.11	0.19	1.06	0.08	0.61	0.23
LGS	0.75	0.37	0.58	0.23	0.42	0.42
LMN	1.04	0.58	0.91	0.45	0.54	0.54
IHR	1.92	1.28	1.32	0.72	0.79	0.79

Bold = spill reduction under Draft IP schedule

The summer spill percentage volume at MCN is actually higher than the spring spill percentage. Therefore, the earlier start date for summer spill under the Draft IP results in higher June spill volumes, when compared to the Court Order schedule (Table 3). August spill volumes at MCN were not different between the two scenarios because both schedules call for 50% spill through August 31st.

For the remaining sites (LGR, LMN, and BON), the Draft IP schedule resulted in reductions in June spill volumes in medium and low flow years (Table 3). However, there were no reductions in June spill volumes in the high flow year (Table 3). This is because flows tend to peak in June and, in a high flow year, it is possible that flows will be above hydraulic capacity for the entire month of June, making it virtually impossible to implement the lower summer spill volumes until flows decrease. This was the case in the high flow year modeled (2011), as flows remained above hydraulic capacity through all of June and into July at these three sites. This general pattern may not be true for all high flow years, as it depends on how long flows exceed hydraulic capacity.

The changes in August spill dates pertain only to the Snake River projects. August spill volumes at the Snake River sites were not affected by the magnitude of the flow year (i.e., high, medium,

or low), as August flows are generally below hydraulic capacity by this time. However, as is illustrated in Table 4, reductions in August spill volumes were dependent on the collection counts of subyearling Chinook. This is why reductions in August spill volumes occurred in all 3 of the modeled years at LGR but only 2 of the 3 years at LGS, LMN, and IHR. Due to high collection counts at LGS throughout August of 2013, spill at LGS, LMN, and IHR would have run through August 31st in this year. However, it is important to note that spill at LGR, LGS, LMN, and IHR would have been terminated early in 78%–100% of the last nine years, depending on the site (Table 2). Therefore, the Draft IP schedule will likely result in reductions in August spill volumes, regardless of the flow year.

Potential Reductions in Spill

Selection of a Single Spill Operation at IHR, JDA, and/or BON

According to RPA 29 of the Draft IP, "...changes in spill or the selection of a single spill operation at a project where two operations are currently being implemented may occur either for testing purposes or after performance standard testing confirms that the performance standards are being achieved." This language would apply to spring and summer spill at IHR and JDA and summer spill at BON and could lead to reduced spill at these projects if the lower of the two spill operations is implemented.

Other Comments

Bulk Spill Pattern at LMN

According to RPA 29 of the Draft IP, spring spill at LMN will be to the Gas Cap, under a bulk spill pattern. While this spill pattern is a continuation of the current Court Order, the fish managers have routinely contended that the use of a bulk spill pattern above flows of 60 Kcfs is not ideal. On June 26, 2012, many members of the Fish Passage Advisory Committee issued a Joint Technical Staff Memorandum (http://www.fpc.org/documents/joint_technical/79-12.pdf) that outlined their concerns with the bulk pattern at LMN. Among the concerns with the bulk spill pattern is the increased TDG production and consequent spill curtailments that occur, just as large numbers of smolts arrive at the project. The Salmon Managers have made repeated requests to change this spill pattern (SOR 2011-02 and FPP Change Form 12LMN007) to one that better meets the 2008 BIOP objectives of RPAs 15 and 29, to provide spill to improve juvenile fish passage while avoiding high levels of TDG.

In past years, the Fish Operations Plans have included a statement, as justification for the bulk pattern, that, "Based on a previous year's study results, dam survival is higher under the 'bulk' spill pattern compared to a 'uniform' pattern." The above mentioned Joint Technical Staff Memo pointed out that this statement is statistically inaccurate and misleading. In fact, the authors of the 2009 study at LMN (Hockersmith et al., 2010) clearly state that the results of comparing bulk and uniform patterns indicated no significant difference in direct concrete survival. The point estimates for concrete survival for yearling Chinook under the bulk and uniform patterns were 0.975 and 0.973, respectively (Hockersmith et al., 2010). Further review of Hockersmith et al. (2010) revealed that the point estimate for the bulk pattern was only higher than that for the uniform pattern because the turbine survival estimate under the bulk pattern was significantly greater than 100%, which is clearly an overestimation. In addition, sole consideration of concrete survivals ignored the fact that the uniform pattern passed more fish

over surface routes with less delay (Hockersmith et al., 2010), while also leading to less TDG production.

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Performance Standards

- Performance tests have utilized radio and acoustic tags, which cannot represent the run-at-large. Smolts are rejected from studies due to size and condition, and therefore represent survival only for the healthiest smolts in the run. Those already affected by disease or injury are those mostly likely to have reduced survival due to dam passage, and their exclusion inflates survival estimates. Recorded rejection rates have ranged from 3.2% to 16.4% of collected fish. Rejection rates are unknown for 2009 studies, which were not conducted for performance standards testing but have been included as such, so their representativeness of the run-at-large cannot be estimated.
- Smolts included in the dam-passage treatment group are released at multiple locations upstream, and some pass through several projects before being included in the test group. This process may eliminate from the sample weaker fish more susceptible to mortality due to tag burden; so only tagged fish most likely to survive dam passage are included in the test group. The inclusion of multiple control groups for each performance test raises concerns that dam passage survival estimates may be artificially inflated. This inflation can be caused by random effects or the unequal mortality between groups from factors such as predation in the tailrace.
- The majority of performance standards tests conducted to date occurred during 2011 and 2012. Both of these years were above-average water flow years. Although the required standards may have been met, it does not address the issue that survival may not be as high when flow levels are lower.
- During periods of high flow, spill levels were not consistent with planned operations during much of the testing in 2011 and 2012. Although required standards may have been met, they do not reflect actual survival estimates that would be expected when spill levels conform to the Fish Operation Plan. Additionally, operations that may affect juvenile survival, such as “open geometry” turbine operations at Bonneville, were not incorporated into performance testing.
- Radio tag studies from 2009 at LMN and LGS should not be included in performance testing requirements. These studies do not report rejection rates, do not include appropriate control groups, have significant tag burdens beyond those of the current JSATS tags, and have utilized different release locations from 2012 studies and therefore are not comparable.

- There is no biological reason given why subyearling Chinook have a dam passage standard of 93% survival, while yearling Chinook and steelhead have a dam passage standard of 96% survival.
- A public database for raw and processed data, when available, with detailed descriptions of post-hoc inclusions and exclusions of data, would make outside evaluation more straightforward. Despite numerous requests and commitments by the Army Corps of Engineers to provide these data, a database of this type has not been made available.
- Dam passage survival, as measured by performance tests, is currently the only required metric of juvenile passage through the hydrosystem. For yearling Chinook and steelhead passing at LGR and all downstream dams, the overall probability of survival as estimated by performance testing must be at least 72% (0.96^8). For subyearling Chinook, this standard is 56% (0.93^8). However, these standards do not include other juvenile metrics which are known to impact adult returns, such as passage route. Smolts that pass through bypass systems are known to return at lower rates than those that pass through spill. However, this has not been incorporated into the Draft BIOP. Performance standards should be a single component of many in determining hydrosystem operations.

Study Fish May Not Represent Run at Large

Juvenile Salmon Acoustic Telemetry System (JSATS) tagging protocols require rejection of fish based on multiple criteria, including size and condition. These rejection rates have ranged from 3.2% to 16%, depending on the year and species. These rejection rates mean that only the healthiest portions of the population are used in performance testing, and survival estimates are inflated to reflect this bias. Actual survival rates are likely much lower than those estimated by performance testing.

Selection on Dam Passage Group May Inflate Survival Estimates

In the Virtual-Paired Release design, fish are released upstream of the dam so they achieve a distribution through passage routes that reflects the run at large. Fish that die between tagging and the forebay of the dam are not included in the study. However, this means that fish that have lower survival through the reaches will not be included in the study. Mortality between tagging and detection was as high as 12.5% in yearling Chinook in 2012. Similar to the effects of tagging only healthy fish, this means that only the healthiest of tagged fish are included in the dam survival estimates.

Performance testing utilizes fish collected at the juvenile bypass unit at JDA (Lower Columbia testing) or LMN (Snake River testing). These fish have successfully survived a minimum of one bypass, as fish that die in the bypass are not included in the study. Therefore, these fish may not represent the survival probabilities of previously undetected fish not included by the study design. Survival estimates greater than 100% for juvenile bypass systems may be due to this aspect of the study design.

Inflated Survival Estimates Due to Experimental Design

The virtual/paired-release design used in most of the performance tests utilizes two control groups: one released in the tailrace of the dam (R_2) and one released further downstream (R_3). The R_3 group is intended to account for any handling mortality experienced by the R_2 group, which could inflate survival estimates.

Under this experimental design, however, upward biasing of survival estimates could be caused by high mortality in the R_2 group. It is unlikely that tagged fish in both stretches of river encounter the same environmental conditions, especially since predation rates at many projects are higher in the forebay and tailrace than mid-reservoir (Petersen 1994, Ward et al. 1995). If survival in the R_2 group is lower than survival in the R_3 group, the ratio of survivals (S_2/S_3) will be biased low and will artificially increase estimates of dam survival. Please see Beeman et al. (2011) and FPC Memos (March 24, 2011; February 15, 2012; March 23, 2012; see http://www.fpc.org/documents/FPC_memos.html) for detailed descriptions regarding upward biases inherent in this study design.

A further cause of differential mortality may be the fact that fish in the R_2 and R_3 groups will not have the vertical or horizontal distribution of fish that are naturally migrating through the hydrosystem. In contrast, fish that pass through the dam are not included in the study group until they have migrated through and survived some distance from their initial release point. At TDA, release of the R_2 group occurs near islands downriver of the dam. At the Studies Review Work Group (SRWG) meeting on February 6, 2012, concern was expressed that this release occurs in an equal distribution across the river, rather than attempting to mimic natural migration patterns. Therefore, it is unlikely that (1) mortality will be equal between release groups and (2) that these releases represent mortality of the run-at-large.

Survival estimates generated with this multiple-release design may further increase dam survival estimates due to random sampling effects, in some cases moving survival estimates upward enough to meet performance standards when they would not have with only one control group. If there is limited handling and transportation mortality, the use of the R_3 group will introduce additional variation to the study. Beeman et al. (2011) concluded that this result is “contrary to the goal of adjusting a paired-release estimate downward to account for handling mortality.”

An example of inflation due to experimental design is the performance testing results from MCN in 2012. The single-release survival estimates were 0.9136 (steelhead), 0.9171 (yearling Chinook), and 0.9149 (subyearling Chinook). None of these survival estimates met the performance testing criteria. After corrections with R_2 and R_3 , these survival estimates were inflated to 0.9908 (steelhead), 0.9616 (yearling Chinook), and 0.9747 (subyearling Chinook). In this case, the experimental design of the performance test has clearly artificially inflated dam survival estimates. Another example of this inflation is reflected in survival estimates greater than 100%. Survivals of greater than 100% were reported for 3 route-specific estimates in 2011 and 5 estimates in 2012.

High Flows in 2011 and 2012 Limit Applicability of Results

In both 2011 and 2012, spill levels during performance testing were much higher than those outlined in the Fish Operation Plan. Although survival estimates may meet the minimum requirements of performance testing, they reflect survivals only during periods of high flow and high spill, and have limited applicability to normal or low-flow water years. This issue has previously been outlined in an FPC Memo dated February 15, 2012 (see http://www.fpc.org/documents/FPC_memos.html).

Spill and Operations During Testing Have Not Been Consistent

For much of 2011 and 2012, spill levels did not conform to the Fish Operations Plan due to high flows. Although performance tests were met when spill levels were much higher than planned, that does not reflect the survival estimates that would be obtained under planned operations.

In addition, many operations are not included in survival estimates although they may affect juvenile survival. One example is the use of “Open Geometry” turbine operations at Bonneville Powerhouse 1. This operation is assumed to have equal or greater survival than other operations, but no actual tests of juvenile survival have been conducted to test this operation.

Studies Not Conducted As Performance Tests Are Not Comparable and Therefore Do Not Meet Standards

Radio-tag studies from 2009 should not be included as performance tests. These studies were not conducted with the intent to measure performance testing, and differ significantly from later tests. Rejection rates are not available from these studies, so it is impossible to assess how representative they are of the run-at-large. Given that the smaller JSATS tags used in 2010 required rejection rates of 12.6%, it can be expected that rejection rates in 2009 were higher.

