WASHINGTON STATE
Academy of Sciences
Science in the Service of Washington State

Scientific Review Panel Comments on "A review of hatchery reform science in Washington State"

The comments below represent all of the comments submitted by WSAS's Committee on the Science of Fish Hatcheries for WDFW's draft "A review of hatchery reform science in Washington State." Reviewers are identified as R1 through R8.[1]

General

R2 Comment: "This is a thoughtful, well researched and well written exposition on hatchery reform science as practiced in Washington State. The report provides an excellent summary and review of the last decade of scientific studies as they relate to hatchery reform and thoughtful observations of the successes and challenges of implementing large scale changes to Washington's extensive salmon and steelhead hatchery programs. The report should be extremely useful for continuing to improve Washington's hatchery programs in a scientifically sound way. The recommendations for development of a more comprehensive monitoring and adaptive management program are particularly timely. I have a few minor comments sprinkled throughout, but generally found the report to be outstanding and have no suggestions for major changes. I congratulate the authors on a job very well done!"

R4 Comment: "Please pass along this document with my compliments to the authors. I found this version to be great improved in content, organization, and readability. In particular I think the section that reviews the newest science in the past 10 years to likely be the most impactful along with their own suggestions for next steps. A focus on program size, while seemingly obvious, is an excellent issue to formalize and highlight and assessments of program size in the context of benefits/costs would be a major beneficial step in hatchery management. Please encourage the authors to contact me directly or through you with inquiries about my comments."

R5 Comment: "Thanks again for the opportunity to participate in this WSAS review. I have reviewed WDFW's revised full manuscript. Although I read the full report, I paid particular attention to the sections on hatchery benefits, adaptive management and the conclusions. I have very few remaining substantive comments or concerns about those sections (below). In particular, I found the conclusions section excellent – it provides a thoughtful, well-articulated vision of what staff feel are the major themes of the report, the major gaps in the existing research, and ideas for a path forward."

[1] This clarifying sentence was added on 11/25/19.

R6 Comments: "This revised report is a thoughtful and very impressive document. The authors have tackled a very complex and controversial topic in a systematic and objective way. In particular, I agree with all the major conclusions and recommendations in the last section. I have a few comments the authors might consider before the report is finalized.

1) This is perhaps a subtle point but important nonetheless: the terminology related to potential benefits and potential adverse effects of hatcheries is not symmetrical. The reality is that there are a number of good things (benefits) that hatcheries can provide (at least some of the time), and there are also a number of bad things that might happen, especially to natural populations. In both cases, although it is easy to enumerate the various possibilities, it is difficult to predict exactly which will come to pass for any given program. That is, in contemplating a new program, or a major change to an existing program, there is in general considerable uncertainty as to which potential benefits will be realized (and if so, to what degree), and the same is true for potential adverse effects. Therefore, a symmetrical way to refer to these collective possibilities would be to talk about potential benefits and potential adverse effects. Instead, the report talks about benefits and risks of hatcheries. Although in some cases "risk" is used just to refer to the probability that something bad will happen, in the current usage it refers to a combination of that probability, plus the adverse consequences that will occur if it does happen. So, in the terminology in the report, the contingent nature of the potential deleterious effects of hatcheries is captured in the term "risks", but the same is not true for benefits. Fortunately, there is a simple solution to this terminology problem: refer to "potential benefits" and "potential adverse effects." Or, alternatively, "potential benefits" and "risks." The current terminology implies that deleterious effects might or might not occur but benefits will occur.

The point regarding dual nature of risks is well taken, and in our previous draft we intended to describe both the likelihood and magnitude of risks where possible. We revised the opening paragraph of the risks section to more explicitly describe this goal. We also made a minor edit to the opening paragraph of the "benefits" stating that hatcheries "aim to provide" benefits, implying that the benefits are not necessarily realized in all circumstances. However, we find the term "potential benefits" too clunky for repeated use, and maintain that "benefits" section addresses both the likelihood and magnitude components where possible.

2) The report cites plenty of empirical examples to illustrate various points (e.g., examples of specific benefits or risks of hatcheries). However, in most cases the reader does not come away with a sense of how likely it is that the potential benefits or potential deleterious effects will be realized. For example, several examples are cited to the effect that hatcheries can help avoid extinction over at least several salmon generations. But how many other times has this been tried without success? The closest the report comes to this type of overall assessment is in discussing results of the meta-analyses by

Scheuerell et al. 2015 and Venditti et al. 2018 of effects on natural abundance. It would be very useful if comparable statements can be made regarding other topics.

As described above, we aimed to describe both the likelihood and magnitude aspects of risks and benefits of hatcheries where possible. The issue raised by the reviewer here might generally be described as the "file drawer effect" whereby scientists report negative results less frequently that positive results. This is a pervasive issue across science, not just hatcheries, and we do not think it necessary to mention it in our review.

3) In several places the report discusses risk-benefit tradeoffs, and Figure 2 illustrates an information-risk tradeoff related to geographic scale. [Note: the report uses both "tradeoff" and "trade-off" so best to pick one and be consistent.] However, I couldn't find any discussion of risk-risk tradeoffs, which are pervasive and essential to consider in trying to implement a hatchery program in as risk-averse a way as possible. The issue here is that it is not possible to simultaneously minimize all risks of hatcheries, because many of the risks are negatively correlated with respect to a given management action. Consider just two examples: broodstock collection and release strategies. a) Possibilities for domestication selection are reduced if juvenile offspring are released after only a short time in the hatchery; however, this reduces potential demographic benefits, and it also increases the likelihood of deleterious ecological interactions with wild fish. b) In many programs it is difficult to ensure that adults collected for broodstock are derived from the target population. Using very stringent criteria for broodstock selection can reduce the chances of mixing individuals from multiple populations, but at the risk of excluding some legitimate, local individuals. As discussed in Waples and Drake (2004), these risk-risk tradeoffs pervade every aspect of hatchery operations and are essential to factor into initial decisions about whether to start a program and if so, how best to implement it."

Good point. We added a paragraph at the outset of the conclusions section describing risk-risk trade-offs, including one of the examples suggested by the reviewer.

R8 Comment: This report is a very impressive piece of work, and the authors are very much to be commended for putting it together. I hope that they will see some value in the edits and comments and not feel, in any way, that I do not fully appreciate their efforts and their accomplishment. I list first some copy edits and other comparatively minor things, and then some broader issues. There may not be time (or agreement) enough to address the second set of issues but they came to my mind and so I share them.

Abstract

R4 Edit (p4, line 88): Replace "hatcheries, drawing upon examples" with "hatcheries, with strong emphasis upon examples."

Change made.

R2 Comment (p4, line 90): On "Hatchery benefits have received much less research attention than hatchery risks" R2 states "Over the last 10 years? I think if you look back over the last 100 years, there has been plenty of research attention paid to benefits. Even of the past 10 years, there are definitely examples of research papers focused on evaluating benefits."

We respectfully disagree with the reviewer's comment, and stand by our statement that hatchery risks have received much more research effort than hatchery benefits. We acknowledge that there are increasingly research papers addressing the conservation benefits of hatcheries, many of which are cited in our review. However, hatcheries are often justified according to economic, social, cultural and legal values and the literature on these topics relevant to hatcheries is thin. Our point here is to state the difference in volume of literature, not to say there is no work on hatchery benefits. No change made.

R1, R7 Edit (p4, line 97): Replace "naturally rivers" with "naturally in rivers."

Change made.

R4 Edit (p4, line 97): Replace "naturally rivers" with "naturally within rivers."

Change made.

VE Edit (p4, line 99): Replace "short term" with "short-term."

Change made.

R1 Comment (p5, lines 102-103): The statement "likely because key assumptions for hatchery effectiveness are rarely met" seems vague, R1 suggests "key objectives."

We added specificity, revising the phrase to read "…key assumptions regarding habitat carrying capacity and hatchery operational objectives (e.g., high proportion natural-origin broodstock) are rarely met."

R4 Edit (p5, line 102): Replace "sparse, likely" with "sparse – at least over the longterm, likely."

In our opinion, the reference to "longterm" is inappropriate in this case because one of the processes we reference (carrying capacity) operates over ecological not evolutionary time scales.

R7 Edit (p5, line 106): Add "through handling stress and injury" after "co-mingled natural populations."

The incidental mortality is not only due to post-release handling stress and injury in mark-selective fisheries, as direct mortality often occurs in non-selective mixed stock fisheries (see Figure 1). We feel this topic is adequately covered in the fishery risks section, and seek brevity in the abstract. No change made.

R4 Edit (p5, line 106): Replace "Constraints on implementing" with "Constraints impede implementation of fisheries."

"Constraints on" provides a more parallel, fluid sentence structure than "contraints impede" because all three phrases describing risks start with a noun followed by a preposition. No change made.

R7 Edit (p5, line 106): Add "(i.e., mark selective fisheries) after "remove only hatchery-origin fish."

We revised "fisheries" to "mark-selective fisheries."

R7 Comment (p5, line 106-109): Regarding the sentence "Constraints on implementing fisheries that remove only hatchery-origin fish, an asymmetry between lost harvest opportunity and the conservation gain of curtailing fisheries, and uncertainty in the harvest benchmarks due to the frequency of naturally spawning hatchery-origin fish contribute to fisheries risk. R7 recommends that "This sentence needs to be revised for clarity."

We revised the sentence to read: "We describe three factors that contribute to fisheries risks: constraints on implementing mark-selective fisheries that remove only hatchery-origin fish; an asymmetry between lost harvest opportunity and the conservation gain of curtailing fisheries; and uncertainty in the harvest benchmarks due to the frequency of naturally spawning hatchery-origin fish contribute to fisheries risk."

R4 Edit (p5, line 110-111): Replace "foraging resources and increasing" with "foraging resources both within freshwater and increasingly within the ocean environment and increasing."

The next paragraph of the abstract emphasizes the lack of information regarding ecological interactions in the marine environment. Here, we prefer a concise statement regarding the

primary risks associated with ecological interactions: competition and increased predation.

R3 Comment (p5, line 120): With respect to the word "prey" in the phrase "releases until after potential prey of hatchery-reared fish" R3 states "Do you mean predators? Also, some hatcheries time releases to periods when prey is most abundant."

No, we intended prey that are of conservation concern. For example, delaying steelhead smolt hatchery releases until after natural-origin subyearling Chinook salmon have migrated. We revised the phrase to read "delaying releases until after the migration of threatened species that hatchery-reared fish might consume as prey" to clarify.

R4 Edit (p6, line 131): Replace "thoughtful management" with "intentional management actions."

We respectfully prefer the phrase as written, no change made.

R1 Edit (p6, lines 131-139): R1 edits the section "However at…remains rare" as follows "However, at larger regional scales, some hatchery practices, including a legacy of intentional transfers of eggs and broodstock between watersheds, have contributed to genetic homogenization and reduced genetic diversity among populations. Studies comparing the number of offspring produced by hatchery-origin fish and natural-origin fish when both groups spawn in the wild (relative reproductive success, RRS) have demonstrated a general pattern of lower reproductive success of hatchery-origin fish. Domestication selection has been highlighted as a plausible cause of reduced reproductive success, although unequivocal, empirical, population-scale evidence for a genetic basis to fitness loss remains rare."

We agree this re-write is an improvement; change made.

R4 Comment (p6, lines 131-134): With regards to the sentence "However, at larger regional scales, hatcheries have contributed to genetic homogenization and reduced genetic diversity among populations, due at least in part to a legacy of intentional transfers of eggs and broodstock between watersheds," R4 "would like to see clear language that distinguishes loss of genetic diversity from domestication per se."

We feel the opening sentence of the paragraph makes this distinction clear. No change made.

R7 Comment (p6, lines 132-133): With regards to the phrase "reduced genetic diversity " R7 asks "Do you mean reduced genetic divergence?"