The study design for 2009 radio-tag studies were conducted with a single release group, a design that has been since abandoned in favor of the Virtual-Paired Release design to avoid excessive inflation of survival estimates. Although the Virtual-Paired Release design also has the potential to inflate survival estimates, the adjusted survival is calculated using different methods than tests in 2009. Consequently, adjusted survival estimates from 2009 are not comparable to adjusted estimates from 2010–2013.

The significance of where control groups are released has been a topic of discussion in numerous SRWG meetings. The control group must be released in a distribution that is representative of the distribution treatment fish will assume upon passage through the dam. If not, the survival estimates of the control group used to adjust the dam survival are being misused. The release points in 2009 studies are not the same points used for the 2012 studies, and there is no experimental or biological justification for this difference. This may have a significant impact on control group survival rates, and therefore on the adjusted survival rates from 2009. Please see the FPC Memo from March 19, 2013 (http://www.fpc.org/documents/FPC_memos.html) for more detail on this topic.

No Biological Reason for Lower Survival Standard For Fall Chinook

There is no biological reason for fall Chinook to have lower survival standards (93%) than yearling Chinook and steelhead (96%). This reduced standard decreases the probability of strong adult returns, and NOAA provides no rationale for this reduction.

JSATS Data Should Be in a Public Database

Currently, the data collected during performance testing, both raw and filtered, is available only through request to the Army Corps of Engineers. However, this method is time consuming and can lead to confusion regarding analyses (see FPC Memos July 29, 2010; February 16, 2011; March 24, 2011; June 21, 2011 posted at http://www.fpc.org/documents/FPC_memos.html). A publicly accessible database, such as that used for PIT-tag data, would minimize these types of discussions and could potentially make results available sooner than the current speed of receiving final reports.

Management Decisions Should Not Be Based on Single-Dam Performance Standards

Past FPC memos have reviewed performance standards testing throughout the hydrosystem (June 24, 2009; July 29, 2010; March 24, 2011; February 15, 2012; March 16, 2012; March 23, 2012; January 4, 2013; February 11, 2013; March 22, 2013; see http://www.fpc.org/documents/FPC_memos.html). Repeatedly, these memos have raised concerns regarding the usage of these studies for project management decisions. Management decisions should reflect the entire life cycle of the fish, rather than survival at projects considered in isolation. All available data should be utilized, rather than ignored in favor of simplistic performance standards that do not reflect the current understanding of salmonid survival factors.

The long-term effects of passage routes for juvenile fish have been well documented in recent years. Fish that survive juvenile bypass systems or powerhouse passage are less likely to survive the first ocean year, and less likely to return as adults, than those that pass undetected through the hydrosystem (Haeseker et al. 2012; Petrosky and Schaller 2010; Tuomikoski et al. 2010; FPC Memos October 6, 2010; January 19, 2011; July 14, 2011; see http://www.fpc.org/documents/FPC_memos.html). These effects of project operations on these metrics are not included in the current performance testing requirements and therefore underestimate the full effects of dam passage.

Acoustic tag studies provide only short-term survivals for specific projects, and current performance standards do not include important metrics like forebay residence time, travel time, or latent mortality. Performance testing cannot fully inform policy makers about methods for improving adult returns. FPC recommends a decision-making framework for the Columbia Basin that will incorporate the strengths and limitations of each data type as part of a straightforward guide to the results of project operations.

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Smolt Transportation**Impacts of Earlier Start of Transportation**

- The Draft BIOP proposes to change the start date for juvenile transportation at LGR to a fixed date of April 21st, which is earlier than what has been implemented under the Court Order since 2007.
- Justification provided for the earlier transport start date focuses on maintaining a 50/50 split in transported and in-river migrants, per the recommendation for a spread-the-risk strategy by the ISAB in 2010. However, the interpretation of the ISAB's recommendation is incorrect. The ISAB never included a specific transport to in-river migration ratio in their recommendations in 2010. They simply stated that transportation should occur with spill, as opposed to turning spill off while maximizing transportation.
- The Draft BIOP acknowledges that an earlier start date for transportation has no benefit for Chinook, yet still proposes an earlier start date.
- Analyses of PIT-tags found that moving the transportation start date to April 21st resulted in an increase in the proportion of fish transported for all groups of hatchery and wild yearling Chinook and steelhead, with the largest impact on wild yearling Chinook. Historically, wild yearling Chinook have shown the least benefit from transportation, particularly in years when transportation began in early April.

- Snake River adults that out-migrated in-river have a higher survival from BON to LGR vs. adults that were transported. The in-river group represents only a portion of the run at large. However, the Draft BIOP uses only adults returning from the in-river juvenile migration group (the known higher survival rate to Lower Granite Dam) to compare to the 2008 Adult Performance Standards.
- Increasing the proportion of transported Snake River fish will lower the overall adult conversion rates which, in some cases, are already below the 2008 BIOP Adult Performance Standards.
- Increasing the proportion of transported fish will increase the population of Snake River hatchery adults that stray above BON. On average, the returning adult population of Snake River hatchery steelhead is more than ten times the combined naturally spawning steelhead population for the Deschutes and John Day river basins, and therefore small increases in straying can potentially have large impacts on downriver populations.. NOAA has identified out-of-DPS (distinct population segment) hatchery strays as a limiting factor to the recovery of the Deschutes and John Day River steelhead populations (NMFS 2009; Appendix A).

Impacts of Earlier Start of Transportation

Background and Justification to Earlier Start of Transportation

The Draft BIOP cites RPA 30 of the Draft IP which specifies that juvenile transport will begin at LGR on April 21st, with transport from LGS and LMN beginning 4 and 7 days after LGR, respectively. Since 2007, the Fish Operations Plan has specified a transportation start date beginning no earlier than April 21st but no later than May 1st at LGR. In each of these years, the actual start date has been determined through coordination with the Technical Management Team. Since 2007, transportation at LGR has generally begun on May 1st, with only one exception (in 2010 when transportation from LGR began on April 25th). Transportation in 2006 began on April 21st and prior to 2006, transportation at LGR generally began in early April.

Section 3.3.3.4 of the Draft BIOP provides background and justification for this change from the current Court Order. One justification provided for the earlier fixed transportation start date is the ISAB's recommendation in 2010 to continue a spread-the-risk strategy. However, the Draft BIOP misinterprets this spread the risk recommendation to mean the maintenance of a specific 50/50 split between transport and in-river migration. The ISAB was convened in February of 2010 to determine whether the cessation of spill from May 7–21, as outlined in the 2008 BIOP, was warranted, given that flows were predicted to be low in 2010. Data from out-migration years 2005 (no spring spill) versus 2007 (spring spill provided) were presented as a justification for providing spill in May, even in a low flow year. In their 2010 report, the ISAB concluded: "...using combinations of transport and in-river migration with spill spreads the risk across species, stocks, and the ecosystem, while offering an approach that can shed light on uncertainties in the longer-term dataset" (ISAB 2010). This statement clearly indicates what the ISAB meant by a spread-the-risk strategy, one that involves a combination of spill and in-river migration with spill. Nowhere in the ISAB's conclusions is there any mention of a specific transport to in-river migration ratio that is needed to meet a spread-the-risk strategy.

A second justification provided for the earlier fixed transportation start date is the maximization of transport/in-river ratios (TIRs). As an example, the Draft BIOP points out that 2006 is the only year among recent years where the TIRs for steelhead and Chinook were below 1.0, which indicates no benefit of transport (transportation in 2006 began on April 21st). The Draft BIOP goes further to highlight that there is a documented seasonal benefit from transport for Chinook, where no benefit is seen prior to May 1st. Given that the data presented by the Draft BIOP seem to support a May 1st start of transportation, there is no biological basis for the April 21st start date that is proposed. In fact, an April 21st start date is contradictory to the point that the Draft BIOP makes regarding the earlier start date in 2006. It appears that the only reason the Draft BIOP proposes an earlier start date is to better meet the target of a 50/50 split between transport and in-river migration. However, as stated above, the desire for a 50/50 split is a misinterpretation of the ISAB's recommendation for a spread-the-risk strategy.

On July 1, 2013, the FPC issued a memo in response to a data request to estimate the impact of moving the start date of transportation to April 21st, over the last five years (2008–2012). These analyses found that moving the transportation start date to April 21st resulted in an increase in the proportion of fish transported for all groups of hatchery and wild yearling Chinook and steelhead, with the largest impact on wild yearling Chinook. Historically, wild yearling Chinook have shown the least benefit from transportation, particularly in years when transportation began in early April (Tuomikoski et al. 2013). Finally, a transportation start date of April 21st resulted in an estimated proportion destined for transport of greater than 50% in 3 of 5 years for hatchery yearling Chinook, 4 of 5 years for wild Chinook, 3 of 5 years for hatchery steelhead, and 4 of 5 years for wild steelhead.

Impacts on Adult Conversion Rates and Straying Effects

The Draft BIOP notes that estimates of adult survival are above what was expected for Snake River fall Chinook, and Upper Columbia spring Chinook and steelhead. In addition it notes that although adult survival for Snake River Chinook, steelhead, and sockeye are below expectations, this is not considered an RPA implementation deficiency. The Draft BIOP goes on to state that there is no obvious explanation for low adult survival for these Snake River stocks and that a variety of factors could be affecting adult passage including the river environment, structural modifications, errors in the harvest or stray rate estimates (which as presented in the BIOP are added back in to adult survival thereby removing their effects), run timing, or concurrent effects of several factors at once.

Many studies have concluded that the transportation of out-migrating juvenile salmon and steelhead negatively affects the adult's return migration (Quinn et al. 1989; Johnson et al. 1990; Solazzi et al. 1991; Mundy et al. 1994; Chapman et al. 1997; Keefer et al. 2008; Tuomikoski et al. 2012). This phenomenon is one component of delayed mortality due to transportation at FCRPS collector dams and has been measured with both radio tags and PIT tags relatively recently (Keefer et al. 2008; Tuomikoski et al. 2012). The decreased survival for adults with a transport history can also be seen by comparing the adult survival of transported and in-river out-migrants as estimated with PIT tags in the Supplemental Comprehensive Analysis (NMFS 2008 SCA, Adult Survival Estimates Appendix). Curiously, the Draft BIOP uses only adult survival for in-river out-migrants to evaluate the RPA which does not represent adult survival of the run as a whole or include detrimental effects of transportation on Snake River stocks (Figure 3.3-1 in

the Draft BIOP). Given all the available evidence, increasing transportation for Snake River stocks will likely decrease the overall adult survival.

The decrease in adult survival for the portion of the run that is transported could be due to impaired homing, straying, mortality, longer travel times/greater exposure to harvest or a combination of factors. Transported Snake River steelhead stray more often than their in-river counterparts and tend to enter the Deschutes and John Day river systems (Keefer et al. 2008; Tuomikoski et al. 2012; Keefer and Caudill 2012). Keefer and Caudill (2012) noted that natural straying occurs geographically near the natal site whereas transported fish stray into much more distant rivers. This unintended consequence of the transportation program has the potential to negatively affect much smaller downriver stocks. Using a modeling exercise, Keefer and Caudill (2012) found that strays from large donor populations can numerically overwhelm native fish in small recipient populations, even at low (~1%) stray rates. The size of the combined Deschutes and John Day River spawning populations (~7131; NOAA 2009 Appendix A) is more than an order of magnitude smaller than the numbers of returning Snake River hatchery steelhead adults that pass these basins (~134,145; geomean of hatchery steelhead count at LGR from 2000–2012). Finally, transported Snake River steelhead stray 2–11 times more often than their counterparts that out-migrated in-river (Keefer et al. 2008; Tuomikoski et al. 2010).

The Middle Columbia River Steelhead ESA Recovery Plan (NMFS 2009; Appendix A) concluded that a significant portion of spawners in the Deschutes and John Day River populations were out-of-DPS strays and identified out of basin hatchery steelhead strays as a limiting factor for the recovery of these subbasin populations. Increasing the numbers of transported hatchery Snake River steelhead increases the potential impact of a large Snake River hatchery steelhead population on smaller subbasin populations (Figure 1).