In our opinion, "reduced genetic diversity among populations" and "reduced genetic divergence among populations" are

synonymous. We prefer to keep the "reduced genetic diversity among populations" so that the phrasing in the abstract parallels the "among population genetic diversity" subsection later in the document. No change made.

R7 Comment (p6, lines 133-134): With regards to the "legacy of intentional transfers of eggs and broodstock between watersheds," R7 states "This practice has led to increased genetic 'diversity' in most cases, but has reduced genetic divergence/distinctiveness."

Please see response to previous comment, no change made.

R4 Comment (p6, lines 134-136): With regards to the sentence "Research on domestication has focused on studies of relative reproductive success (RRS) comparing the number of offspring produced by hatchery-origin fish and natural-origin fish when both groups spawn in the wild," R4 adds in the comments"And to lesser extent of production of wild fish spawned in hatchery (Christie et al)."

It is true that RRS research does, in some cases, address the productivity of natural-origin fish spawned in the hatchery. However, here in the abstract, we prefer a concise statement and tight focus on measurements of RRS by naturally spawning fish, as they provide a direct, clear connection to the issue of fitness loss.

R7 Edit (p6, lines 138-139): Delete ", though unequivocal, empirical, population-scale evidence for a genetic basis to fitness loss remains rare" and replace with "when attempting to spawn in natural environments, but their impact on the fitness of wild populations varies widely across hatchery programs."

The reviewer suggests stating that the magnitude of fitness impacts vary across populations when the empirical, population-scale RRS evidence for a genetic basis to fitness loss in Pacific salmon and steelhead is limited to two case studies. In our opinion, we should first focus on the evidence for heritable fitness loss before addressing its magnitude. No change made.

R4 Comment (p7, line 152): With regards the sentence ending in "fitness loss," R4 asks "Any ecological effect trade-off?"

It is not clear to us what ecological trade-off the reviewer is questioning. Trade-offs regarding fitness, pNOB and population demography are thoroughly addressed in the novel genetic modeling in the broodstock management section and associated appendices. No change made.

R4 Comment (p7, line 157-158): With regards to the statement "Hatchery reform is but one of several factors requiring careful planning and aggressive implementation needed to achieve meaningful recovery of salmon populations," R4 asks "Are the objectives for recovery clearly expressed and quantifiable?"

For the most part, yes. Recovery plans, which are required for any population segment listed as threatened or endangered under the U.S. Endangered Species Act, must include a statement of recovery goals. The specificity of these goals can vary among different geographic region. Regardless, an evaluation of recovery goals and recovery plans is beyond the scope of our paper. No change made.

R7 Comment (p7-8, lines 156-183): "Consider numbering these bullets as 1a,b,c; 2a,b,c,d; 3a,b,c,d"

We considered this suggestion but do not feel it necessary to refer to conclusions by number.

R1 Comment (p7, line 159): "This is a very important point and glad that it's highlighted here."

Thank you for the confirmation.

R8 Comment: In the Knowledge gaps (p. 8) I might emphasize that ecological processes in the marine environment are especially poorly understood, with respect to foraging, competition and other processes that might affect overall outcomes with multiple salmonid and non-salmonid species.

We agree that ecological interactions in the marine environment are especially poorly understood. We make this point earlier in the abstract, and further emphasize it in the linked section of the conclusions. Here we prefer a concise statement regarding lack of knowledge on ecological interactions. No change made.

Introduction

R3 Edit (p9, line 195): Replace "fish" with "anadromous stocks of fish."

We prefer "fish" in this instance in order to focus on the action of releasing fish rather than the concept of stocks or populations. No change made.

R4 Comment (p9, line 195): With regards the statement "Hatcheries typically (though not always) release fish into freshwater," R4 responds that "In terms of raw numbers this

wouldn't be true. Vast majority of 5 billion released hatchery fish in North Pacific are pinks and chum that are released into salt directly. Perhaps clarify to Outside of Alaska or Japan."

Good point. We added "In the western coterminous United States,…" to the beginning of the sentence.

R8 Edit: (p9, line 198): add annually

Change made.

R4 Comment (p9, line 203): Replace "as a tool in conserve or recover" with "as a conservation tool to recover."

Change made.

R7 Edit (p9, line 203): Replace "a tool in conserve" with "a tool to conserve."

Change made.

R8 Edit (p. 9, line 203): "to conserve."

Change made.

R7 Edit (p9, line 204): Replace "most" with "the vast majority of." Delete "either."

We feel the distinction between harvest and conservation goals for hatchery programs is accurate, and the term "most" allows for some flexibility. No change made.

R7 Edit (p9, line 205): Delete "or promoting" and replace with "but some also attempt to facilitate."

We feel the distinction between harvest and conservation goals is accurate. No change made.

R4 Comment (p10, lines 209-210): With regards to the phrase "reduction in adaptive evolutionary potential, and loss of population fitness," R4 recommends "Domestication should be stated here."

We added "through domestication" after "loss of population fitness."

R4 Comment (p10, lines 211-212): For the sentence "In the Pacific Northwest, where many populations are listed as threatened or endangered under the U.S. Endangered Species Act," R4 states "In final version it would be good to add teeth to this comment. What % of ESU or total # of ESUs or DPUs are listed in PNW?"

At this point in the very beginning of the manuscript, we prefer a brief statement regarding conservation status rather than a more lengthy discussion of listing status under the U.S. Endangered Species Act, and how Evolutionary Significant Units (ESUs) are defined. The focus of this paragraph is on defining hatchery reform. No change made.

R7 Edit (p10, line 213): Delete "balancing" and replace with "balance."

In this instance, we think "balancing" is appropriate. No change made.

R2 Comment (p10, lines 215-216): On the "principles and recommendations of" the HSRG, R2 asks "Should there be a citation to a document or website?"

We added a reference to the HSRG 2015 summary of hatchery reform recommendations.

R4 Edit (p10, line 219): Replace "decades of evolution" with "decades of amassed knowledge regarding."

We intend to describe the changes in hatchery practices, not the change in knowledge of them. No change made.

R7 Edit (p10, line 219): Delete "evolution" and replace with "changes."

We prefer the word "evolution" in this instance, implying the slow, steady nature of the changes in hatchery practices. No change made.

R7 Comment (p10, lines 222-223): With regards to the statement "The purpose of our paper is to review the science supporting hatchery reform as practiced in Washington State," R7 requests "Please clarify if this only includes WDFW hatchery programs."

The statement is accurate as written. Many of the examples we provide (Figure 1, Figure 5, Appendix 1, Tables 1 - 3) present hatchery production levels, outcomes or goals inclusive of (or relevant to) tribal and federal hatchery programs within Washington State. However, later in the document, where we specifically discuss only WDFW hatchery programs (Table 4, text of broodstock management, program size, and conclusions sections), we have revised text to ensure we explicitly state so.

R4 Edit (p11, lines 235 & 236): Replace "Washington" with "Washington State" in two places within sentence.

We feel the word "state" is unnecessary and the geographic reference is clear as written, especially given the previous sentence. No change made.

R4 Comment (p11, lines 235-236): With regards to the statement "Salmonid hatchery production in Washington State is among the highest in the world," R4 recommends "Again would be good to put this in context by region and species. In terms of chinook, coho, and steelhead this is totally right."

Good point, we qualified the statement by stating that "Hatchery production of Chinook salmon, coho salmon and steelhead trout in Washington is among the highest in the world."

Benefits of Hatcheries

R8 Comment: Under Harvest benefits (p. 12) one might note that the largest runs in the state are, I believe, pink salmon that are essentially all wild, and there are many very substantial wild chum runs as well. It is really coho, Chinook, and steelhead that are primarily hatchery-supplemented, yes?

The reviewer's statement that pink salmon runs are nearly entirely wild is accurate. However, our treatment of this topic acknowledges exceptions, and we prefer to focus on the general pattern rather than the exception. Furthermore, there is substantial chum salmon hatchery production in Washington State, notably subsidizing a commercial fishery in Hood Canal. No change made.

R3 Edit (p12, line 257): Insert "component of only a" after the words "is a" and before the phrase "small minority."

Good suggestion, change made.

R4 Comment (p12, lines 249-257): In response to opening paragraph of the benefits of hatcheries, R4 states "The extent to which the goals of the hatchery influence the program size and processes are what are important. I would like to see a comment in the opening that acknowledges that having a different goal or objective per se does not change the benefits or risks. Only through different processes does this matter."

We agree that hatchery operations, not goals per se, will determine hatchery risks and benefits. Indeed, the entire hatchery reform section, which we consider the core of the paper, addresses the operational specifics of hatcheries and

their impact on risks. We further emphasize the importance of ensuring consistency between goals and hatchery performance metrics in the conclusion. In our opinion, this is a complex, layered discussion that requires presentation broodstock management concepts (PNI, pHOS, pNOB) and the HSRG goal system of population designations and recovery phases. Here, in the benefits section, such topics have not yet been introduced and we prefer to remain tightly focused on describing hatchery benefits. No change made.

R5 Comment (p13-15): "There are many economic estimates presented, but it is not clear how inflation was handled. Are these all in 2019 dollars, or are they in dollars current when those studies were published? If they have all already been converted into a common currency, then the report could make that clear. If they have not, this could be done relatively easily using the Consumer Price Index (most people use the "Urban" index). But since there is really no attempt to sum these numbers or compare them with each other, a minimum approach would be to at least note that these dollar amounts should be compared with care since they were not inflated to common dollars."

We added a sentence stating that economic dollar values are provided as stated in the relevant reports and were not converted to common dollars.

R8 Comment (line 274 and vicinity): Since some of the benefits of hatchery programs are put in economic terms, mixed, of course, with social and cultural benefits, might we also ask how much they cost in dollars, as well as the ecological and genetic (etc.) risks discussed later? What is the direct outlay for hatchery operations by the state, federal, and tribal facilities? Without even considering the value of the land and the facility construction, it is likely substantial, perhaps?

This is a valid question, but was beyond the scope of our review. As we point out in the last paragraph of economic benefits section, hatchery cost-benefit analyses accounting for the expenses of fish production and biological risks have rarely been attempted. We think there is a strong need for this type of research to better assess the risk-benefit trade-off, a point we emphasize in the opening paragraph of the conclusions section.

R8 Comment (line 274 and vicinity): Given the very small economic benefits of non-tribal commercial fisheries, the large benefits associated with recreational fisheries (that can, realistically, only catch substantial numbers of some species but not others, and value some species more than others), one wonders whether the allocation process might be re-examined but perhaps this is more a matter of fishery management than hatchery reform. Still, they are closely linked.

We view any re-allocation of harvest among non-treaty commercial and recreational fisheries as a consideration or task for the Washington Fish and Wildlife Commission and fishery managers. In our paper, we aim to provide the information but avoid comparative value judgments among fisheries. No change made.

R4 Comment (p13, line 274): Under the "Economic benefits" heading, R4 suggests the authors "Might consider additional subheadings for commercial/sport (or recreational)."

The economic benefits section is rather short (3 paragraphs), so we feel that additional subheadings are unnecessary.

R2 Comment (p13, line 275): On the 2006 paper, R2 comments "If this is the most recent economic analysis available, might want to note that. NMFS reports commercial fishery revenue by state through 2017 (https://foss.nmfs.noaa.gov/apexfoss/f?p=215:200:3427515592799::NO:::), but I'm not sure how those numbers are comparable or useful for this analysis or not."

We acknowledge that the economic values reported in our review are out of date, but they were the best available information. Thank for you suggesting the NOAA database, which provides the value of landings (i.e., ex-vessel value) by state and species. The values we report also include seafood processing costs, and secondary, indirect benefits to local economies, which we consider a more comprehensive assessment. We had, in fact, previously reviewed NOAA Fisheries' series of reports on fishery economics but thought the information too coarse for our review (e.g., statewide recreational fishing value not broken out by species).