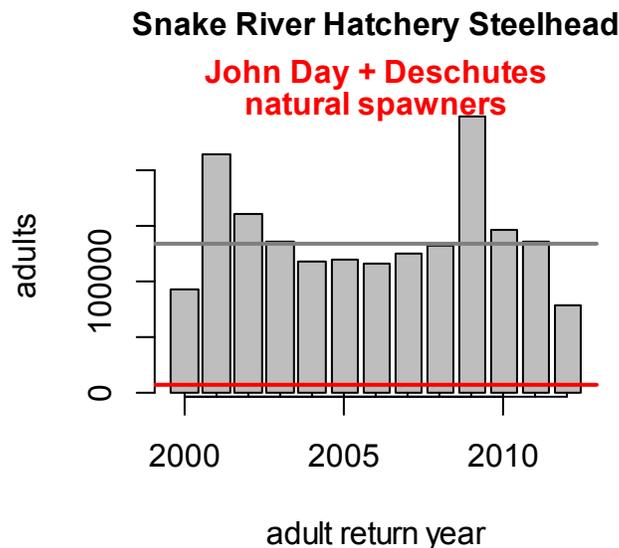


Figure 1. The grey bars are the adult counts at Lower Granite Dam for hatchery steelhead from 2000 through 2012; the grey line is the geometric mean. The red line is the combined geometric mean of spawning populations for the John Day and Deschutes River basins from NMFS (2009 Appendix A).

General Editorial Comments

- The Action Description section of RPA 30 (Draft IP) references a Table 3 and Table 4. These tables no longer exist and, thus, these references should be removed.
- The Action Description section of RPA 30 (Draft IP) mentions that the Corps and BPA will continue to collect and transport juvenile fish at MCN. However, the Adaptive Management section states that transportation for MCN will not occur in 2014–2018.

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Benefits of Spill for Fish Passage – Experimental Spill Management

- NOAA’s use of the spring Chinook returns from 2011 as the basis for rejecting experimental spill management is unfounded because they fail to consider other hydrosystem operations that were adverse for 2011 spring outmigrants. NOAA also fails to recognize the record fall Chinook adult return from the 2011 outmigration, which also experienced high flow and spill throughout the migration period.
- NOAA fails to account for the uncontrolled spill levels due to uncontrolled flows and flood control operations, which were exacerbated by unit outages at projects. The adverse hydrosystem operations resulted in TDG levels often exceeding 130%. In addition, the federal action agencies were unable to; manage debris build-up in the forebays of projects, complete powerhouse outages, and the removal of fish screens sending more fish through turbine units. NOAA failed to address the role these factors played in spring Chinook survival during the 2011 outmigration.
- NOAA fails to recognize or address the adverse outmigration conditions that occurred in 2011 that were a result of the present FCRPS system configuration and operation. NOAA is inappropriately comparing the conditions that occurred in 2011 to a voluntary and controlled spill program as considered under Experimental Spill Management, where 125% TDG is considered as a maximum implementation level.

The Draft BIOP contains the following statement: “We note the adult returns from the year 2011, a year which had high levels of spill and flow, has produced below average adult return rates. Results such as this reinforce our current management approach to hydrosystem operations. Substantial progress has been made in improving survival of juvenile anadromous fish in the hydrosystem.”

It is true that 2011 was a year with high flow and high spill, and thus far the spring Chinook adults that have returned in 2013 appear to be below average. Spring Chinook are the only stock with complete adult counts for 2013 as of this date, with 83,345 adult spring Chinook returned to

Bonneville Dam. The 10-year average count is 141,713. Steelhead returns also appear to be less than the 10-year average, but the B run steelhead that primarily migrated as juveniles in 2011 are still migrating and counting continues.

NOAA's subsequent conclusion that 2011 returns of spring/summer Chinook are a reason to dismiss the experimental spill approach in favor of the current management approach ignores what could likely be the effect of other conditions that occurred in 2011. Furthermore 2011 clearly indicates that the current management approach is flawed, because the current management approach did not provide adequate protection for downstream migrants under the current FCRPS configuration. In 2011 debris could not be managed, screens were removed, and operations were implemented that were adverse to fish.

The Draft BIOP alludes that this is a reason for not addressing Experimental Spill Management, but makes no attempt to address the several distinctions that must be made between the conditions that occurred in 2011 and those under a voluntary spill program as considered in the Experimental Spill Management analyses. NOAA neglects to point out that at least 50% of the juvenile yearling Chinook were past MCN and BON by mid-May. Spill and TDG were generally less than 120% through this period and would have had only positive benefits for fish survival.

It is also important to note that implying the low adult returns from 2011 were a function of high flow and spill is not based on any analysis of the data. Figure 2 shows the juvenile passage dates at LMN for juvenile PIT-tagged Snake River yearling Chinook that were detected as adults at BON this year. While over 60% of the returning PIT-tagged adults that were detected at BON this year passed through the Snake River as juveniles when TDG was below the current States' standards of 120% in the tailrace, the other 40% were migrating in the Snake River when TDG levels exceeded 125% and, at some projects, exceeded 130%. This suggests that NOAA has made the statement in the Draft BIOP prematurely and their dismissal of the Experimental Spill Management on the basis of the low returns from 2011 is unfounded.

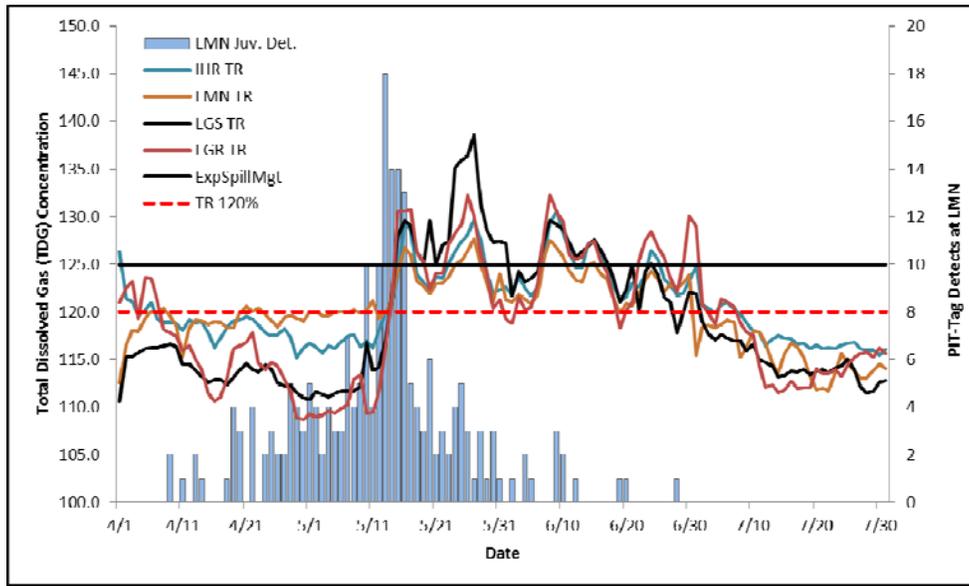


Figure 2. Juvenile passage date (2011) at Lower Monumental Dam of Snake River spring Chinook detected at Bonneville Dam in 2013.

Additionally, the record returns of fall Chinook to the Snake River were predominantly (about 60% based on PIT-tagged adult fish at BON) from 2011 juvenile outmigrants returning as adults. The juvenile passage timing for subyearling migrants was earlier than average and reflected the high flow conditions (Figure 3). More than half of subyearling migrants passed through the hydrosystem during June and the first half of July, and were subjected to the high levels of TDG.

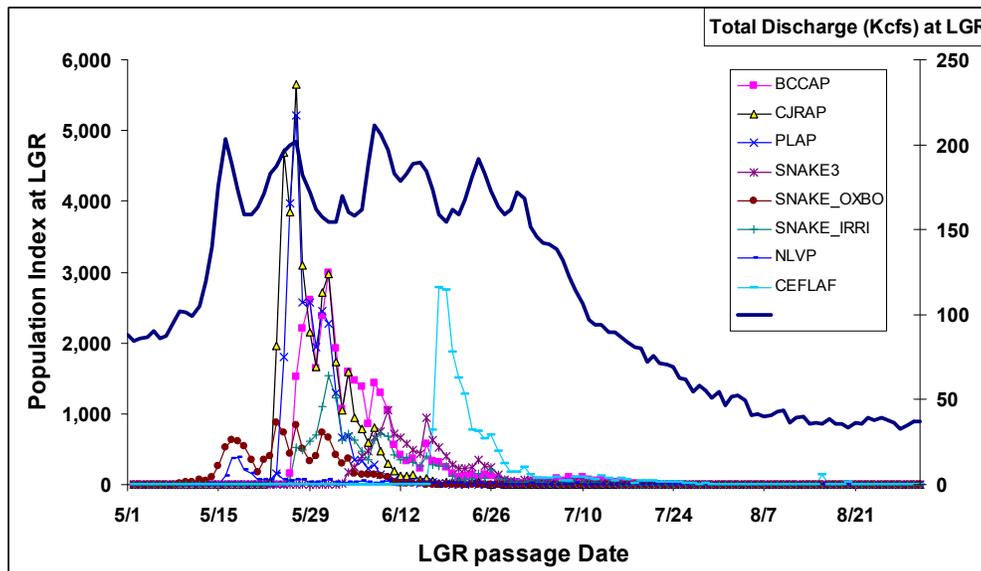


Figure 3. Passage timing of Snake River Fall Chinook PIT-tagged above Lower Granite Dam in 2011 in comparison to high flows that year. Pit-tag release groups include: BCCAP – Big Canyon Creek Acclimation Pond; CJRAP – Captain Johns Rapids Acclimation Pond; PLAP-Pittsburgh Landing Acclimation Pond; SNAKE3-Snake River Releases of Production Fish; SNAKE_OXBO – Snake River releases of fish reared at Oxbow Hatchery; SNAKE_IRRI- Snake River releases of fish reared at Irrigon Hatchery; NLVP-North Lapwai Valley Acclimation Ponds; CEFLAF-Cedar Flats Acclimation Ponds.

Data are too preliminary to determine what the overall SARs for Snake River fall Chinook PIT-tag groups will be, but it is likely the SARs will be relatively high compared to other recent years. In-river fish comprise a large portion of the return. Preliminary SAR analyses of six PIT-tag release groups from migration year 2011 showed five of six groups had transport/in-river-ratios less than one indicating higher return rates for in-river migrants than for transported fish. While it is too early to make any statements about conditions and the return of fall Chinook, the information is introduced here to suggest that NOAA's reliance on 2011 spring Chinook returns to reject Experimental Spill Management is unfounded particularly since fall Chinook, migrating during the peak of the high flow and spill event in 2011, are returning at notably high rates.

High flow conditions are advantageous for fish survival. However, the present configuration and operation of the FCRPS presents issues in a high flow year. High flows often exceed the hydraulic capacity of projects and the excess water must be passed via the spillway. The high spill levels are uncontrolled and cannot be addressed even when criteria for fish condition monitoring for by gas bubble trauma (GBT) are exceeded, as happened in 2011. In fact in 2011 the total dissolved gas levels exceeded the 120% tailrace criteria for extended periods of time during the spring migration period, and, from mid-May until the end of June, the TDG was well in excess of the 125% level, often exceeding 130%. Biological criteria were also exceeded at some projects during this period (FPC Annual Report 2011).

The following graph (Figure 4) shows the tailrace TDG levels that occurred through June 2011 and uses the 125% Experimental Spill Management level for reference. The 125% is the highest level considered in the Experimental Spill Management analyses for fish survival in a risk-based spill program. Consequently, comparing 2011 to what happens under a planned, voluntary spill program in Experimental Spill Management is not appropriate.

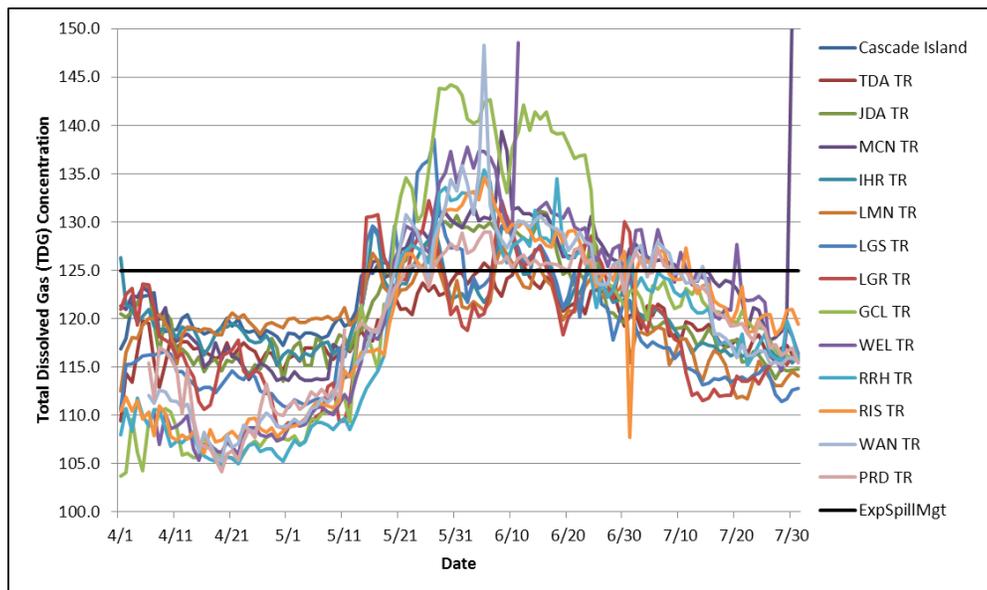


Figure 4. Total dissolved gas (TDG) concentration at each of the TDG tailrace monitors in the Columbia River Hydrosystem, and the experimental spill management level of 125%.

In addition, there were several specific hydropower project configuration operation-related issues that occurred in 2011 that likely contributed to adverse passage conditions for fish, including:

1. Grand Coulee Dam was operated for flood control during most of the spring and early summer season. This resulted in high levels of TDG entering the Upper Columbia River below Chief Joseph Dam, and the high levels of spill and TDG continued through the Upper Columbia projects. TDG levels in the tailraces of projects upstream of Rock Island Dam (where GBT samples are obtained for the Smolt Monitoring Program) exceeded 135% on several days during this period. These high levels of TDG are reflected by the increased GBT incidence in several samples collected between May 31st and June 23rd.
2. There was a very limited hydraulic capacity at LGR due to two out of six units being out of service during the majority of the fish migration season. This resulted in high levels of spill throughout the migration season, with tailrace TDG levels near or greater than 130% for over a month.
3. The Little Goose powerhouse experienced two separate problems that affected all six generating units. This meant that the entire flow in the river was spilled at this project from May 24th at 0600 hours to 1200 hours on June 1, 2011. The TDG below Little Goose exceeded 130% for a 5-day period and reached a 12-hour average high of 138.6%. TDG levels greater than 130% were also observed at the LMN forebay gage during this time period. Both the incidence and severity of the signs of GBT (up to Rank 3 signs) increased during this period. The COE originally expected the repairs to be completed in just a few days, however, during repairs the transformer core was exposed to the atmosphere under wet weather conditions and additional work requiring several additional days was needed to remove all moisture from the transformer core insulation. The biological criteria for spill management were exceeded at LMN as a result of this hydro operation. In a controlled spill program the spill levels would have been reduced, whereas in this operation there was no ability to address these issues.
4. At MCN there were turbine outages throughout the spring and summer, limiting powerhouse capacity. The high river flows that occurred for flood control operations and the limited hydraulic capacity of the project, resulted in uncontrolled spill in excess of hydraulic capacity during early April and from early May until later in July. Tailwater TDG levels often exceeded 130% during this time.
5. There were other factors that occurred in 2011 that are not associated with TDG production that likely contributed to the less than average survival observed by NOAA. High flows are associated with a high debris load from terrestrial runoff. In an undeveloped system the debris would pass through the system, whereas in the developed system debris accumulates at the upstream end of a project and interferes with the safe operation of the hydroproject for fish passage and survival.
6. Considerable debris was collected on fish screens at BON and the COE did not have the resources to remove the debris in a timely manner so as to not affect the juvenile fish

migration. Consequently, due to the increased debris accumulation, the fish screens at BON were removed beginning on May 19th until July 12th. During this period the Bonneville first powerhouse was operated in an overload situation (best geometry), which has unknown impacts for juvenile migrants since no data has been collected on passage through the project under these operations. In addition, the removal of fish screens and its impact are unknown because adequate sampling below BON does not occur in such a way that allows survival estimation.

To summarize, NOAA makes definitive but unfounded conclusions regarding the below average returns of spring Chinook and the attribution of the low returns to high flow and spill that occurred in 2011. Several hydrosystem-related issues occurred during that year that likely contributed to the high TDG levels observed. In addition the potential for project-related mortality from dam operations was high that year. Moreover, the ongoing record return of fall Chinook, with a preponderance of the 2011 cohort in the population, confounds NOAA's statement.

Most importantly, however, is that a high uncontrolled flow and spill year (such as 2011) with the present project configuration and operation is not at all comparable to a voluntary spill program. Experimental Spill Management would be implemented to limit TDG levels, would not exceed 125%, and would be implemented without the configuration and management issues that occurred in 2011.

The Draft BIOP excludes consideration of recent data and analyses and maintains the status quo

The hydrosystem portions of the Draft BIOP rely on three separate documents prepared by consultants for the Action Agencies. They focus unsuccessfully on discounting analyses and conclusions in Haeseker et al (2012). These are:

Manly, 2012. *Review of Assessing Freshwater and Marine Environmental Influences on Life-Stage-Specific Survival Rates of Snake River Spring–Summer Chinook Salmon and Steelhead.*

Skalski et al., 2013. *Limitations of correlative investigations of identifying causal factors in freshwater and marine survival of Columbia River salmonids.*

BioAnalysts, Inc. and Anchor OEA, LLC, 2013. *Federal Columbia River Power System Improvements and Operations Under the Endangered Species Act – A Progress Report, 2013 (Progress Report).*

FPC has reviewed each document and provide the following detailed comments on each document. Our overall summary conclusions are listed below followed by specific comments on each document.

- None of these documents, either considered in total or separately, provide a valid, scientific basis for excluding recent data and analyses from the Draft BIOP.

- Manly 2012 is supportive of recent data and analyses and primarily comments that additional data used in the analyses would be useful. These data were presented graphically, but journal policies did not allow tabular presentation of the same data. The Skalski et al. (2013) document contains several problematic analytical issues that raise serious concerns about the validity of its conclusions.
- The Progress Report is not accurate. It inappropriately relies on Skalski et al. (2013) to discount or exclude new data and analyses that raise serious questions regarding the validity of fundamental components of the Draft BIOP. A significant body of technical review comments and concerns have been raised since 2008 which are not addressed in the progress report, in particular those regarding the application of at-dam performance standards.

Manly (2012)

Manley (2012), was generally a positive review of Haeseke et al. 2012. The Manley review states (bold added by FPC):

“Although I have suggested a reanalysis of the data it seems likely that the results obtained will not change much, and, in particular, that the **Spill and PDO variables will still be estimated to be important to the smolt to adult survival rates.**”

The overarching concern of the Manley (2012) review was that Haeseke et al. (2012) did not include the detailed dataset and tables of all survival estimates, their variances, and the environmental variables for examination and alternate analyses. Due to space limitations in peer reviewed journal articles, tabular presentation of all the raw data and every estimate was not presented in Haeseke et al. (2012). However, all data are available upon request from the lead author.

Skalski et al. 2013

General Comments

Skalski et al. (2013) presents complicated statistical treatments to discount the conclusions of Haeseke et al. (2012). They utilize correlation analyses to argue against correlation analyses. Skalski et al (2013) select a variable that the subject population is not exposed to, in order to illustrate that correlation is not causation. In this analysis Skalski et al. (2013) shows that fish that are transported as smolts show better survival to adult when spill levels are high. Based on this analyses Skalski et al. (2013) concludes that the benefits of higher spill must be false since transported fish would not experience the higher spill levels. Although these statistical treatments may be impressive to some, the basic approach suffers from logical errors and does not provide a convincing argument against the Haeseke et al. (2012) analyses that documents the benefits of spill at multiple life stages. We provide specific comments on these analyses in the following discussion.

Skalski et al. (2013) selects a variable that the subject population is not exposed to, in order to illustrate that correlation is not causation. This is a fundamental principal, but good principals can also be used to buttress bad arguments (Gould 1991). This is similar to historic arguments regarding smoking and the occurrence of lung cancer in which Ronald A. Fisher (preeminent statistician and paid consultant for the Tobacco Standing Committee) argued that the considerable body of decades of data that showed a significant correlation between smoking and lung cancer did not establish causation. On that basis, legally required warnings and recognition that smoking caused lung cancer was delayed for decades at obvious costs. Skalski et al. (2013) argues against observational studies while recognizing that in the Columbia River system controlled experiments are not possible. Observational studies are scientifically well established, accepted, and extensively used in the fields of ecology, toxicology, paleontology, geology, and epidemiology in particular (Cochran 1983, Eberhardt and Thomas 1991, Rothman and Greenland 1998, Woodward 2005, Jewel 2005).

We do not believe that the complicated statistical treatments in Skalski et al. (2013) provide anything meaningful in terms of spill or transportation effects. For example, Skalski et al. (2013) failed to consider the fact that transported smolts and in-river smolts have shared experience in their downstream migration. They share a river experience as they migrate together to the first transportation collection site and share their migration experience below Bonneville Dam where transported fish are released and migrate with in-river migrants. In general high spill and high flow occur together in the Snake and Columbia rivers. Fish migrating in high flow conditions could arrive at the upstream transportation collection site faster and in better condition, and possibly better withstand the rigors of the transport system. In addition, in high flow years, which usually occur with high spill, transported and in-river smolts could experience better migration conditions through the lower Columbia River below Bonneville Dam due to higher flows. In attempting to disclaim the benefits of spill for fish passage, Skalski et al. (2013) presented a correlation analyses of an effect not experienced by their subject population but may not have considered the possibility that their spill variable actually represented a third variable, of high flow.

Specific comments

- Skalski et al. (2013) makes the comment that yearling Chinook have a maximum juvenile survival when spill is 20%, yet the model used for this same analysis predicts a maximum Chinook survival at 0% percent spill, which is obviously incorrect.
- Our attempt to replicate the multicollinearity analysis of Skalski et al. (2013) suggests that incorrect data or an incorrect calculation was used. Once this error is resolved, the section on multicollinearity is moot due to low levels of multicollinearity.
- Three methods of analyses show that the data in Haeseker et al. (2012) support linear relationships, but do not support higher-order processes, optima, thresholds, or spline relationships for the hydrosystem operations variables under management control (spill and water transit time).
- The comment by Skalski et al. (2013) that model averaging shouldn't be used is not referenced and is not supported by data or analyses. This point is at odds with a large body of peer-reviewed publications that use these techniques.

- Many studies support the use of the model averaging methods employed by Haeseker et al. (2012) as an appropriate and rigorous procedure to account for model selection uncertainty and to improve inference.
- Skalski et al. (2013) uses the length of wild and hatchery smolts marked and transported at LGR as a surrogate for fish condition. Since hatchery smolts are clearly larger than their wild counterparts, length does not seem to be a useful or realistic surrogate for fish condition.
- An analysis of the dataset of length for wild and hatchery smolts marked and transported at LGR found a correlation between smolt length and offshore upwelling. The authors seem to be suggesting that the lengths of wild and hatchery fish are somehow related to nearshore ocean processes, which defies logic and conflicts with Skalski et al.'s (2013) earlier point of causation and correlation.
- The authors' finding of a correlation between transported SAR and in-river variables isn't unexpected as the distance from barge release to the river mouth is approximately one-third the distance from LGR to the river mouth. It is not a biologically reasonable hypothesis that smolt survival is independent of river conditions, in particular flow or water transit time, for the remaining 145 miles from Skamania Landing to the river mouth.
- The list of predictions presented by Skalski et al. (2013) are illogical convolutions of the hydrosystem-related delayed-mortality hypothesis of Budy et al. (2002). The analyses presented to investigate their predictions fail to use as they stated earlier, "appropriate spatial and temporal scales of the survival processes...in order to measure the potential covariates at the right geographic scale."
- The power analysis presented in Skalski et al. (2013) greatly exaggerates the amount of time that would be required to detect changes in survival at multiple life stages associated with a spill management experiment.