We further acknowledge that there is a strong need for more research on the economics of salmon fisheries supported by hatcheries, which we state at the beginning of the conclusions section. We also acknowledge in the abstract that the economic, social and cultural benefits of hatcheries are reviewed briefly but are not the focus of our paper. Writing this review has sparked our interest in further research on hatchery economics, and if we pursue a follow up paper, expect that the NOAA landings database would be a valuable resource.

R5 Comment (p14): "I appreciate the responsiveness to earlier comments about BCAs being widely used elsewhere in environmental policy. Instead of Krupnick and Morgenstern, I would just cite the most recent BCA of the CAA (the "Second Prospective Study"), which can be found here: https://www.epa.gov/clean-air-act-overview/benefits-and-costs-clean-air-act. For a second and more relevant example, the federal government has required BCA for water related projects (water resources, flooding, navigation, etc) involving federal funding since the 1970's. This was codified into the "Principles and

Guidelines" in 1983, which were amended under President Obama in 2013 to be the "Principles, Requirements and Guidelines". They are sometimes referred to as "four accounts" analysis. The archived Obama website has a lot of relevant links, and there was a lot of ink spilled about this circa 2011-2014, but you could just the read splash page for the main gist. Final notice in the Federal Register here."

We replaced the Krupnick and Morgenstern 2002 reference with the U.S. EPA 2011 Clean Air Act cost-benefit report suggested by the reviewer. Thank you for the information on water related projects, we find this an interesting field of study.

R4 Comment (p14, line 300): R4 states at the end of the paragraph, highlighting "(e.g., Krupnick and Morgenstern 2002)", that it "Would be useful to acknowledge the implicit and explicit subsidies provided to hatcheries and how including that might change the calculus."

We view the issue of subsidies (i.e., cost to government of producing hatchery-reared fish) as a component of the larger cost-benefit analysis concept presented in this paragraph. Rather than present a partial picture of an issue that clearly merits more research, we prefer to concisely highlight the lack of information on this topic. No change made.

R2 Comment (p14, line 306): On the "Washington State, USFWS (2014)" study R2 asks "Would WDFW have even better data on this from e.g. fishing license sales information? Seems odd that you are citing a USFWS report when WDFW is the state's primary fish management agency."

We have added a short statement providing license sales information in the form of catch cards issued for salmon, steelhead, halibut and sturgeon. This information does not isolate anglers for salmon and steelhead but provides a nice complement to the UWFWS survey data.

R3 Edit (p15, line 325): Replace "study estimates" with "study to estimate."

Change made.

R2 Comment (p16, line 345): On the word "immeasurable," R2 asks "Is this literally true? Or just synonymous with 'high'?"

We feel the word "immeasurable" is appropriate in this instance. Despite some searching, we were unable to find social science research quantifying the cultural value of salmon to Native American Indian tribes. Furthermore, given the brevity, importance, and gravity of this section, we feel that strong

language is justified. "High social and cultural value" would not adequately articulate the issue. No change made.

R2, R8 Edit (p17, line 376): Delete "Hathchery" and replace with "Hatchery."

Change made.

R2 Edit (p18, line 387): Delete "vacant."

Change made.

R6 Comment (p18, lines 395-397): Regarding Redfish Lake, R6 states that the authors "could add that in addition to maintain the population for almost 3 decades, the captive broodstock program caused relatively little inbreeding, in addition to that caused by the drastic bottleneck itself (Kalinowski et al. 2011)."

We added "with relatively little inbreeding attributable to captive breeding (Kalinowski et al. 2012)" to the end of the sentence.

R4 Comment (p18, line 397): Highlighting (Kline and Flagg 2014), R4 states "I think also a good example of assessing success over short vs. long term. Short term rescue for sure…long term rescue without massive continued intervention (seems unlikely)"

Perhaps so, but we wish to focus on what the hatchery has achieved, not speculate on what level of population recovery may or may not be achieved in the future. No change made.

R3 Comment (p19, lines 399-403): With regards to the statement "Hatchery managers avoided non-local releases in perpetuating Elwha River Chinook salmon following construction of dams that blocked the vast majority of habitat (Brannon and Hershberger 1984), and the population currently represents a unique genetic lineage (Ruckelshaus et al. 2006)." R3 states "This sounds more like the benefit of NOT using a traditional hatchery approach."

Sure, the reviewer is correct that avoiding inter-basin transfers helped maintain the genetic integrity of the Elwha Chinook population. However, in this section, we are not addressing the value or outcomes of different hatchery management strategies, which are described in the hatchery reform section. Rather, we are describing the conservation objectives that research has demonstrated hatcheries can achieve. No change made.

R4 Comment (p19, line 409): R4 highlights "First, summer chum salmon were" and asks "Hood Canal?"

We added "(eastern Strait of Juan de Fuca)" after "Chimacum Creek" to specify the geographic location of the study site.

R4 Edit (p19, line 416): Replace "species, supplementation" with "species, and supplementation."

Change made.

R7 Edit (p19, line 416): Replace "species, supplementation is ongoing (Galbreath et al. 2014)" with a correction and an addition as follows "species, and supplementation is ongoing (Galbreath et al. 2014) with some evidence for local adaptation of naturalized broodstock (Campbell et al. 2017)."

Good suggestion, we added the phrase "with some evidence for adaptive evolution of hatchery broodstock (Campbell et al. 2017)."

R4 Edit (p20, line 421): Add ", revealing the importance of understanding limiting factors" after the word "carrying capacity."

We feel the sentence is adequately descriptive as written, and wish to avoid the term "limiting factors" because it often implies the specific biological processes determining carrying capacity (e.g., rearing space, food, predators, etc.) are known with precision. No change made.

R7 Edit (p20, line 423): Insert the following sentence "Waters et al. (2015) also demonstrated that a line of integrated broodstock has prevented divergence from the originating wild population of spring Chinook samlon in the Yakima River," after the sentence ending in the words "stable natural-origin returns."

The Waters 2015 paper primarily describes among population genetic diversity patterns according to hatchery broodstock management. We discuss its results and implications for hatchery management in the appropriate places of the "genetic risks," "broodstock management," and "emerging science" sections. However, the point we make here in the "conservation benefits" section is that the Yakima spring Chinook hatchery program increased the abundance of naturally spawning salmon, and Fast et al. 2015 provide a more thorough assessment of abundance data than Waters 2015. No change made.

R8 Edit (p. 20, line 425): Add "near the facility" or something like that, though in some cases such as Cedar River Chinook salmon, they spawn naturally some distance away, as there is no hatchery on that river.

We respectfully disagree with the reviewer that hatchery-origin salmon only spawn naturally near the hatchery facility. In our experience, this is not necessarily the case, and the reviewer's own comments appear to acknowledge as much. Indeed, this passage references Appendix 1, which shows two Puget Sound rivers without Chinook salmon hatchery releases that typically experience pHOS ≥ 20%. No change made.

R7 Edit (p20, line 427): Replace "numericallly" with "numerically"

Change made.

R4 Edit (p21, line 448): R4 highlights "density dependent processes" and states "Again could reiterate the need for limiting factor analyses."

As stated previously, we do not think it necessary to refer to limiting factors analyses. No change made.

R3 Edit (p21, line 457): Replace "to supplementation" with "to when supplementation."

Change made.

R4 Edit (p21, line 458-459): Replace "from naturally produced redds" with "Natural origin spawners."

In this case, we intend naturally produced redds (i.e., spawning occurring in the river), regardless of whether the spawners were hatchery-origin or natural-origin. No change made.

R7 Edit (p22, line 476): Add the sentence "However, it was notable that all supplementation programs in these studies had low PNI," after the sentence ending with "(Venditti et al. 2018)."

Although Venditti et al. (2018) refer to a shortage of natural-origin broodstock in one of their two study basins (Clearwater) during phase one of their study, they do not report PNI. We therefore do not feel justified in adding the suggested sentence. However, the point is well taken that pNOB and PNI are crucial metrics for evaluating hatchery program performance. We added a sentence to the Emerging Science section, after pNOB and PNI have been introduced and described, stating that neither Scheuerell et al. 2015 nor Venditti et al. 2018 reported pNOB and PNI, so the extent to which the hatchery programs under investigation utilized hatchery reform principles is unclear.

R8 Comment (p22, line 477 and vicinity): Mention might be made of the Cedar River sockeye salmon hatchery, which was greatly expanded to stabilize and rehabilitate the run, without loss of the wild population. Owing to factors external to the hatchery, the wild and hatchery runs have diminished greatly.

We acknowledge the relevance of the Cedar River sockeye story, but in this section, we wish to focus on hatchery effectiveness studies for which we a reference a thorough agency report or paper examining conservation outcomes. As with many hatchery programs in Washington State, description of the Cedar River sockeye hatchery would require novel presentation and analysis of unpublished data and information to substantiate any inferences regarding conservation effectiveness. This was beyond the scope of our review. No change made.

R7 Edit (p22, line 478):Replace "muliple" with "multiple."

Change made.

R2 Edit (p22, line 480): Delete "vacant" and replace with "inaccessible."

In our opinion, reintroductions are not limited to cases of migration barriers, but also include accessible areas with little to no natural spawning. Chimacum Creek summer chum salmon is a good example. However, we recognize that the tone of "vacant" implied biologically empty, which is not necessarily the case. We revised "vacant" to "unoccupied."

R2 Comment (p22, lines 482-483): On "Chimacum Creek summer chum salmon" R2 asks "Was this reported in one of the studies cited above? If, would be good to provide the reference (or new reference if not)."

Yes, references for the Chimacum Creek summer chum example are provided earlier in this section. However, this sentence focuses on the *absence of other successful reintroduction examples* in which natural reproduction was sustained for multiple generations following termination of hatchery releases. The Chimacum Creek references are not appropriate for that statement. No change made.

R2 Edit (p22, line 485): Delete "extermely" and replace with "extremely."

Change made.

R7 Edit: (p22, line 486): Add the sentence "However, broad implementation of supplementation programs with high PNI have not been implemented to allow wide

evaluation of this approach to recovery," after the sentence ending in "(Scheuerell et al. 2015; Venditti et al. 2018)."

At this point in the manuscript, the hatchery reform concept of PNI has not yet been introduced. However, we added a sentence to the Emerging Science section stating that neither Scheuerell et al. 2015 nor Venditti et al. 2018 reported pNOB or PNI, so the extent to which the hatchery programs under investigation utilized hatchery reform principles is unclear.

R4 Edit (p23, line 489): Replace "(e.g., degraded habitat)" with"(e.g., degraded habitat, connectivity)."

We replaced "degraded habitat" with "degraded or inaccessible habitat."

R8 Comment (p 23, line 489): I would add conditions in marine waters and also, at least in some cases, excessive fishing, to habitat degradation.

Sure, marine survival and overfishing certainly are two of many factors that have contributed to salmon declines. However, in this passage, we are not seeking a list or review of these factors, but rather a simple, concise example of a constraint on conservation effectiveness. Furthermore, we aim to make a connection to habitat carrying capacity (see subsequent sentence), and think that focusing the reader's attention on habitat quality and quantity best serves this purpose. No change made.

R4 Comment (p23, line 493): With regards to (Liermann and Hilborn 2001) R4 states "Overall I think the evidence that depensation or Allee effects are common in salmonids is very weak."

OK, we respectfully acknowledge this perspective. However, in this case, we only briefly introduce the concept of depensation, and make no statements regarding its frequency or magnitude. We feel the tone of the sentence (use of word "might") is appropriate. No change made.

R7 Edit (p23, line 497): Add "or high pNOB," after the words "e.g., exclusively natural-origin broodstock."

Good point, though at this stage of the manuscript the definition of pNOB has not yet been introduced. We revised the statement to read "e.g., exclusively or high percentage of natural-origin broodstock").

R4 Edit (p24, line 510): Replace “provide prey” with “provide additional prey.”

Change made.

R2, R4, R7, R8 Edit (p24, line 518): Delete “sectioon” and replace with “section.”

Change made.

R1 Comment (p24, lines 519-520): The statement “marine rearing habitats have unlimited capacity to support additional salmon” “is illogical and incorrect. However, it would be correct to say that an implicit assumption is that there is some capacity to support additional salmon.”

Good point, we replaced “unlimited” with “sufficient.”

R2 Edit (p24, line 520): Delete “unlimited” and insert “the” prior to the words “additional salmon.”

We replaced “unlimited” with “sufficient.”

R4 Comment (p24, line 520-521): With regards to “hatchery stocks identified for increased production are available as prey to killer whales in time and space,” R4 states “Hopefully you will circle back to this and highlight the life history diversity aspects of prey availability. Increasing abundance and availability are often quite separate.”

This is a valid point, salmon life history diversity is certainly important to their role as prey for killer whales. However, a thorough treatment of the topic would require at least a brief review of killer whale biology (e.g., size selective predation, seasonal timing of greatest need for additional prey). We prefer a brief statement regarding killer whale prey rather than a more extended review. Note, however, we do return to the theme of salmon life history diversity in the conclusions section, emphasizing the need for more research on the connection between salmon diversity and ecosystem stability.

R4 Edit (p24, line 521): Replace “accessible” with “available.”

We prefer the word “accessible” in this instance. No change made.

R8 Comment (p24, line 521): The report might be more explicit here in stating that other predators (e.g., harbor seals) might benefit rather than killer whales from any surplus

Chinook salmon production that could be accomplished. This would also, presumably, necessitate no increase or even a reduction in fishing take.

Sure, increased prey for harbor seals is certainly a potential outcome of the proposed increased hatchery production for killer whale prey. However, as noted above, we wish to avoid a more detailed examination of the killer whale hatchery initiatives as they are not the focus of our more general review. Note that we point out the short and long term risks of increased predation in the ecological risks section. No change made.

R1 Edit (p24, line 521): delete "in time and space."

We feel it important to briefly point out the spatial and temporal components of killer whale prey availability. No change made.

R4 Comment (p24, line 527): Highlighting the sentence that ends in "resources," R4 states "And implicitly that salmon come from hatcheries and that it is our job to care for salmon in captivity and then release them back into the wild when we deem it ready."

We added a sentence making this point, as suggested by reviewer 4 below.

R8 Edit (p24, line 529): "than"

Change made.

R4 Edit (p24, line 530): R4 suggests the paragraph end with the additional sentence "That being said, education programs must be aware of the implicit lessons that may be perceived from children about the role of people vs. nature in producing salmon" and comments on this addition that "This came up recently in an invasive species meeting here in AK. A colleague at ADFG was lamenting that there was not enough enforcement to bust folks that dump fish out of buckets and there was some evidence that recent introductions came from children (who perhaps didn't have the heart to flush the gold fish). At the same time ADFG is promoting children to dump fish (albeit a different fish) out of buckets as part of salmon in the classroom."

Good suggestion, we added a sentence to that effect.

Risk of Hatcheries

R7 Edit (p25, line 535): Insert a comma after the word "Thus."

Change made.

R8 Edit (p25, line 541): The wild populations tend to be less productive as well as less abundant, and that is more important, really, I think.

Good point, we revised to indicate wild populations are less abundant and less productive than hatchery populations.

R4 Comment (p26, line 555): After the word "unmarked" in line 555, R4 inserts in (natural origin?), seeking clarification.

In most cases, yes – unmarked Chinook and coho salmon are natural-origin. However, there are a few hatcheries that avoid adipose marks to reduce exposure to mark-selective fisheries (see mass marking section) and in some cases clipping error leads to mark rates less than 100%. Mass marking is generally widespread in Washington State (see Table 1). We refer to unmarked fish as "presumably natural-origin" in the second sentence of this paragraph and feel that is sufficient for the purpose of describing the nature of the fishery risks. No change made.

R4 Comment (p26, line 563): At the end of the paragraph, R4 highlights the word "populations" and states "A quick review of the rates of marking is useful here too. Only if there is consistent 100% marking does this seem viable. My understand (at least through about 2009) in the Columbia there as huge variation in proportion of releases marked among programs."

We emphasize the importance of "mass marking all fish released from a hatchery" for implementation of mark-selective fisheries in the "mass marking" sub-section of the "hatchery reform" section. We acknowledge that a review of marking rates would better describe levels of fishery risks and would be an interesting follow up study. However, this would not be a concise exercise, as there are 159 hatchery programs operated by WDFW alone, and thus it was beyond the scope of our review.

R7 Edit (p26, lines 559-560): Insert "and post-release mortality has not been quantified for most mark-selective fisheries," after the words "not all unmarked fish that are released survive."

We added the following sentence "Further research on the manner in which fishery management (e.g., gear type, time and area openings) affects these metrics would improve estimates of incidental mortality in mark-selective fisheries".

R8 Comment (p25, line 568 and vicinity): Hmmm. Does this not imply that there is already, in some sense, a surplus of hatchery Chinook salmon swimming around? If so, then why the

pressure to make more of them for killer whales and fishermen? Won't that only make things worse for the wild runs, and no better for the killer whales?

These questions are worthy of discussion, but we also feel that addressing the political climate surrounding proposals for increased hatchery production to benefit killer whales is outside the bounds of our paper. Throughout the report, we aim to raise the relevant scientific issues, but avoid passing judgment on any particular hatchery program or specific hatchery proposal.

R7 Edit (p26, line 571): Replace "restricting" with "restrict."

Change made.

R4 Comment (p27, line 576): At the end of the paragraph, R4 highlights the word "populations" and states "Also leads to the conundrum of protecting the wild fish through fisheries by not catching as many of the hatchery fish, BUT by doing so increase the potential interactions on the spawning grounds."

Sure, this is a good point, an example of the risk-risk trade-off suggested by Reviewer 6's opening comments. We chose to describe this situation as an example of risk-risk trade-offs in the conclusions section, after we have described the genetic risks of interactions on the spawning grounds. Here in the fishery risks section, we aim to simply point out that the demographic imbalance created by large scale hatchery production introduces challenging fishery management decisions, as we have not yet introduced the topics of genetic risks and pHOS.

R4 Comment (p27, line 585): For the sentence ending in "natural-origin salmon," R4 recommends it would be "Worth citing Rachel Johnson work on this too in Sacramento."

We added a reference to Johnson et al. 2012 (PLoS One) addressing uncertainty in natural population productivity introduced by the presence of naturally spawning hatchery-origin fish.

R1 Comment (p29, lines 622-633): With regards to the section "Hatchery propagation…Lairike 1991)," R1 asks "Is this all true if the hatchery population contributes to increased overall population abundance (hatchery + natural), or contributes to the majority of salmon within a watershed? For example is the effective size of Nooksack Spring Chinook population smaller now than it was before the captive broodstock program was initiated, or is it larger now if Ne has increased with N?"

Ne does not necessarily increase with increasing abundance. If the hatchery program samples the natural population, and that subset is amplified through increased survival such that the proportion of the total population that is hatchery-produced is more than it would have been otherwise, all else equal, Ne will go down and inbreeding will go up. Its possible Ne would increase with increasing census abundance but necessarily so.

We do not have specific knowledge regarding Ne of the SF Nooksack spring Chinook salmon before vs. after the captive broodstock program. However, the captive broodstock program definitely amplified a subset of the population – those few fish that genetically appeared to be SF Nooksack springs – which is essentially the Ryman-Laikre in action.

No change made. At this point in the manuscript, we aim to provide a concise statement of the risks, and avoid an extended discussion of possible scenarios regarding increasing vs. descreasing Ne and population size. We return to the topic of genetic diversity and the ability of hatchery programs to maintain it in the emerging science section, after we have introduced the fundamental concepts of hatchery reform and broodstock management.

R8 Comment (p29, line 630 and vicinity): The variance in reproductive success of naturally spawning salmon is very great, as shown by many studies including those by the authors. In a hatchery, if all females get their eggs protected (in trays, etc.) the variance in female RS should be less than in the river. In the old days only a few males were spawned, and I know all about sperm competition when milt is mixed, but it still seems that inbreeding might be less in a hatchery than a river. Is this not something one might look for? With parentage analyses, one could determine how often hatchery staff inadvertently spawn siblings. Given the heritability of return date, this might occur, but would in the river too, right?

A key aspect of the Ryman-Laikre effect is that the entire hatchery plus natural aggregate population experiences an increase in variance in reproductive success, not just the hatchery component or just the natural component. Furthermore, we note that inbreeding is defined more broadly than suggested by the reviewer, reflecting mating among any relatives, not just siblings. Such matings would become more frequent under the Ryman Laikre effect. Although the reviewer suggests an interesting line of research comparing variance in reproductive success between naturally spawning and hatchery-spawned salmon would be interesting, we maintain that we have defined the Ryman-Laikre effect accurately. No change made.

Likewise, for the section on genetic diversity. It is not true that hatcheries experience no gene flow. In many cases naturally produced fish come in and get spawned, or at least this was a very common practice when only a fraction of the hatchery fish was marked. In addition, if the hatchery reduces variance in RS (e.g., by protecting all eggs, etc.) then might it not have the opposite effect, at least with respect to neutral genetic variation?

Some hatcheries certainly experience geneflow, depending on their management. Indeed, intentional spawning of natural-origin fish is a component of the integrated broodstock management, as detailed in the hatchery reform section. Here we simply explain the genetic risks of hatcheries, and use the hatchery reform and emerging science sections to describe the ability of hatchery management to reduce the risks, including several examples in which hatcheries have maintained genetic diversity, as suggested by the reviewer. No change made.

R7 Edit (p30, line 647): Add "; Waters et al. 2015" after Ford et al. 2016 in the reference "(e.g., Dickerson et al. 2002; Seamons et al. 2007; Williamson et al. 2010; Ford et al. 2016)."

Here, we cite reproductive success papers demonstrating that not all adults contribute to the next generation. Waters et al. 2015 showed that diversity dropped in the segregated line, but associated that trend with genetic drift, and not specifically to variability in reproductive success. Thus, we do not think Waters et al. 2015 is an appropriate addition. No change made.

R2 Comment (p30, line 654): On "Empirical studies," R2 states that the authors "Might also consider citing Van Doornik et al. 2011 which compared diversity in hatchery, supplemented, and non-supplemented Chinook salmon populations over time and found not much difference between the three types.
https://afspubs.onlinelibrary.wiley.com/doi/full/10.1080/02755947.2011.562443
Waters's et al. RAD-seq study on the Cle Elum hatchery also seems important to mention as it looked directly at genomic diversity over time doi:10.1111/eva.12331."

Thank you for suggesting the Van Doornik 2011 reference, we added a reference to it making the point that not all supplemented populations have shown evidence for reduced genetic diversity.

We added a reference to the Waters et al 2015 study at the end of the "Empirical studies…" sentence to support the statement that the likelihood and magnitude of hatchery impacts on population diversity vary greatly according to hatchery program management. Note this study is also described in the "emerging science" section as evidence that integrated broodstock management can limit genetic divergence between a hatchery stock and its associated natural population.

R4 Comment (p31, line 663-664): For the reference (Greene et al. 2010; Schindler et al. 2010; Braun et al. 2016), R4 recommends "Schindler et al. 2015 Portfolio concept in Ecology is a better citation here."

We added the Schindler 2015 citation, but also wish to keep the other three because they are more specific to salmon.

R7 Edit (p31, line 668): Add "in fish that may be locally adapted" after the words "the break-up of co-adapted gene complexes."

We added "that may be locally adapted" as suggested.

R7, R8 Edit (p31, line 672): Replace "locals stocks" with "local stocks."

Change made.

R7 Edit (p32, line 691): Add a period at end of the sentence.

Change made.

R2 Comment (p32, line 693): Under the subsection on domestication, R2 suggests that "The Waters's et al papers on the Cle Elum hatchery should be cited somewhere in here."