Yearling Chinook have maximum survival at 20% spill: Williams et al. reference

Skalski et al. (2013) references Williams et al. (2005) to note that, "For yearling Chinook salmon, survival reached a maximum at 20.6% spill." The general additive model (GAM) that Skalski et al. (2013) refers to also predicts a maximum Chinook survival when spill is zero (Figure 5). Many lines of evidence based on empirical data show that zero spill is detrimental for juvenile survival and in fact we are not aware of any other analysis available that would show that the highest possible survival would occur under zero spill operations.

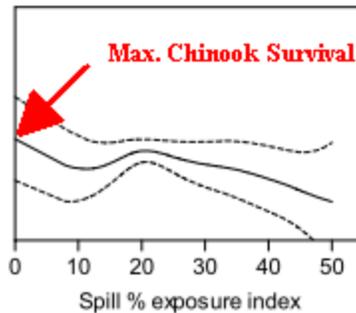


Figure 5. Results of generalized additive model for Chinook showing the highest survival is when spill is zero. From Figure 37 in Williams et al. (2005). Williams et al. (2005) notes that the y-axis units are not meaningful but that the relative influence of spill % on survival can be ascertained from the model. Red arrow and text added by FPC.

For their own GAM analysis Skalski et al. (2013) used survival data from FPC. However their method for combining separate reach survival estimates of LGR to MCN and MCN to BON to achieve LGR to BON estimates is questionable. The FPC data are from 1-week cohorts in the Snake River and 2-week cohorts in the Columbia. Skalski et al. (2013) combined data into an annual LGR to BON single cohort by multiplying the geometric mean survivals for each reach. This hybrid survival estimate was then coupled with environmental covariates which were weighted by the number of dams in each reach. With this sort of manipulation it is dubious if the resulting data resemble the initial data set from FPC. There is no mention of the methods used to fit their GAM (e.g., how many knots were allowed in the models? How did they assess as fit?). Since they used such an unusual method for reach survival calculations it seems unlikely that any form of weighting was used to account for different variances in the survival estimates. Estimates for the MCN to BON reach were much more variable than those for the Snake River. And recent estimates were less precise. There was no mention of how these were accounted for, which could lead to biased model coefficients if all observations were treated equally. In addition, there is a substantial risk of overparameterization associated with fitting thirteen observations with a three-variable generalized additive model. This overparameterization would be expected to result in increased bias and poor predictive performance.

Multicollinearity

On May 31, 2013, Rebecca Buchanan (University of Washington) requested the survival estimates from Haeseker et al. (2012) in order to “perform analyses using the same data that you used” and “to explore additional relationships associated with hydrosystem operations.” Those survival estimates, their variances, and the environmental variables were sent to Rebecca Buchanan on June 6, 2013. Despite the fact that the necessary data was provided to them, Skalski et al. (2013) failed to examine whether multicollinearity was an issue in the data analyzed in Haeseker et al. (2012).

Skalski et al. (2013) states that serious concerns over multicollinearity arise when the variance inflation factor for any of the covariates exceeds the value of 10 (Neter et al. 2004). To address whether multicollinearity was an issue in the analyses presented in Haeseker et al. (2012), we calculated the variance inflation factors as specified in Neter et al. (2004). The variance inflation

factors for the environmental variables used in Haeseker et al. (2012) were all less than 3.5 (Table 5) indicating that multicollinearity was not a serious issue in the regression results presented there. It is important to note that even when correlations among predictor variables are present, these correlations do not inhibit the ability to obtain a good fit, the ability to make predictions of new observations, or the ability to make inferences about mean responses (Neter et al. 1996).

Table 5. Variance inflation factors for environmental variables analyzed in Haeseker et al. (2012).

Variable	Chinook	Steelhead
Day	1.2	1.2
% Hatch	1.1	1.8
WTT	3.2	3.5
Spill	3.0	3.3
SST	1.6	2.2
PDO	1.4	2.2
Upwelling	1.3	1.3

Skalski et al. (2013) devotes a large section to the idea of multicollinearity and the potential confounding results on models. Despite having the data from Haeseker et al. (2012), Skalski et al. (2013) did not conduct analysis to examine whether or not their concerns regarding multicollinearity were valid. Instead, as proof of concept, Skalski et al. (2013) constructs a new data set of several in-river, oceanic, and terrestrial variables. Multicollinearity in the new Skalski et al. (2013) dataset is tested using the variance inflation factor (VIF) for each variable. When VIF exceeds 10 then there are multicollinearity issues. Skalski et al. (2013) states that VIF values among the in-river variables in the dataset ranged from 30.31 to 185.69 suggesting “extreme” issues of multicollinearity. *In fact these VIF values are so high that an error in calculation or an incorrect dataset is likely.* A VIF value of 185.69 between two predictor variables means that a linear regression of those two variables would have an R^2 value of 99.46%. (Table 6).

Table 6. Calculations of VIF presented in Skalski et al. (2013) and those calculated by FPC (highlighted) using the data from Table 2 presented in Skalski et al. (2013). Skalski et al. (2013) notes that model results would be a matter of concern when VIF exceeds 10. FPC calculations of VIF for in-river variables are always less than 10 and 20–27 times smaller than those presented by the authors.

Publication	Range in VIF for Ocean	Range in VIF for In-river
Skalski et al. 2013	1.09-1.60	30.01-185.69
FPC	1.09-1.60	1.56 - 6.90

Skalski et al. (2013) provides a correlation matrix for the new dataset in their Table 2. Estimates for VIF can be easily calculated as the diagonals from the inverse matrix of portions of Table 2. FPC calculated the VIFs among the ocean variables and then among the river variables from the correlations presented in Table 5. As compared to Skalski et al. (2013) our calculation was identical for the ocean variables and 20 to 27 times smaller for in-river variables (Table 6). The

values we calculated for in-river variables were all less than 10, the cutoff suggested by the authors.

Comments on Generalized Additive Models and non-linear relationships

Skalski et al. (2013) faults regression analyses that ignore higher-order processes, the possibility of optima, thresholds, or spline relationships and recommend that advanced regression techniques such as generalized additive models be used. When the data support these approaches, they may indeed improve model fit. However, when the data do not support these approaches, overparameterization, increased bias, and poor predictive performance will result. Skalski et al. (2013) fails to mention these risks.

FPC used three methods to assess whether non-linear, higher-order processes or generalized additive models would have improved model fit in the data presented in Haeseker et al. (2012). First, we conducted simple linear regressions between the hydrosystem operations under management control (water transit time and average percent spill) and the stage-specific survival rates presented in Haeseker et al. (2012). These simple linear regressions suggest that there is a linear response between hydrosystem operations and freshwater survival, ocean-adult survival, and smolt-to-adult survival rates (Figures 6 and 7). These data also do not show indications of higher-order processes, optima, or thresholds. Contrary to the coarse-scale, and likely overparameterized, analysis on freshwater survival presented in Skalski et al. (2013), the data in Haeseker et al. (2012) indicate increasing survival at all life stages with increasing spill levels and reductions in water transit time (Figures 6 and 7). The data simply do not support Skalski et al.'s (2013) conclusion that survival is maximized at 35% spill levels.

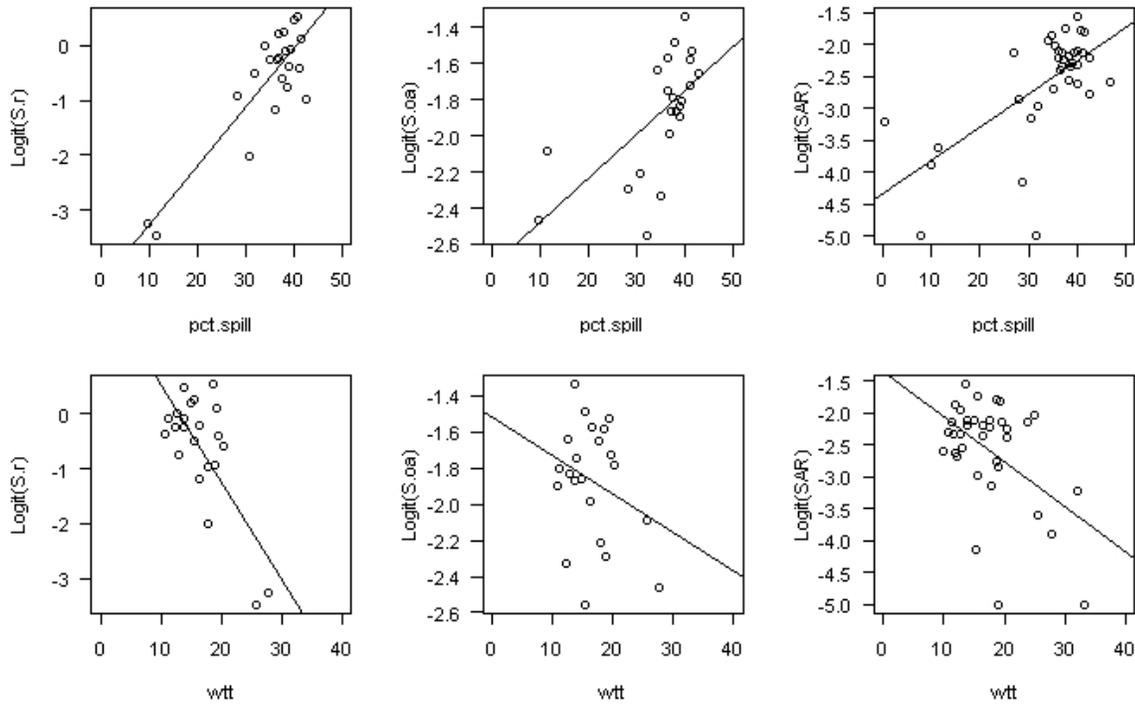


Figure 6. Simple linear regressions of logit-transformed freshwater survival (S.r), ocean-adult survival (S.oa), and smolt-to-adult survival (SAR) versus average percent spill and water transit time for spring/summer Chinook salmon using data presented in Haeseker et al. (2012).

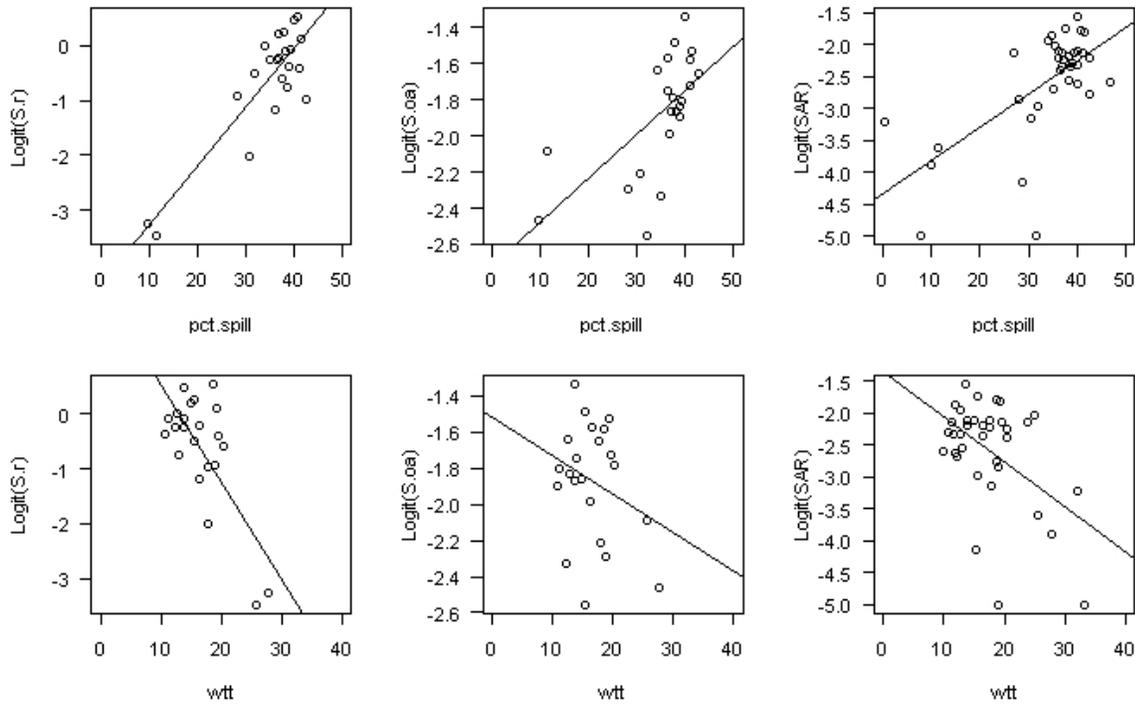


Figure 7. Simple linear regressions of logit-transformed freshwater survival (S.r), ocean-adult survival (S.oa), and smolt-to-adult survival (SAR) versus average percent spill and water transit time for steelhead using data presented in Haeseker et al. (2012).