The focus of the Waters et al. 2015 paper is a comparison of integrated vs. segregated broodstock management effects on genetic divergence between a hatchery and natural population. The Waters et al. 2015 paper is thoroughly discussed in that context, but domestication selection is not a major theme of the paper. No change made.

R7 Comment (p32, lines 696-697): With regards to the references listed, R7 states "The Benjamin et al reference is not highly relevant here since the study is on delta smelt."

We removed the Benjamin 2018 reference, in line with our focus on Pacific salmon and steelhead.

R8 Comment (p32, line 700): The exception being spawning timing, where in some cases there has been strong and deliberate adjustment relative to the local wild or source population.

Good point, though the intentional selection on spawn timing was likely accompanied by unintentional domestication selection on other traits. We add sentence making this point.

In this section, perhaps there would be value in mentioning, briefly, some traits for which there are known or plausible genetic bases that might shift in a hatchery after inadvertent selection? Aggression, feeding, predator avoidance, redd site selection, etc. That might be easier for people to grasp than epigenetics, important though they can be.

Good point, we added a sentence to the opening paragraph of the domestication risks section stating that many salmon traits are known to have a genetic basis, providing ample scope for inadvertent selection in the hatchery.

R7 Edit (p33, line 711): Insert "However, studies of RRS in Chinook salmon and steelhead have revealed that high PNI can be effective to limit fitness effects to the wild population when hatchery origin fish mate with wild fish (Hess et al. 2012; Ford et al. 2016; Janowitz-Koch et al. 2019)," after the sentence ending in the words "spawning in the wild (e.g., Araki et al. 2007; Williamson et al. 2010; Thériault et al. 2011; Ford et al. 2016; Janowitz-Koch et al. 2019)."

The connection between PNI and fitness effects is certainly an important point, but at this stage of the manuscript, the concepts of hatchery reform, broodstock management, and PNI have not yet been introduced. In the emerging science section, we directly state the inference that high PNI can limit negative fitness effects, referring to Janowitz-Koch 2019 and Ford 2016 studies, as suggested by the reviewer. No change made to the genetic risks setion.

R1 Comment (p33, lines 711-712): With regards to the sentence "In some but not all RRS studies, researchers have confirmed that the lower fitness of hatchery-origin fish has a genetic basis (Araki et al. 2008; Christie et al. 2014a)," R1 states "There may be a better way to state this. Araki et al. 2009 concluded heritable fitness loss. No others have. Ford et al. 2016, is quite consistent with Araki, et al. 2009, and should be mentioned, although with some potential for other non-genetic effects. To the uninformed, stating it this way and citing two additional studies that both refer the Hood River study, might be unintentionally misleading."

Good point. We revised the sentence to read "Two RRS studies have provided evidence that the lower fitness of hatchery-origin fish has a genetic basis (Araki et al. 2008; Ford et al. 2016), whereas two others with an appropriate study design did not (Thériault et al. 2010; Ford et al. 2012)." Furthermore, in the revised manuscript, we go into greater detail regarding the evidence for a genetic basis to fitness loss in the emerging science section, specifically stating that only the Araki Hood River and Ford Wenatchee River studies demonstrate heritable fitness loss.

R3 Comment (p33-34, lines 724-732): With respect to "An emerging…unanswered question" R3 responds "Good addition."

Thank you for the confirmation.

R7 Edit (p33, line 720): Replace the phrase "A great deal of recent research effort has been devoted" with "Recent research effort has also been devoted."

Change made.

R8 Comment (p35, line 762): But see, for example,

Nickelson, T. E., M. F. Solazzi, and S. L. Johnson. 1986. Use of hatchery coho salmon (*Oncorhynchus kisutch*) presmolts to rebuild wild populations in Oregon coastal streams. Canadian Journal of Fisheries and Aquatic Sciences **43**:2443-2449.

In our opinion, the Nickelson paper entirely fits the pattern suggested by Figure 2. Nickelson et al. 1986 report reach scale juvenile coho salmon densities in stocked and unstocked streams, and describe relationships to total adult abundance upon return of the same cohorts. They offer an ecological explanation for their results (carrying capacity, competitive asymmetries) that is more precise than many population scale studies, but more coarse than studies in experimental stream channels that often measure individual fish traits (e.g., body size, growth rate, behavior). No change made.

R3 Edit (p35, line 763): Replace "labortories" with "laboratories."

Change made.

R3 Edit (p35, line 767): Replace "assymetries" with "asymmetries."

Change made.

R4 Edit (p35, line 758): Delete "of" before the words "between hatchery."

Change made.

R4 Comment (p36, line 772): R4 highlights "habitats" and asks "I think all in freshwater?"

Perhaps, though when generalizing the literature we prefer to describe patterns rather than speak in absolutes to leave open the possibility of a study design that does not fit the pattern. No change made.

R4 Edit (p36, line 780): Add "counterparts" after "to competition with hatchery."

We revised "hatchery" to "hatchery-reared fish."

R3 Edit (p36, line 781): Replace "enviornments" with "environments."

Change made.

R8 Comment (p37, line 793): I am not convinced by the Ruggerone and Goetz study, despite full respect for the authors, and suggest more caution when referring to it.

We revised the sentence to read "Ruggerone and Goetz (2014) suggested that indirect foraging competition with pink salmon limited the marine survival of hatchery Chinook salmon…" in accordance with the reviewer's suggestion.

R8 Edit (p37, line 797): "ultimate"

Change made.

R7 Edit (p37, line 805): Replace "seprate" with "separate."

Change made.

R4 Comment (p37, line 807): With regards to the reference (Davis et al. 2018), R4 states "Cunningham et al. 2018 Global Change Biology revealed a strong negative correlation between wild AK chinook and hatchery chum from Hokkaido. That interaction if real must occur in high seas."

Good point, we added a sentence describing the relevant results from the Cunningham 2018 study to the preceding paragraph focusing on marine carrying capacity and competition. However, in our opinion, the suggestion that hatchery-natural competition is most likely to occur for species with extensive use of estuaries and nearshore areas is reasonable and helps identify areas for future research on hatchery risks, a theme we build upon in the conclusions section.

R1 Edit (p38, line 817): Delete "of marine habitats."

We revised this passage, as described in our response to the next comment.

R1 Comment (p38, line 817): With regards to the reference (Tatara and Berejikian 2012) R1 states "This review and synthesis discussed competition among juveniles in freshwater. Carrying capacity in marine environments is also important, but the number of hatchery fish released in Washington state is a very small proportion of hatchery releases into the

North Pacific. Discussion of hatchery effects on competition and marine carrying capacity should carefully articulate the spatial and temporal scale, as done in the preceding paragraph."

Good point. We rewrote the conclusion of this paragraph to identify the parallel to Tatara 2012's statements on freshwater carrying capacity, while making clear the distinction from our own suggestion of a similar importance of hatchery releases relative to marine carrying capacity. We further added two sentences stating that Alaska pink salmon and Japanese chum salmon dominate hatchery releases in the Pacific Ocean, thus, Washington's hatchery releases likely have the largest influence on marine competition in more local habitats (Puget Sound, Columbia River estuary, nearshore Washington coastal habitats).

R8 Comment (p. p39, line 842): The Huber and Carlson study was in California so you might cite this as the general issue, and then the local example.

Good point, we revised the sentence to indicate that the Huber 2015 paper addresses California Chinook salmon whereas the Nelson 2019 study addresses Salish Sea Chinook salmon.

R4 Comment (p39, line 850): R4 highlights "populations" at the end of the paragraph and states "Consider citing new paper in CJFAS by Andy Seitz et al. showing high salmon shark predation on large (age 3) chinook in Bering sea and gulf of AK. Potential for hyperpredation/apparent competition if hatcheries are supporting boom of predators that prefer larger prey."

We appreciate the suggested reference, the topics of late-stage salmon marine mortality and predation by salmon sharks are interesting. However, the predation events described in the paper are too geographically and temporally distant from hatchery facilities to specifically consider the ecological role of hatchery releases (i.e., short term impacts). The paper also does not address the population dynamics of salmon sharks or other salmon predators (i.e., long term impacts). Thus the connection to hatchery practices seems weak. No change made.

R4 Comments (p42, line 920): R4 highlights the word "pathogens" at the end of the paragraph and states it is "Worth highlighting the role of climate in these potential impacts as well."

We added a sentence, along with two relevant references, to the disease risks section acknowledging that climate change will alter diseases dynamics.

R4 Comments (p43, line 929): R4 highlights the word "point" and adds "And serve as disease vectors."

We dedicate an entire section to disease risks. We prefer to comprehensively address disease issues within that section rather than briefly touching on the topic in the facility effects section. No change made.

R8 Comment (p43, line 936): Might point out that if the hatchery takes a standard amount of water, and climate change reduces flows, the natural population may suffer disproportionately

This is a reasonable point. However, thoroughly addressing climate change would significantly expand the scope of our review, and necessitate addressing a host of issues. We prefer to concisely state the facility effects risks, but avoid suggesting possible scenarios due to changing climate. No change made.

R2 Comment (p44, lines 956-959): On the Levin (2001) study, R2 mentions "As an aside, it seems like it would be worth repeating this study using a) an additional 20 years of data, and b) a more accurate estimate of ocean conditions."

Sure, we agree the Levin 2001 study is worth revisiting but we are not in a position to repeat their analyses for our hatchery reform science paper.

R8 Comment (p. 44, line 959): If the study was by Levin (singular), then it should be "author"

In the previous sentence, we revised "Levin (2001)" to "Levin et al. (2001)" to indicate the paper had multiple authors.

R3 Edit (p44, line 962): Replace "Snake River basin" with "Snake River Basin."

Change made.

R8 Comment (p45, line 969): Was the correlation positive or negative? More fish released was associated with higher or lower productivity?

We revised "correlation" to "negative correlation" to specify the nature of the relationship.

R3, R8 Edit (p45, line 977): Replace "proporation" with "proportion."

Change made.

R8 Edit (p46, line 992): "negatively"

Change made.

R3 Edit (p46, line 996): Replace "popualtions" with "populations."

Change made.

R7 Edit (p46, lines 999-1001): Replace "Lister et al. (2013) used a unique study design to separate the hatchery-origin fish reduce natural-origin productivity hypothesis from the hatchery-origin fish perform poorly in the natural environment hypothesis," with "Lister et al. (2013) used a unique study design to separate contrasting hypotheses: hatchery-origin fish may reduce natural-origin productivity versus hatchery-origin fish perform poorly in the natural environment."

Good rewrite, change made. However, we replaced "contrasting" with "different" and removed "may" because we are describing a hypothesis, not a result or conclusion.

R7 Edit (p46, line 1001): Replace "He" with "They."

The Lister 2013 only has one author so "he" is appropriate in this case. However, we removed the erroneous "et al." from the preceding sentence that created the confusion.

R8 Edit (p47, line 1009): "al.'s"

Change made.

R8 edit (p47, line 1016): "Alaska"

Change made.

R4 Edit (p47, line 1019): Add "and fueling the belief that hatchery production was attributable" after "fishery catch (Amoroso et al. 2017)."

This is a reasonable point, though we softened the reviewer's suggested language to simply point out that the shift in ocean conditions contributed to the uncertainty regarding the success or value of the hatchery programs. We are not in a position to comment on the rationale or political climate surrounding Alaska's pink salmon hatcheries.

R3, R4, R8 Edit (p48, line 1037): Replace "popualtion" with "population."

Change made.

R3, R4 Edit (p48, line 1039): Replace “targetted” with “targeted.”

Change made.

R4 Edit (p48, line 1045): Add “in either freshwater or marine ecosystems” after “natural populations.”

We feel the lack of knowledge on predation is clear from the sentence as written and there is no need to specify the two major ecosystems that salmon inhabit. No change made.

VE Edit (p48, line 1045): Replace “popuations” with “populations.”

Change made.

Hatchery Reform

R1 Comment (p49, line 1066): “This paragraph is very important. This point is not often articulated.”

Great, thank you for the confirmation.

R3 Comment (p49-50, lines 1066-1073): “Good Section.”

Thank you.

VE Edit (p50, line 1084): Replace “long term” with “long-term.”

Change made, though we believe the reviewer is referencing line 1069 not 1084.

R4 Edit (p51, line 1097): Add “, which in turn facilitate metapopulation level resilience and stability” after “portfolios.”

We agree that a diversity of life history portfolios facilitates resilience and stability; however, we feel that it is unnecessary to spell that out in this paper. The Schindler citation should be sufficient. No change made.

R4 Comment (p51, line 1108): Replace “In fact” with “Indeed.”

Change made.

R3 Edit (p52, line 1114): Replace “on the these“ with “on these.”

Change made.

R1 Comment (p52, line 1117): On the word "wild," R1 asks "'Natural' to be consistent with your glossary?"

Yes, in this instance we changed "wild" to "natural" because this refers to the origin of the fish, rather than its lineage.

R1 Comment (p52, line 1127): "Does this equation assume that pre-zygotic mechanisms are responsible for reduced RRS of hatchery fish (e.g., pre-spawning mortality, reduced competition for access to females, etc)?"

HSRG does not indicate from where in the life cycle RRS is reduced and therefore whether this reduction is pre- or post-zygotic. HSRG states that "hatchery-origin fish spawning in the wild may on average produce fewer adult progeny than natural-origin spawners," and uses this equation to adjust pHOS. We are uncertain if we would categorize pre-spawn mortality as pre-zygotic rather than the last part of the lifecycle where post-zygotic selection may occur. Regardless of whether HSRG intended effective pHOS to reflect pre- or post-zygotic mechanisms, our understanding of Ford (2002) is that this "adjustment" to pHOS is made effectively in the second part of Equation 5 in Ford (2002; (1-pw) equals pHOS), and therefore effective pHOS should not be used as a parameter in Ford (2002). That is, if effective pHOS was used, selection would have been applied twice. We comment on HSRG's effective pHOS in footnote 2, which was inadvertently removed from this second draft of the report (see comment below). No change made.

R3 Edit (p52, line 1129): Replace "hatcher-origin" with "hatchery-origin."

Change made.

R2 Comment (p53, line 1138): For the equation on line 1144, R2 highlights the small "2" to the right of the equation with a question mark.

Thank you for this comment. The small "2" denotes a footnote, which was inadvertently removed from the final draft. We have re-instated that footnote in the final draft.

R3 Edit (p54, line 1156): Replace "temporally separating" with "temporally or spatially separating."

Good point, change made.

R2 Edit (p54, line 1166): Replace "populations' " with "population's."

We maintain that the position of the apostrophe was correct as it refers to plural possessive.

R4 Comment (p54, lines 1162-1164): With regards to the statement "Indeed, 34 of the 35 (97 %) conservation hatchery programs operated by WDFW employ integrated broodstock management, whereas only 47 of 124 (38 %) WDFW harvest hatchery programs employ the integrated approach," R4 comments "This is a great statistic. Perhaps allude to this above where I commented on different objectives only being meaningful if they have different strategies. This suggest that they do."

Thank you for the confirmation. However, as stated in our response to the earlier comment, we prefer to use introductory sections to present key concepts, and draw the linkages between goals and hatchery operations later in the manuscript after key ideas have been sufficiently described.

R1 Comment (p55, lines 1175-1178): With regards to section "Third, HSRG (2013) identified four stages...two dams (HSRG 2012)," R1 states "This was a great process for a situation like the Elwah, where almost immediately the capacity of the watershed to support salmon and steelhead increased greatly. Is this a reasonable framework for most of the rest of watersheds in WA state, most with declining or stable habitat, or does it apply only to Elwha-similar situations, which are rare."

This is an excellent question. As stated in the conclusions, we do think the four phases of recovery approach has scientific merit because it recognizes the spectrum of conservation hatchery intervention. However, we suggest that the implementation of the four phases, and especially use of the preservation and recolonization phases, has confounded conservation and harvest objectives. We directly addressed the reviewer's comment in the conclusion section, which we revised to more strongly emphasize that the preservation designation should only be used where a conservation hatchery is necessary to ensure population persistence, and recolonization designation reserved for populations presented with an opportunity for significant spatial expansion.

R8 Edit (p55, line 1187): "prerequisite" seems like the wrong word here

We rewrote the phrase to read "the recolonization phase is characterized by an increase in suitable habitat."

R3 Edit (p58, line 1251): Replace "Perseveration" with "Preservation."

Change made.

R2, R4, R7, R8 Edit (p58, line 1252): Replace "becuase" with "because."

Change made.

R1 Comment (p58-59, lines 1246-1254): With regards the section "HSRG (2013) stated…identity and diversity," R1 states "Authors seem to be arguing that high PNI targets should be adopted during the first two phases. If so, I'd suggest stating that directly and then defend it more specifically. To say that it seems counter-productive is a bit vague and doesn't inform the issue much."

We appreciate this comment and R1's previous comment directly above, as both comments forced us to think more clearly about our reservations concerning HSRG's phases of recovery. We rewrote the section on PNI and pHOS targets for Preservation and Recolonization phases. Furthermore, we directly state our recommendation for PNI goals at least during the recolonization phase in the conclusions section.

R7 Edit (p59, line 1272): Replace "If you remove the recovery phase" with "If the recovery phase is removed."

Change made.

R3 Comment (p60, line 1275): Replace "Four of the five of these programs are" with "Of these, four of the five are."

Change made.

R3 Edit (p61, line 1301): Is "wildlife" meant to be "wild fish?"

Yes, change made.

R4 Edit (p60, line 1284): Replace "captive-breed" with "captive-bred."

Change made.

R1 Edit (p61, line 1301): Delete "life."

Change made.

R3 Edit (p61, line 1317): Replace "moderate, to extreme" with "moderate, or extreme."

We are specifying a range; "to" is more appropriate than "or," although we changed text to read "negligible to extreme."

R7 Comment (p62, lines 1325-1334): With regards to the section "In Figure 3, we provide four different ...you do both (Figure 3)," R7 states "This illustrates a simple point with these four combinations, but more complex scenarios would benefit from more combinations if taken to publication."

Thank you for this suggestion. We will consider other scenarios, as appropriate in any manuscript or other reports.

R7 Comment (p62-63, lines 1339-1341): With regards to the statement "This means that you can directly compare different broodstock and escapement management options across parameter space, to determine which management option provides the lowest risk of fitness loss to the wild population," R7 mentions "The additional scenarios in the appendix are helpful."

Thank you. The additional scenarios were included based on reviewer comments from the previous version of the report.

R7 Comment (p63, lines 1348-1349): With regards to the statement "decreasing pHOS provides greater fitness gain than increasing pNOB," R7 responds "In some cases, effective pHOS may be lower than pHOS so this might achieve the same effect as modeled."

In most cases effective pHOS will be lower than pHOS since HOS is weighted by RRS, which is typically less than one. However, we think it is inappropriate to use effective pHOS with the models in Ford (2002), since these models already account for fitness-loss of hatchery-origin natural spawners, as explained in footnote #2. No change made.

R8 Comment (p64, line 1368): "delete "a" so it reads "benefits to wild populations."

Change made.

R7 Comment (p65, lines 1398-1399): With regards to the statement "and high pNOB may decrease recruitment from the hatchery population by decreasing hatchery fitness;" R7 responds "Not clear. There isn't strong selection in the hatchery environment."

Is R7 suggesting that there is no selective difference between the hatchery and natural environments? We would disagree. The models used in this report assume that there is a selective difference between the hatchery and natural environments, but those differences can range from weak to strong, and are summarized in Figure A2-1. A natural-origin fish will have

lower fitness in the hatchery than a hatchery-origin fish in the hatchery. Therefore, by definition, increasing pNOB will lower hatchery production, by a small to large amount, depending on the "hatchery effect" (Appendix 2). No change made.

R7 Comment (p65, lines 1402-1404) With regards to the conclusion that "the demographic model suggests that reducing pHOS produces greater natural recruitment, recruit per spawner, and wild fitness than increasing pNOB (Appendix Figure A4-3)" R7 states "Agreed for these scenarios. However, not clear if pNOB is maintained at high levels. It would also be worth discussing effective pHOS vs. pHOS."

As discussed above, because the Ford (2002) model already accounts for fitness loss of hatchery-origin fish that spawn naturally, it would be inappropriate to use effective pHOS instead of census pHOS in these models. No change made.

R6 Comment (p66): R6 states additional implications and recommendations with respect to the Baskett-Waples model, "The Baskett-Waples model makes clear the fitness trough that occurs when the selective optimum of the hatchery population is different enough that fitness of H fish in the wild is compromised, but not so different that H fish fail to reproduce in the wild. It could be pointed out that this has important implications for establishment of new segregated hatchery programs, which should be strongly domesticated to be divergent enough from the local wild population. Unless a non-local population is imported (along with all the risks that entails), it will be necessary to develop a new divergent hatchery population locally. That would take a number of generations, and unless containment is essentially 100% effective, the new hatchery population could have severe adverse fitness consequences for the wild population during the period when it is only moderately diverged."

Thank you for these comments. In the May 3 draft reviewed by the WSAS, we included a section on segregated hatcheries in our discussion of the Baskett-Waples model. In that section, we stated that Baskett and Waples effectively argued that there existed only a limited set of biological circumstances when segregated hatchery programs perform better that integrated programs in mitigating genetic risk to wild populations. Two reviewers of our original draft stated that we were misinterpreting Baskett and Waples. As a result, we decided to remove our discussion of segregated hatchery programs from the revised draft. We agree with R6's comments above. In fact, this comment and the first paragraph in the discussion in Baskett and Waples supports our original contention that it is difficult to implement a segregated hatchery program that mitigates genetic risks to wild populations better than an integrated program. No change made.

R7 Comment (p68, line 1462): For the section on "Controlling pHOS," R7 recommends "A paragraph on effective pHOS vs pHOS is warranted in this section."

We respectively disagree. Please see comments above concerning our reservation with using effective pHOS. The phenotypic model of Ford (2002) is the basis for PNI (see HSRG 2009b), and Ford (2002) already accounts for the lower RRS of hatchery-origin fish compared to natural origin fish.

R7 Edit (p68, line 1471): Add the phrase "and for species such as steelhead that spawn near peak runoff," after "implement effectively, especially on larger rivers."

Good point, we added a slightly modified version of this phrase as suggested.

R7 Comment (p69, line 1475): With regards to "(Wilson et al. in prep)" R7 asks "Is there a link to a draft that can be viewed?"

This report was finalized during the revision process, and we have revised the citation accordingly. It can be downloaded at:

https://wdfw.wa.gov/publications/02117

VE Edit: (p70, line 1497): Replace "downstram" with "downstream."

Change made.

R8 Comment (p70, line 1506): There is a growing literature indicating some effects of catch and release fisheries, so if the hatchery run is large enough to generate a lot of effort, then the wild population may still suffer. For example, if 5% of the wild steelhead caught and released die, and we catch enough of them (including multiple captures) it might add up. Here, as with so many aspects, the problems tend to scale with the relative size of wild and hatchery populations.

These are good points but are covered elsewhere in the report. We direct the reviewer to the fishery risks section for a discussion of the asymmetry between impacts to wild populations and abundance of hatchery populations. Hatchery program size is also a major theme of the paper, including a stand-alone conclusion stating that nearly every aspect of hatchery risks scales with hatchery program size. Here, we wish to focus on the ability to control pHOS. No change made.

R8 Comment (p71, lune 1527): Might cite Campton (2004) on this topic, but then note that these protocols are totally artificial and bear no resemblance to what occurs in rivers. Thus I think calling random mating "genetically benign" is unwarranted (Quinn 2005).

Campton, D. E. 2004. Sperm competition in salmon hatcheries: The need to institutionalize genetically benign spawning protocols. Transactions of the American Fisheries Society **133**:1277-1289.