As a second method to assess whether higher-order processes were supported by the data presented in Haeseker et al. (2012), we conducted F-tests comparing the full models at each life stage to models that also included quadratic terms for spill and water transit time. None of those tests supported the inclusion of quadratic terms for spill or water transit time (all P-values > 0.36). As a third method to assess whether non-linear processes were supported by the data, we fit generalized additive models to the data presented in Haeseker et al. (2012). Both smoothing splines and locally-weighted scatterplot smoothing (loess) forms of generalized additive models performed far worse than linear regression for both species and at all survival rate stages based on Akaike's Information Criterion (Table 7). In addition, the generalized additive models required the estimation of double to triple the number of parameters as the linear regression approach. In the case of steelhead ocean-adult survival, the generalized additive model approach required the estimation of more parameters than the number of observations. Clearly, applying a generalized additive model approach to the data in Haeseker et al. (2012) would have been inappropriate and would have resulted in an overparameterized model with increased bias and poor predictive performance.

Skalski et al. (2013) failed to examine whether generalized additive models would have improved model fit in Haeseker et al. (2012), despite being provided with the data to do so. In summary, the three sets of analyses presented here show that the data in Haeseker et al. (2012) support linear relationships, but do not support higher-order processes, optima, thresholds, or spline relationships for the hydrosystem operations variables under management control (spill and water transit time). Furthermore, the linear regressions presented in Haeseker et al. (2012) are likely to have less bias and better predictive performance than would be obtained by a generalized additive model approach that would suffer from overparameterization.

Table 7. Summary statistics (n = number of observations, k = number of estimated parameters, AIC = Akaike's Information Criterion) for linear regression, generalized additive model regression using smoothing splines, and generalized additive model regression using locally weighted scatterplot smoothing (loess) approaches for modeling freshwater (S.r), ocean-adult survival (S.oa), and smolt-to-adult survival (SAR) using data presented in Haeseker et al. (2012).

Method	Species	S.r			S.oa			SAR		
		n	k	AIC	n	k	AIC	n	k	AIC
Linear Regression	Sp/Su Chinook	33	6	-10.1	33	9	8.1	36	9	2.3
GAM (spline)	Sp/Su Chinook	33	13	-6.1	33	22	13.5	36	22	5.7
GAM (loess)	Sp/Su Chinook	33	14	-9.4	33	26	22.2	36	26	14.3
Linear Regression	Steelhead	22	6	-9.5	22	9	0.2	36	9	42.5
GAM (spline)	Steelhead	22	13	-0.1	22	22	NA	36	22	45.6
GAM (loess)	Steelhead	22	14	1.8	22	26	NA	36	28	58.8

Comments on Model Averaging and Model Selection Uncertainty

Skalski et al. (2013) acknowledges that model averaged predictions account for model selection uncertainty but says no theoretical basis has been given for model averaged regression or model averaged coefficients. However, Burnham and Anderson (2002) state that with closely related

models, selecting a single model is often unsatisfactory because it can incorporate a “model selection bias” which will affect the coefficient estimates. To account for model selection uncertainty, Burnham and Anderson (2002) provide a formal method for inference from more than one model. Although the method of model averaging and the use of Akaike’s Information Criterion (AIC) is not promoted by Skalski et al. (2013), it has been used extensively in the Columbia River Basin (Scheuerell et al. 2009, Holsman et al. 2012) and in particular it is used in mark recapture studies. A Google Scholar search revealed that Burnham and Anderson (2002) has been cited *twenty thousand, one hundred and forty nine times* as of September 18, 2013.

In fact, in some cases model averaging may be the preferred tool of inference over the use of a single model. Lukacs et al. (2010) used a Monte Carlo simulation to compare model averaged results using similar methods as in Haeseker et al. (2012) versus results from a single model selected with stepwise regression. Those authors found that using a single model for inference instead of employing model averaging produced confidence intervals with poor coverage and estimated coefficients that were biased up to three times larger than the true value. Finally, the authors note that stepwise regression in particular is not the problem, but rather that inference from a single model can produce a biased result. These results were confirmed by Claeskens and Hjort (2008) who demonstrated both mathematically and with simulations that using a single model for inference can underestimate standard deviations, bias coefficient estimates, and underrepresent type I error. These and other studies support the use of model averaging as an appropriate and rigorous procedure to account for model selection uncertainty and improve inference.

Comments on “Regression, Retrodution, and Beyond”

The authors state that drawing inferences from observations (retrogression) can be improved through the further development of hypotheses that can be tested (hypothetico-deductive model) through falsification of predictions. Budy et al. (2002) presented and discussed evidence that some of the mortality that occurs during the period of estuary and early ocean residence is related to earlier hydrosystem experience during downstream migration, a concept known as the hydrosystem-related, delayed-mortality hypothesis. Several analyses in Haeseker et al. (2012) provided support for the hydrosystem-related, delayed-mortality hypothesis for downstream-migrating spring/summer Chinook salmon and steelhead from the Snake River. Skalski et al. (2013) presents several predictions that would follow if the hydrosystem-related, delayed-mortality hypothesis of Budy et al. (2002) were true. However, our examination of these predictions show that the premises underlying the Skalski et al. (2013) predictions are illogical convolutions of the hydrosystem-related, delayed-mortality hypothesis of Budy et al. (2002). We discuss the illogic of each of their predictions below.

No correlation between ocean covariates and in-river covariates

Contrary to Skalski et al.’s (2013) assertion, the hydrosystem-related, delayed-mortality hypothesis makes no statements about whether there is or is not a correlation between freshwater and ocean covariates. The hypothesis simply states that some of the mortality that occurs during the period of estuary and early ocean residence is related to earlier hydrosystem experience during downstream migration. The prediction of no correlation between freshwater and ocean covariates based on the hydrosystem-related, delayed-mortality hypothesis is therefore an illogical premise. Although this premise is illogical, it should be noted that Haeseker et al.

(2012) found no correlation between freshwater and ocean covariates. Furthermore, FPC believes that Skalski et al. (2013) incorrectly calculated the amount of association between in-river and ocean variables (see the Multicollinearity section of this document).

No correlation between the adult return rate (i.e., SAR) of transported fish and in-river covariates because transported fish have little opportunity to experience the hydrosystem

This hypothesis and its relation to Haeseker et al. (2012) is not logical. The hydrosystem-related, delayed-mortality hypothesis states that some of the mortality that occurs during the period of estuary and early ocean residence is related to earlier hydrosystem experience during downstream migration. A key element of the hydrosystem-related, delayed-mortality hypothesis is exposure to a “hydrosystem experience during downstream migration.” Because transported fish do not have a “hydrosystem experience during downstream migration,” the hydrosystem-related, delayed-mortality hypothesis makes no predictions on the factors that influence the survival of transported fish. The premise behind this prediction does not follow from the hydrosystem-related, delayed-mortality hypothesis. It is important to note the transported fish do experience some portion of the hydrosystem prior to collection at the transportation sites, and flow conditions downstream of BON following release may influence their survival rates. Therefore, researchers should utilize the “appropriate spatial and temporal scales of the survival processes...in order to measure the potential covariates at the right geographic scale” following the admonishment of Skalski et al. (2013). Unfortunately, Skalski et al. (2013) fails to follow their own advice in the specification of appropriate temporal and geographic scaling for covariates related to transported fish. Transported fish are typically released at Skamania Landing which is very near Bonneville Dam. Therefore transported smolts are released approximately 145 miles from the mouth of the Columbia. This distance is more than a third of the distance from LGR to the river mouth. We agree that transported smolts have little opportunity to experience the hydrosystem. However, it is not likely or a biologically reasonable hypothesis that their survival is independent of river conditions for the remaining 145 miles from Skamania Landing to the river mouth. In addition, the flows likely affected both the survival and condition of the juvenile migrants prior to their collection for transportation.

No correlation between in-river smolt survival and ocean covariates

A reasonable explanation for a potential correlation between in-river survival and ocean covariates was not presented by the authors. The hydrosystem-related, delayed-mortality hypothesis states that some of the mortality that occurs during the period of estuary and early ocean residence is related to earlier hydrosystem experience during downstream migration. The mechanisms (injury, predation, physiological stress, migration delay, and disease) underlying the hydrosystem-related, delayed-mortality hypothesis were described by Budy et al. (2002). There is no plausible mechanism whereby freshwater smolt survival would influence abiotic features of the ocean. Therefore, there is no logical premise or mechanism behind this illogical prediction. This hypothesis was presented by the authors directly preceding an analysis that used only transported fish which do not emigrate in-river. This hypothesis was not tested by the authors.

No correlation between smolt condition and ocean survival

The hydrosystem-related, delayed-mortality hypothesis states that some of the mortality that occurs during the period of estuary and early ocean residence is related to earlier hydrosystem experience during downstream migration. Given appropriate geographic and temporal scaling of

comparison groups, the hydrosystem-related, delayed-mortality hypothesis would make predictions that hydrosystem-related smolt condition indices would be correlated to ocean survival. For example, Tuomikoski et al. (2010) found that the number of bypass experiences influenced ocean survival rates for spring/summer Chinook salmon and steelhead, consistent with the hydrosystem-related, delayed-mortality hypothesis. Skalski et al.'s (2013) prediction of no correlation between hydrosystem-related indices of smolt condition and ocean survival is actually opposite that predicted by the hydrosystem-related, delayed-mortality hypothesis. Again, FPC agrees with Skalski et al.'s (2013) admonishment that researchers should utilize the "appropriate spatial and temporal scales of the survival processes...in order to measure the potential covariates at the right geographic scale." However, smolt length at time of transportation is not a hydrosystem-related index of smolt condition. Smolt length is a function of growth in hatcheries or rearing tributaries over the previous 2 years. Therefore, smolt length at transportation is not an appropriate metric to measure hydrosystem-related smolt condition. The data that the authors use to test this hypothesis are length data at LGR for hatchery and wild smolts marked at the same location. The authors do not show any supporting evidence for their use of length as a surrogate for condition. Further, given that these data contain a mixture of hatchery and wild smolts, and hatchery smolts are clearly larger than their wild counterparts, what does the length variable represent? This hypothesis was not tested adequately enough to support any conclusions by the authors. Finally, the finding of the authors that the dataset of hatchery and wild fish lengths are correlated with offshore upwelling is not supported by data. The authors appear to suggest that the length of hatchery fish at LGR is related to nearshore ocean processes measured at the same time. The authors do not offer any rationale, data, or mechanism to support their conclusion. This conclusion in their analysis contradicts their own caution regarding the dangers of not knowing the "appropriate spatial and temporal scales of the survival processes...in order to measure the potential covariates at the right geographic scale." The identification of a positive correlation between length and upwelling is an example of how poor specification of mechanisms can lead to erroneous conclusions.

In summary, the list of predictions presented by Skalski et al. (2013) are illogical convolutions of the hydrosystem-related, delayed-mortality hypothesis of Budy et al. (2002). The analyses presented to investigate their predictions fail to use "appropriate spatial and temporal scales of the survival processes...in order to measure the potential covariates at the right geographic scale." As a result, their analyses do not provide falsification of the hydrosystem-related, delayed-mortality hypothesis that was investigated by Haeseker et al. (2012). Given these inappropriate predictions and flawed analytical approaches, their criticisms of Haeseker et al. (2012) are not well founded and lack sufficient scientific credibility to raise any doubts about the conclusions of Haeseker et al. (2012). Their analysis would benefit from proper application of the hypo-deductive method using accurate predictions of the hydrosystem-related, delayed-mortality hypothesis and data collected at the appropriate spatial and temporal scales of the survival processes.

Power Analysis

The power analysis presented in the discussion section of Skalski et al. (2013) falls short for a number of reasons. First, the power analysis focuses on a short reach (LGR-MCN) that has less variability among years explained by spill than longer reaches (LGR-BON) or other life stages (e.g., ocean survival or smolt-to-adult survival). By focusing on a short reach with less

variability explained by spill effects, the resulting duration is overestimated compared to that expected by an experiment focusing on longer reaches or SARs. Second, the magnitude of change in LGR-MCN survival (10%) estimated by Skalski et al. (2013) is small relative to the changes in SARs expected through increased spill levels. Simulations presented in Hall and Marmorek (2013) indicate that SARs may increase by nearly 100% at high spill levels. With these larger expected differences in SARs, the expected amount of time that would be required to detect a change is much shorter. Because of these issues, the power analysis presented in Skalski et al. (2013) greatly exaggerates the amount of time that would be required to detect changes in survival at multiple life stages associated with a spill management experiment.