Quinn, T. P. 2005. Comment: Sperm competition in salmon hatcheries - The need to institutionalize genetically benign spawning protocols Transactions of the American Fisheries Society **134**:1490-1494.

Sperm competition, which is the focus of the Campton 2004, is certainly one of several factors affecting genetic diversity within hatchery populations. The concept of sperm competition and the Campton 2004 paper specifically are both cited in the Fisch 2015 review paper, which we feel is the most comprehensive overview of the issues related to mating protocols and genetic diversity. We also make no suggestions that hatchery mating protocols can mimic or replicate natural spawning patterns. Statements describing deviations from natural patterns could be made regarding every aspect of hatchery propagation (i.e., spawning, growth, rearing, release, etc.). Thus, the tone of our treatment of hatchery risks is of reducing risks, not eliminating them. Given the overall lack of monitoring on genetic diversity, we prefer a rather concise discussion of issues surrounding genetic diversity and mating protocols. No change made.

R8 Comment (p72, line 1542): Hmmm… Is it the number released or the number returning that is more important? Wouldn't it depend on the nature of the issue? Competition among juveniles might be more related to number released, whereas number returning might affect genetic concerns?

Good point, we added a statement that release number, in turn, will regulate the number of returning adult hatchery-origin fish. Note also that we implicitly acknowledge the reviewer's point by using adult hatchery-origin fish straying to the natural spawning grounds as an example in subsequent sentences. However, for the purposes of clarity in terminology, we prefer to define program size as release number.

R3 Edit (p72, line 1549): Replace "naturally" with "natural."

Change made.

R3, R8 Edit (p72, line 1556): Replace "into Washington" with "into the Washington."

Change made.

R2 Comment (p72-73, lines 1558-1560): On the statement "Chinook salmon hatchery production generally increased through the 1970s, peaked in the late 1980s (200-250 million), and subsequently declined (currently approximately 170
biomass released has also declined? I wonder if some of the decline since the 1980's is due to releasing smaller numbers of larger fish?"

Good point, a trend towards releasing larger sized fish likely did contribute to the overall reduction in release numbers, and we revised the manuscript accordingly.

R7 Edit (p73, line 1560): Replace "Coho hatchery production" with "Hatchery production of coho salmon."

Change made.

R7 Edit (p73, lines 1570-1571): Replace "the median conservation program size is 212,500 and the harvest program size is 1,750,000," with "the median conservation program size is 212,500 juveniles released, which is relatively much smaller than and the harvest program size is 1,750,000 juveniles."

We revised for clarity in the intent of the reviewer, though with slightly different wording.

R8 Edit (p73, line 1574): "al."

Change made.

R2 Edit (p74, line 1581): R2 highlights the word "program" with a question mark to indicate a word may be missing.

We revised "program" to "objectives."

R3 Edit (p74, line 1581): Replace "program" with "program goals."

We revised "program" to "objectives."

R7 Edit (p74, line 1581): Replace "to meet conservation program" with "to meet program goals."

We revised "program" to "objectives."

R2 Comment (p74, lines 1600-1602): On the statement "These observations indicate that AHA lacks the predictive precision needed for a hatchery program-by-program

determination of release number needed to keep hatchery impacts on natural populations within acceptable limits," R2 states that "I think I still agree with this, but on the other hand it might be better than nothing…"

Sure, we agree AHA has value, but are aim to carefully articulate the limits of its precision and utility. No change made.

R7 Edit (p76, line 1628): Replace "outsize" with "outsized."

Change made.

R3, R7 Edit (p76, line 1628): Replace "important" with "importance."

Change made.

R7 Edit (p77, line 1654-1655): Replace "long term evolution" with "long-term evolution."

Change made.

R7 Edit (p79, line 1699): Replace "collected eyed eggs from" with "eyed eggs collected from."

Change made.

R8 Comment (p80, line 1712): It has long been known that releasing large smolts results in more jacks and younger females too in Chinook. Consequently, a balanced evaluation of the benefits of large smolts needs to consider this smaller size and inflated survival rate unless one uses (for example) an age-4 adult equivalent in Chinook and adjusts downward returns of younger fish.

Agreed, and this is an excellent reason why there is a strong research need to examine different rearing strategies in production-oriented programs intended for provide harvest opportunities (e.g., fall subyearling release Chinook). Numerically, these programs compose most of WDFW's hatchery releases, but they have not received as much research attention. We aim to point out this research need, but it is beyond the scope of our review to suggest how such studies might be designed or executed. No change made.

R4 Comment (p80, line 1720): R4 highlights "(data not shown)" and states "Would be a great figure or review in and of itself. Are those data available from HSRG work?"

We agree that this topic is worthy of more analysis, even a stand-alone paper. WDFW maintains a hatchery releases database

that would be valuable for assessing out-of-basin releases but it would require significant analysis, which was beyond the scope of our review. The Myers 1998 NOAA Chinook status review provides a long table of hatchery releases, separating releases sourced within vs. outside the recipient ESU. We are not aware of any other efforts to summarize transfers.

R8 Edit (p81, line 1730): transfers were

Change made.

R7, R8 Edit (p82, line 1765): Replace "Another strategy are volitional releases" with "Another strategy is volitional release of smolts."

Change made.

R3 Edit (p82, line 1774): Replace "specifics" with "specific."

Change made.

R3, R7 Edit (p82, line 1776): Replace "non-exist" with "non-existent."

Change made.

R2 Comment (p83, lines 1781-1782): On the statement "as WDFW hatcheries have generally transitioned away from fry (non-smolting fish)" R2 asks "Does this explain some of the declining trend in release numbers since the 1980's?"

Yes, we revised the section on "program size" to explicitly say so.

R8 Edit (p83, line 1778): "timing…is"

Change made.

R7 Comment (p83, lines 1790-1792): With regards to the statement "Indeed, Snow 2015 reported that earlier releases of summer Chinook salmon had a higher smolt to adult survival rate than later releases." R7 responds "Earlier releases should be smaller than later releases, so this point is not clear."

We revised "earlier releases" to "earlier, smaller-bodied releases." In our opinion, this point is clear. The previous sentence states that survival is not as simple as "bigger is better" and the Snow 2015 example supports that claim by demonstrating that earlier and smaller releases had higher survival than later, larger releases.

R8 Comment (p83, line 1790): Snow 2015 – this reference is missing.

We added Snow 2015 to the references section.

R7 Edit (p84, line 1804): Delete "of" in the phrase "effectiveness of releasing of actively smolting fish" so it reads "effectiveness of releasing actively smolting fish."

Change made.

R8 Comment (p85, line 1827): If ad clips are so good, then why are so many salmon released with other, internal marks only, or none at all? What is the overall fraction of ad clipped Chinook and coho from state, federal, and tribal programs? Such a breakdown might be informative.

The purposes of the alternative mark types are clearly described in this section. This includes the use of coded wire tags to estimate marine survival and harvest mortality, and the use of thermal otolith marks to avoid exposure to mark-selective fisheries. A detailed breakdown of mark types is beyond the scope of our paper. Adipose clip and CWT mark data are available at www.rmis.org in a publicly accessible database. No changes made.

R2 Edit (p85, line1833): Insert "are" after "However, they."

Change made.

R8 Comment (p85, line 1833): "they are costlier"

Change made.

R3, R7 Edit (p85, line 1837): Replace "used on conservation" with "used in conservation."

Change made.

R3 Edit (p86, line 1850): Replace "PRV (Meyers 2017)" with "Piscine orthoreovirus (PRV; Meyers 2017)."

Change made.

R3 Edit (p86, line 1850): Replace "'The Salmonid.." with "In addition, 'The Salmonid…"

Change made.

VE Edit (p87, line1882): Replace "long term" with "long-term."

Change made.

R7 Edit (p85, line 1825): Add the sentence "However, mass marking may subject wild fish to higher exposure to handling stress and delayed mortality after release," after the sentence ending in "the hatchery (i.e., controlling pNOB)."

The context here is marking hatchery-origin fish for the purpose of their identification in fisheries and hatchery spawning, not marking of wild or natural-origin fish. No change made.

R7 Edit (p85, line 1826): Add the phrase "unless other genetic approaches could be implemented to rapidly identify fish" after the words "confounded with natural-origin salmon."

This opening paragraph of the mass marking section simply points out the purpose and outcomes of mass marking, it does not explicitly describe marking techniques. We include parentage-based tagging in our discussion of marking techniques in the next paragraph. Thus, genetic techniques are treated in the same manner as adipose clipping or coded wire tags – they are all options available to hatchery managers to identify hatchery-origin fish. No change made.

R4 Comment (p85, line 1843): R4 highlights "(Steele et al. 2019)" at the end of the sentence and adds in the comments "And doesn't allow for mark selective fisheries."

We added a final sentence to the marking paragraph indicating that "thermal otolith marks and parentage-based tagging do not allow for mark-selective fisheries on their own, and only rarely are CWTs used for this purpose."

R8 Edit ((p87, line 1865): "in treating dish disease"

Change made.

R3 Edit (p87, line 1869, 1879 & 1885): Replace "Elliot" with "Elliott."

We made all three spelling corrections.

R2 Comment (p88, line 1913): R2 asks whether in the adaptative management section "I wonder if you want to say something about the possibility of running the entire hatchery system in a way that would allow for great power to detect effects, for example by deliberately varying release numbers in a systematic way?"

In the conclusions section, we describe the need for a large-scale experimental approach to understanding hatchery risks and hatchery reform effectiveness. No change made.

R6 Comment (p88): R6 makes a general comment on adaptive management, stating "One of the limitations of adaptive management vis a vis salmon hatcheries is that deleterious effects on fitness-related traits are very difficult to demonstrate empirically, in part because natural demographic and environmental variation is large. This means that, even with a very large monitoring effort, substantial changes to the natural population could occur years before they can be reliably detected (see Hard 1995). This reality has important consequences for risk-averse management."

This difficulty of separating hatchery effects from other influences on population performance is directly emphasized in the review of population-scale empirical studies in the ecological risks section. We also considered this comment in our revisions to the discussion of RRS studies testing for genetic fitness effects in the emerging science section, pointing out the tremendous investment in monitoring required for an appropriate study design. We again touched on this theme in a paragraph added to the emerging science section stating that separating hatchery-natural ecological interactions from other factors affecting population productivity remains challenging. Finally, we do not consider it our charge to advocate for a risk-tolerant or risk-averse management strategy, only to describe the relevant risks to the best of our ability.

R4 Edit (p89, line 1920): Add "and even less so offshore" after "Columbia River estuary."

This is a valid point, though we chose not to make any edits in order to maintain a tight focus on the habitats that we feel such information is most critical.

Emerging Science

R4 Comment (p91, lines 1956-1959): With regards to the statement "In our literature summary, we sought to identify the most influential studies from 2010 to the present addressing a hatchery reform action, or more indirectly, informing the likelihood or magnitude of a hatchery benefit or risk," R4 mentions that "This will make excellent paper in and of itself."

Agreed, there is a wealth of information here worthy of review.

R2 Comment (p92, line 1985): With respect to the reference to Christie et al. 2014a; Ford et al. 2016, R2 also suggests "you might also mention Ford et al. 2012 (doi: 10.1111/j.1755-263X.2012.00261.x) in this context."

We added a description of Ford et al. 2012 to Table 5 and the text of the emerging science section.

R7 Edit (p92, line 1980-1981): Add "Hess et al. 2012;" to the references so it reads "(Hess et al. 2012; Christie et al. 2014a; Waters et al. 2015; Ford et al. 2016; Janowitz-Koch et al. 2019)."

Hess et al. 2012 is included in the Christie et al 2014a review so it is not cited directly in this sentence.

R7 Edit (p93, line 1991-1992): Add "Hess et al. 2012;" to the reference so it reads "(Hess et al. 2012; Janowitz-Koch et al. 2019)."