BioAnalysts, Inc. and Anchor QEA, LLC, 2013

- Assessing the success of actions in the 2008 Biological Opinion implementation on the basis of achieving performance standards is inappropriate and does not consider recent data and analyses.
- Smolt-to-adult return rates of Snake River spring Chinook and steelhead remain dangerously low.
- Comments provided in the critique of Haeseker et al. (2012) are out of date and do not consider recent updates to the analysis which include the effect of removable spillway weirs (RSWs) and surface passage. These updated analyses have been presented in public forums, have been reviewed, and are publicly available.
- Arguments made in this document regarding the inability to affect flow in the present configuration of the hydrosystem provides support for the consideration of Experimental Spill Management.
- The authors provide critiques of peer reviewed and published analyses that do not support their point of view, while presenting without consideration of public review critiques of published analyses that do support their point of view.

Bonneville Power Administration (BPA) provided a draft report of “Federal Columbia River Power System Improvements and Operations under the Endangered Species Act – A Progress Report.” The report is a review of the results of the implementation of the actions in the 2008 Biological Opinion, and is based on analyses conducted by BioAnalysts, Inc. and Anchor QEA, LLC. The report addresses improvements in juvenile survival through the FCRPS and concludes that:

“...monitoring results and performance tests indicate that the new configuration and operation of dams have improved juvenile fish survival through the FCRPS to levels roughly comparable to those realized decades ago, when fewer dams were in place. Annual estimates indicate an upward trend in survival of juvenile steelhead and yearling Chinook salmon migrating through the Snake and Columbia rivers over the last two decades.”

The draft paper is largely an endorsement of current implementation based on the studies conducted relative to performance standards at each hydroproject. The draft report does not address significant serious technical concerns that have been raised over the past several years

regarding the concept and approach of performance standards. NOAA has failed to address or consider recent data and analyses that raise serious issues regarding the validity of the performance standard concept and approach, specifically that route of dam passage affects later life stage survival (see our previous comments on performance standards). Recent data and analyses indicate that freshwater passage experience affects later life stages and adult returns, which are not considered in performance standard implementation in this Draft BIOP. Recent data indicate that a smolt-to-adult return rate would provide a more realistic performance standard.

Specifically, while juvenile survival rates may have improved compared to years when in-river conditions were hazardous to fish survival due to minimizing spill and the maximization of transportation, data regarding adult return rates show that on average spring Chinook from the Snake River have smolt-to-adult survival rates of less than 1% and steelhead smolt-to-adult survival rates average less than 2%. This should be put in context of the Northwest Power and Conservation Council goals of achieving smolt-to-adult return rates between 2% and 6%, with an average of 4%.

The BPA draft report also provides a critique of Haeseker et al. (2012) based on reviews conducted by Skalski et al. (2013) and Manly (2012) who were commissioned by the Action Agencies. (Note: these references are cited as 2012 and 2011, respectively, in the BPA paper. In light of the fact that Haeseker et al.(2012) was published in 2012, it would make a Manly review in 2011 most improbable). The reviews of Skalski et al. (2013) and Manly (2012) are addressed in detail elsewhere in this paper. However, The Progress Report (BioAnalysts Inc. and Anchor QEA, LLC 2013) excludes consideration of recent data and analyses, and mistakenly concludes that Haeseker et al. (2012) does not address adult returns past 2006 and therefore does not incorporate the benefits of RSWs. The recently updated analyses of Haeseker et al. (2012) incorporate the effect of surface passage structures. These recent analyses have been reviewed and presented in public forums and they are publicly available. The updated analyses of Haeseker et al. (2012), to incorporate the effect of surface passage structures, was completed in response to a direct recommendation from NOAA Fisheries.

One point that BPA brings up in the review is,

“Water particle travel time (an index of river flow) was another key predictor variable in the Haeseker et al. (2012) analysis. They noted that lower SARs were associated with higher WTT indices and projected that reduced WTT would promote higher SARs. However, it is not clear what incremental changes in WTT are possible within any given water year, given water management operations that need to balance across sometimes competing demands including flood control, irrigation, recreation, water quality (total dissolved gas limits under the Clean Water Act), and international water treaties, as well as fish resources.”

FPC agrees with the statement that given the present configuration, operation, and obligations of the hydrosystem there is little opportunity to provide additional changes in river flow. This is precisely why Experimental Spill Management is so appealing since it can be implemented in any flow year, without impacting reservoir operations. The question of TDG limits under the

Clean Water Act can easily be addressed through the assessment of biological information that has been collected under much higher spill and TDG limits (due to the impacts of the hydrosystem operation under the present FCRPS configuration) than presently contemplated under Experimental Spill Management.

It is unfortunate that, while BPA commissioned reviewers to assess the Haeseke et al. (2012) paper, they present Rechisky et al. (2013) without any such reservation. The Rechisky et al. (2013) paper presents information contrary to the conclusions in Haeseke et al. (2012). In fact, when the CSS Oversight Committee reviewed the Rechisky et al. (2013) study (review attached) they found significant flaws with the methods and assumptions used in the study. Results indicate significant confounding due to tagging and handling effects. Critical assumptions regarding offshore ocean migration patterns and array detection efficiency are inconsistent with the available data. Because of these issues, the conclusions regarding hydrosystem-related delayed mortality are overreaching and unsupported. In addition, the Rechisky et al. (2013) study was conducted for only 3 years and had little contrast in ocean and river conditions as compared to recent long-term studies (9-60 years) that provide evidence of significant hydrosystem delayed mortality (Deriso et al. 2001, Schaller and Petrosky 2007, Petrosky and Schaller 2010, and Haeseke et al. 2012).

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Attachment

Review of “Influence of multiple dam passage on survival of juvenile Chinook salmon in the Columbia River estuary and coastal ocean” by Rechisky, Welch, et al., April 23, 2013



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MEMORANDUM

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FROM: Michele DeHart

DATE: May 22, 2013

SUBJECT: Review of "Influence of multiple dam passage on survival of juvenile Chinook salmon in the Columbia River estuary and coastal ocean" by Rechisky, Welch, et al. April 23, 2013

The Comparative Survival Study Oversight Committee, comprised of technical representatives of the state, federal and tribal fishery management agencies reviewed the subject article. The article was published in the April 2013 edition of the Proceedings of the National Academy of Science (PNAS).

The Oversight Committee review found significant flaws with the methods and assumptions used in this study. Results indicate significant confounding due to tagging and handling effects. Critical assumptions regarding offshore ocean migration patterns and array detection efficiency are inconsistent with the available data. Because of these issues, the conclusions regarding hydrosystem-related delayed mortality are overreaching and unsupported. In addition, the Rechisky study was conducted for only three years and had little contrast in ocean and river conditions as compared to long term studies (9–60 years) that provide evidence of significant hydrosystem delayed mortality (Deriso et al. 2001, Schaller & Petrosky 2007, Petrosky and Schaller 2010, and Haeseker et al. 2012). As part of the scientific, peer-review process, the Oversight Committee determined that it was necessary to submit a Letter to the Editor of the

PNAS describing our technical concerns with the methods, assumptions, and conclusions of the article. The PNAS criteria for Letters to the Editor limit the response to 500 words and 5 references. PNAS at times publishes longer commentaries but those are “by invitation only.” A brief Letter to the Editor was submitted to PNAS. Below is the complete version of the CSS Oversight Committee review of the Rechisky/Welch journal article.

Comments on “Influence of multiple dam passage on survival of juvenile Chinook salmon in the Columbia River estuary and coastal ocean” by Rechisky et al. (2013)

Introduction

As a test for the hydrosystem-related delayed mortality hypothesis (Budy et al. 2002), Rechisky et al. (2013) conducted a mark-recapture study using acoustic telemetry that compared the estimated survival rates of a group of hatchery-origin spring Chinook salmon that migrated through four dams to a group of hatchery-origin spring Chinook salmon that migrated through eight dams in the Columbia River basin. They report that their estimates of river, estuary and early-ocean survival rates were similar between the two groups. Because their estimates did not provide evidence that hydrosystem-related delayed mortality occurred in the estuary or the first month in the coastal ocean, they suggest that hydrosystem mitigation efforts may be ineffective. However, our examination of their methods, assumptions, and results indicate that their results are confounded by significant tagging effects and that their assumptions are inconsistent with the available data. In addition, this study was conducted for only three years and had little contrast in ocean and river conditions as compared to long term studies (9–60 years) that provide evidence of significant hydrosystem delayed mortality (Deriso et al. 2001, Schaller & Petrosky 2007, Petrosky and Schaller 2010, and Haeseker et al. 2012). For these reasons, their conclusions regarding hydrosystem-related delayed mortality are overreaching and unsupported.

Non-Representative Tagging and Tagging Effects

A key requirement of tagging studies is that tagged individuals should be representative of the untagged population of interest. If significant differences exist between the tagged and untagged populations, then inferences can become limited to the tagged individuals themselves and may not be applicable to the untagged population of interest due to confounding. Rechisky et al. (2013) fail in numerous ways to meet this basic requirement for the design and implementation of tagging studies, which results in a highly confounded study.

Hatchery spring Chinook salmon from Cle Elum National Fish Hatchery (NFH) in the Yakima River exhibit smolt-to-adult return rates (SARs) that are substantially higher than SARs of Dworshak NFH (Tuomikoski et al. 2012). For spring Chinook salmon from Cle Elum NFH and

Dworshak NFH to be used as populations for inference, fish selected for tagging should have been representative of the releases that have exhibited the differential survival rates reported in Tuomikoski et al. (2012). However, the fish that were selected for tagging differ from these groups selected for inference in several important ways. Fish with acoustic tags were 10–20 mm longer, were released 21–83 days later, and were released 59–249 rkm further downriver than their corresponding hatchery populations of inference. Length at tagging (Zabel and Achord 2004), timing of release (Haeseker et al. 2012, Scheuerell et al. 2009), and migration distance (Faulkner et al. 2012) have all been shown to influence survival rates of Chinook salmon at multiple life stages. Any of these factors alone confound comparisons with the populations of inference, let alone the combination of all three. In addition, hydrosystem conditions in terms of water velocity and spill percentages experienced by out-migrating juvenile Chinook salmon can vary substantially within and among years (Haeseker et al. 2012). Thus, the hydrosystem conditions experienced by groups released 21–83 days apart were most likely different. In summary, highly confounded differences in length at tagging, timing of release, location of release, and probable differences in hydrosystem conditions following release most likely invalidate any comparisons between the Cle Elum and Dworshak hatchery stocks that were tagged with passive integrated transponder (PIT) tags and the acoustic-tagged individuals that were examined in Rechisky et al. (2013). Any differences or similarities between the groups could have been attributable to any number or combination of the above confounding factors. Therefore, the claim that the acoustic-tagged groups are representative of the Cle Elum and Dworshak hatchery stocks is unsupported due to these confounding factors caused by the study implementation.

In addition to the substantial differences in length at tagging, timing of release, and location of release between study fish and their corresponding hatchery populations of inference, Rechisky et al. (2013) compare acoustic-tagged Cle Elum and Dworshak hatchery Chinook salmon that were significantly different with regard to their length at tagging. Cle Elum hatchery Chinook salmon were significantly longer in 2006 (two-tailed t-test assuming unequal variances; $P = 6E-52$), but were significantly shorter in 2008 ($P = 8E-28$) and 2009 ($P = 0.005$). The data on length at tagging contradict the authors' claim that the Cle Elum and Dworshak hatchery groups were "size-matched" and add an additional layer of confounding to the results.

If one assumes that the large differences between the acoustic-tagged individuals and the Cle Elum and Dworshak hatchery populations are unimportant and not influential to study results, then those populations can therefore be used to assess differential handling and tagging effects of the acoustic tags used in this study versus conventional PIT-tag-based estimates. The Chinook salmon studied in Rechisky et al. (2013) were dual-tagged with both an acoustic tag and a PIT-tag, allowing for comparable survival calculations using the PIT-tag detection systems that are installed at the hydrosystem dams (Tuomikoski et al. 2012). The PIT-tag system technology also allows for calculation of SARs for PIT-tagged Chinook salmon, a key metric that could not be measured by solely relying on the acoustic tags employed in Rechisky et al. (2013).

Using the methods and results described in Tuomikoski et al. (2012), we calculated SARs from the point of hydrosystem entry (i.e., McNary Dam for Yakima River releases and Lower Granite Dam for Clearwater River releases) until adult return 1–3 years later at Bonneville Dam. This allowed for comparisons of SARs between the acoustic-tagged groups and the PIT-tag-only Cle Elum and Dworshak hatchery groups (Figure 1). Results show that for the acoustic-tagged fish, there were zero adult returns from the Yakima River releases in 2006, from the Clearwater River releases in 2006 and from the Yakima River releases in 2009. In four of the six stock-years evaluated, the SARs for the acoustic-tagged groups were substantially lower than SARs for the PIT-only hatchery groups, indicating that tagging and handling effects likely compromised the SAR estimates.

The SAR estimates also indicate that the magnitude of the tagging and handling effect appears to be different between the Yakima River releases and the Clearwater River releases. Across-years, the PIT-tag-only SARs were nearly equivalent to the acoustic-tag SARs for the Clearwater River releases, but were 11.9 times higher than the acoustic-tag SARs for the Yakima River releases. This observation of differential tagging effects between the two release locations is sufficient to explain the failure to detect differential delayed mortality.

The effects of the excessive tag burdens used in this study have been previously reviewed numerous times (FPC memos: March 13, 2009, November 13, 2008). Those reviews showed significant reductions in survival for acoustic-tagged fish relative to comparable PIT-tagged fish that were released in similar locations and at similar times. The detrimental effects of tagging and handling were particularly severe from releases in 2007 (Porter et al. 2009). Those data and results were not reported or even mentioned in Rechisky et al. (2013). In 2007, acoustic-tagged fish from Dworshak hatchery stock showed an estimated 92% mortality from release to below Bonneville Dam. Similarly, acoustic-tagged fish from Cle Elum hatchery stock showed an estimated 89% mortality from release to below Bonneville Dam. As a “proof-of-concept” study (Rechisky et al. 2013), we believe that it is somewhat misleading to withhold results from study implementation during one year of a four-year study.

Recent studies on the effects of excessive tag burden raise additional concern about the methods used by Rechisky et al. (2013). Field studies have been conducted on yearling spring Chinook salmon comparing the survival and migration rates of PIT-tagged and acoustic-tagged groups that were well controlled in terms of similar length at release, location of release, and timing of release (Wargo-Rub 2009, 2011). These studies found significant differences in survival, which increased as fish moved downriver. Significant differences in survival were observed for acoustic-tagged Chinook salmon with average tag burdens of 2.3%, well less than Rechisky et al. (2013) burdens of 4.4–9.4%.

Detection Arrays

The two ocean acoustic detection subarrays used to estimate the marine survival rates for the first month at sea for this study were located off shore of Willapa Bay, Washington, and Lippy Point, Vancouver Island. These two subarrays considerably differed in the number of receivers and the extent of their distance off shore. The Willapa Bay subarray had 40–45 receivers extending up to 36 km off shore, whereas the Lippy Point subarray had 24 receivers extending only 19 km off shore. The authors assumed that salmon migration was confined to the coastal zone spanned by these subarrays. However, for yearling Chinook salmon, Peterson et al. (2006) found yearling Chinook in trawl catches beyond 36 km off the Washington coast during June. Therefore, it appears sampling at the Willapa Bay and Lippy Point arrays are not equivalent, which would confound survival estimates made between these two locations.

The authors identified that estimation of detection probabilities for the Lippy Point subarray was not possible because of too few detections of tagged smolts at the distant Alaska subarray. This inability to estimate detection probabilities at Lippy Point puts into question the survival estimates used to draw conclusions concerning delayed mortality. In our view, the sensitivity analysis used to explore the effects of alternative assumptions is far too narrow given the true uncertainty about the actual detection probability of the Lippy Point subarray.

Emigration from Study Area

Rechisky et al. (2013) assume that all fish migrated north on the continental shelf at depths shallower than 200 m and through the Lippy Point subarray. If this assumption is not valid, the survival estimates will be biased low. The recent study by McMichael et al. (2011) comprehensively measured the direction and speed of acoustic-tagged yearling Chinook salmon as they entered the Columbia River plume with a 15 by 20 km “box” centered on the mouth of the Columbia River and extending to the 100 m depth contour (Figure 2). A total of 638 yearling Chinook salmon were detected on their array and only ~23% of the Chinook salmon were detected on the north boundary of the array. The majority of the detections (~58%) occurred on the western terminal array directly off the mouth of the Columbia River, and a substantial portion of the detections (~19%) occurred on the south boundary of the array. The median rate of emigration from leaving the mouth of the Columbia River until detection on the plume arrays was 47 km per day. At this rate of emigration, spring Chinook salmon migrating in a westerly direction would be expected to remain on the continental shelf for a little over a day. A study by Schreck et al. (2005) that deployed short arrays off the northern and southern jetties at the Columbia River mouth detected 12% of the tagged Chinook salmon on the southern jetty array. Further, Rechisky et al. (2012) detected two yearling Chinook on an array positioned 131 km south of the Columbia River mouth. This array was in place during 2009 only. Combined, these results suggest that some yearling Chinook salmon, and perhaps even a majority, may have migrated off the continental shelf and emigrated from the study area monitored by the subarrays.

As a result, the estimates reported in Rechisky et al. (2013) are likely biased low, although the degree of this bias is unknown.

Effects of Hydrosystem Development and Operations on Freshwater and Ocean Survival

In-river survival is not always 50% as reported. It varies between 25% and 83% and is influenced by hydrosystem conditions (Haeseker et al. 2012). Similarly, ocean survival rates, SARs, and overall life-cycle survival rates are influenced by hydrosystem conditions (Schaller and Petrosky 2007, Petrosky and Schaller 2010, Haeseker et al. 2012). In addition, this study was conducted for only three years and provided limited contrast in ocean and river conditions as compared to the studies that found support for considerable level of hydrosystem delayed mortality. Therefore, the conclusion that hydrosystem mitigation efforts may be ineffective is neither justified nor supportable. These studies spanned 9–60 years and analyzed over 600,000 PIT-tagged individuals.

Conclusions

During the first month's migration through the estuary and coastal ocean, Rechisky et al. (2013) found no evidence that Snake River hatchery Chinook smolts experienced lower survival rates than hatchery Chinook from the Yakima River (mid-Columbia River) that migrated through fewer dams. However, the authors acknowledge these estimates represented tagged groups whose size, holding, and timing of release had been manipulated to accommodate acoustic tags that were large relative to fish size. As a result, tagged fish were not representative of the hatchery populations of inference, confounding comparisons between the acoustic-tagged fish and the hatchery populations. Similarly, the size distribution of the hatchery study fish was larger than all but a small fraction of the wild individuals, concurrent with differences in migration timing between study fish and wild fish. The study was short term (three years), and the migration conditions that study fish experienced were different than migration conditions experienced by most wild and hatchery fish. Because there were very few numbers of fish detected at the northern ocean arrays, the detection efficiency of the Lippy Point array is unknown and the assumption about the extent of the distance of offshore migration is unsupported by data, the survival estimates for the first month at sea are highly questionable. Thus, their conclusions that "hydrosystem mitigation efforts may be ineffective if differential mortality rates for wild or hatchery fish develop in the ocean for reasons unrelated to dam passage" is unsupported.

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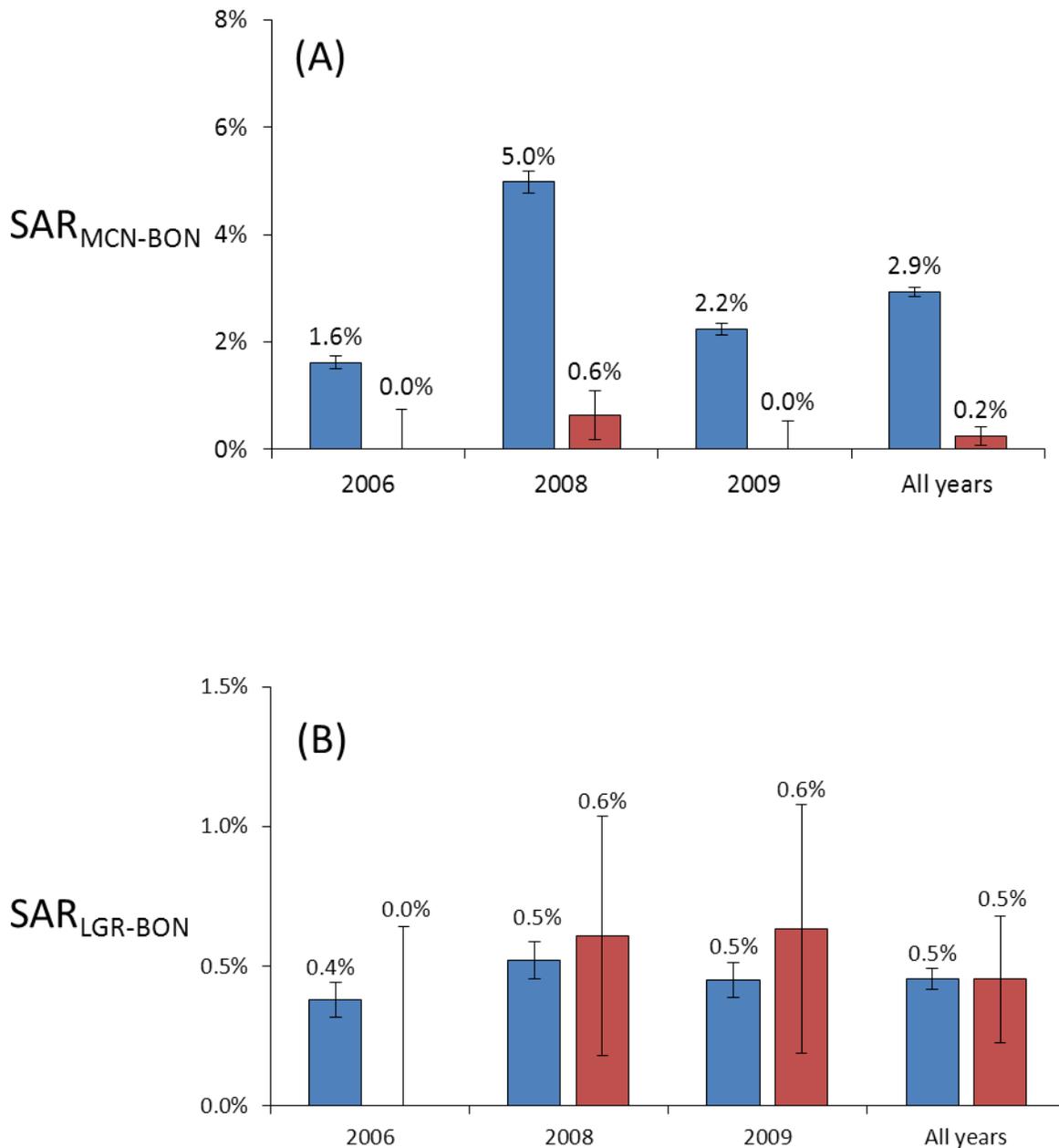


Figure 1. (A) Estimates of smolt-to-adult return (SAR) rates measured from McNary Dam to Bonneville Dam for Cle Elum hatchery Chinook salmon tagged using PIT-tags-only (blue bars) and tagged with both acoustic- and PIT-tags (red bars) during 2006, 2008, 2009, and across all years. (B) Estimates of smolt-to-adult return (SAR) rates measured from Lower Granite Dam to Bonneville Dam for in-river-migrating (C0) Dworshak hatchery Chinook salmon tagged using PIT-tags-only (blue bars) and tagged with both acoustic- and PIT-tags (red bars) during 2006, 2008, 2009, and across all years. For both panels, error bars represent one standard error and the SAR estimate is printed above the error bar.



Figure 2. Position of the Willapa Bay receiver array used by Rechisky et al. (2013) and the Columbia River plume network of arrays used by McMichael et al. (2011). The circles represent receiver positions and their diameters are scaled according to the percentage of detections that were recorded at each receiver. These percentages are printed next to each circle.