Janowitz-Koch 2019 is essentially an update of the Hess 2012 study so we choose to only cite the more recent paper, and avoid the potentially misleading suggestion that there were two separate case studies demonstrating lower fitness costs for hatchery programs employing 100% natural-origin broodstock.

R2 Comment (p93, lines 1994-1995): With regards to the statement "Major changes in hatchery management provide important opportunities to evaluate hatchery reform" R2 states "Somewhere in here should you mention Nelson et al. 2019's study that found no apparent negative (or positive) effect of hatchery releases on Puget Sound Chinook salmon productivity? This is sort of the flip side of the Scheuerell and Venditti studies – not a lot of evidence of large scale benefits from conservation hatcheries, but perhaps also not a lot of evidence for large scale demographic depression from harvest-oriented hatcheries (at least in Nelson's study – this is in contrast to some others you cite earlier such as Levin et al. 2001 and Buhle et al. 2009)."

Good suggestion, we added the Nelson 2019 study to the table, as well as a brief paragraph on ecological interactions referencing the Nelson study.

R8 Edit (p93, line 2004): "al." Ditto for line 2005

Both changes made.

R4 Comment (p94, line 2029): R4 highlights the word measure at the end of the paragraph, and states "Think call out to Dittman et al. embryonic imprinting to aid supplementation is worth noting. Cover of Fisheries."

Good suggestion, we added Dittman 2015 to both Table 5 and the text of the emerging science section.

R7 Edit (p94, line 2010-2011): Add "; Janowitz-Koch et al. 2019" to the references so it reads "(Berejikian and Van Doornik 2018; Janowitz-Koch et al. 2019)."

Good suggestion, we added the Janowitz-Koch 2019 reference.

Conclusions and Recommendations

R2 Comment (p95, lines 2040-2041): R2 agrees with the statement that "additional research on the economic, social, political, and legal value of hatcheries that will help clarify the benefit-risk tradeoff."

Thank you for the confirmation.

R8 Comment (p95, line 2042): Why must the choice be between fisheries subsidized by hatcheries or wild runs with no fishing? Put another way, why has the department been so reluctant to embrace what so many anglers want – more access, even if it means less or even no retention. The department seems to think that nobody will buy a license unless they can maximize their cooler load but increasingly this is not the case. Not only is the department not showing leadership, it is not even willing to follow when and where the anglers lead. But I digress, perhaps.

Here, we simply point out that improved information regarding social preferences regarding hatcheries, such as that provided by the reviewer, would aid decision making. We use one potential question as an example of such information; we do not imply that it is the only or most important question. We wish to refrain from value judgments regarding social preferences in favor of a statements that research on the social values of hatcheries are generally lacking. No change made.

R2 Edit (p96, line 2064): Delete "unreasonable" and replace with "unsupportable."

We feel the word "unreasonable" is appropriate in this instance, and prefer it to "unsupportable." No change made.

R4 Comment (p96, lines 2069-2070): With regards to the conclusion "hatchery reform is largely aimed at reducing risk in a relative but not absolute sense" R4 responds that this is a "Constantly changing target with modifications to hatchery programs (e.g. release size)."

We are unclear on the intent of this comment. We wish to maintain tight focus on the limits of hatchery reform here, and not introduce related but different ideas (e.g., hatchery programs often change). No change made.

R4 Comment (p97, lines 2079-2080): With regards to the conclusion "In WDFW's hatchery system, a cultural focus on efficiency and maximizing abundance prevent widespread

implementation of risk reduction measures" R4 asks "I wonder if you might make this statement even more robust: it seems that large scale production programs are simply not compatible with risk reduction measures."

We feel the verb "prevent" is as strong as or stronger than "simply not compatible with." However, we deleted the word "cultural" in order to focus on the outcome rather than the process.

R3 Edit (p97, line 2080): Replace "prevent" with "prevents."

Change made.

R2 Edit (p97, lines 2088-2089): Replace "Such measures become progressively difficult, or at least time consuming and costly" with "Such measures become progressively more difficult, or at least more time consuming and costly."

Change made.

R8 Comment (p97, line 2093): Why stop short of the obvious next step? Why not suggest that the state, by itself or in partnership with other regional entities, commence a really good, carefully-planned study of its own on wild and hatchery salmon? See also line 2161.

We make this recommendation at the outset of the "knowledge gaps and major assumptions of current hatchery management" section.

R8 Comment (p98, line 2110): OK – but given the demands from killer whale constituents, anglers, and tribal fishermen, how are so going to dial back on hatchery release numbers?

Point taken. However, our goal for this paper is to raise the relevant scientific issues, and review relevant studies. It is the role of the Washington Fish and Wildlife Commission and WDFW leadership to determine hatchery policy and hatchery production levels. We expect decisions regarding WDFW's hatchery policy to ensue after we present our work to the Commission.

R2 Edit (p98, line 2115): Replace "be more" with "be the more."

We revised "be more" to "be a more," as suggested by Reviewer 3.

R3 Edit (p98, line 2115): Replace "be more" with "be a more."

Change made.

R4 Comment (p98, lines 2110-2111): With regards to the conclusion "Program size requires more careful scrutiny and scientific justification because it affects virtually every

aspect of hatchery risks" R4 states "Strong conclusion that seems obvious but excellent to formalize."

Thank you for the confirmation.

R2 Comment: (p100, lines 2141-2144): On the statement "we recommend crafting a stand-alone monitoring and adaptive management plan for each hatchery program that quantifies both benefits and risks, and explicitly links hatchery performance metrics to potential operational changes," R2 states "Agree, but I think you should also recommend that the programs have a monitoring and adaptive management plan as a group, at least on a regional level. In other words, I'm not sure you want to recommend that each program have an independent, separate plan – rather they should work together to maximize information content and to address risks that present themselves at a level larger than a single program (e.g. ecological interactions in Puget Sound)."

Good suggestion, we added a sentence suggesting a regional component to monitoring and adaptive management programs.

R1 Comment (p100-101, lines 2162-2173): With regards the section "The scientific community… intended to provide harvest" R1 states "I'd be careful here. As difficult as it was to pull off the Idaho Supplementation studies, even that study did not come close to the timeframe needed to evaluate 'hatchery reform effectiveness', if by that you mean manipulating PHOS or PNI and measuring population response. It would take 10's of generations to do so. I think treatment-reference studies on a landscape scale should be conducted, but I'd suggest being very clear here about the expectations for that type of study. For example, in a 20-year timeframe you may be able to detect changes in measures of genetic diversity, natural population abundance, freshwater productivity (adult to smolt), and that would be quite valuable. But, because so much of the HSRG and discussion in this document is about heritable fitness loss caused by hatcheries and implementation of HSRG principles (broodstock management) to mitigate that risk, the reader may think that a treatment-reference study can answer the questions surrounding broodstock management and effects on fitness, which is very unlikely."

Good point, we added two sentences to the following paragraph discussing time frames indicating that if broodstock management can improve fitness, the length of time needed to observe an effect is likely dependent on the degree of fitness lost and the aggressiveness of hatchery reform implementation. In some cases, broodstock management and high PNI may take 10s of generations (decades) to increase population fitness and population performance.

R2 Comment (p101, lines 2172-2173): On the statement "we suggest a similar research program is needed to evaluate the risks of hatchery programs intended to provide harvest," R2 states "Agree – excellent suggestion."

Thank you for the confirmation.

R2 Edit (p101, line 2174): Replace "a experimental" with "an experimental." R8 Edit: "an experimental approach"

Change made.

R2 Comment (p101, lines 2177-2180): On the statement "We suggest that large-scale manipulative experiments that evaluate major changes in hatchery management are critical opportunities to advance hatchery reform science in Washington State," R2 states "Agree! You might want to highlight some of these recommendations with bullet points and in the executive summary – worried they may be easily buried and ignored here. Actually, I just looked and see you did this, although might consider expanding the exec summary slightly to include of these recommendations more explicitly."

The bolded summary (topic) sentences from our conclusions are repeated verbatim as bullet points in the abstract. We prefer to the keep the abstract concise, with the explanation and details provided in the conclusions section.

R1 Comment (p102, line 2192): On "have unlimited rearing capacity" R1 comments that "Again, I would be careful concluding that this assumption is implicit. There is clearly an assumption that there is additional rearing capacity, but what is the evidence that managers or proposers of increased hatchery production are assuming 'unlimited rearing capacity'. I just don't think that's likely and should be restated to 'implicitly assumes…….have additional capacity.'"

Good point, we replaced "unlimited" with "sufficient" and reframed the following sentence to state that "marine ecosystem carrying capacity is rarely considered in determining hatchery program size."

R2 Comment (p102, line 2192-2193): On the statement "marine ecosystems will have some limit to the number of salmon (including hatchery-reared fish) they can support," R2 responds "Yes, although as demonstrated by the Alaska pink and chum programs, the ocean may have a lot of 'extra' capacity to support salmon and its possible we are not close to that limit now. Clearly it's an important thing to study and figure out, however."

Ruggerone and Irvine (2018, Marine and Coastal Fisheries 10: 152-168) suggests that the ocean may have reached its carrying capacity for pink and chum salmon in recent decades. We feel the passage emphasizes both the importance of understanding marine carrying capacity, and need to better characterize it (i.e., uncertainty) as written. No change made.

R2 Comment (p102, lines 2193-2195): R2 agrees with the conclusion "efforts to characterize marine carrying capacity are essential to developing hatchery management strategies that account for competition."

Thank you for the confirmation.

R2 Comment (p102, line 2198): With respect to the reference to Sharma (2006), R2 states "The Buhle et al. 2009 study is also a good example of a situation where hatchery production was clearly demonstrated to depress wild production. On the other hand, I'm not sure anyone has gone back and evaluated how total (hatchery and wild) coho salmon abundance on the Oregon coast changed after cessation of hatchery releases there – from a purely fisheries perspective I don't know if the tradeoff was worth it or not."

A valid point. However, in our opinion, Sharma (2006) provides a slightly more explicit, detailed biological description of interactions between natural production and hatchery production in governing total adult returns. Here we simply aim to provide an example of how one might consider carrying capacity in hatchery management, not a comprehensive review. We feel the Sharma 2006 paper serves that purpose; the Buhle (2009) study is described and cited in the ecological risks section. No change made.

R4 Comment (p102, line 2203-2204): With regards to the statement "understanding the role of life history diversity on hatchery-wild ecological interactions is a significant research need" R4 adds in his comments "And ecosystem benefits/risks."

Good suggestion, we revised the sentence to read "understanding the role of life history diversity on hatchery-wild interactions and ecosystem stability is a significant research need," both here and in the abstract.

R4 Comment (p103, lines 2215-2217): With regards the conclusion "we recommend a more rigorous, consistent and intentional evaluation of cumulative hatchery effects across multiple hatchery programs operating within a geographic region" R4 states "This is a strong suggestion but also tip toes around the need to define the appropriate spatial scale for assessing cumulative impacts. Is all of Puget Sound appropriate? All PNW? I don't know the answer but it is critical to the discussion. Below you suggest it is at the ESU level. Perhaps that's right…"

This is a good question, but one we do not expect to resolve or answer in our review. We aim to generally state the need for cumulative effects assessment, but a specific plan is beyond the scope of our review. We suspect the appropriate geographic scale may vary by species and location.

R7 Edit (p103, line 2206-2207): Replace "population a collapse (Carlson et al. 2011)" with "a population collapse (Carlson et al. 2011)."

Change made.

R7 Edit (p103, line 2213): Replace "potentially" with "potential."

We revised "this hypothesis has potentially major implications" to "this hypothesis potentially has major implications."

References

R3 Comment: R3 recommends that the authors make sure "Elliott" is spelled correctly (see edits on p87 from R3).

We corrected the spelling of "Elliott" in all three relevant references.

R7 Edit (p113, line 2536): Add in reference to Hess, M. et al 2012 after Herr et al.

We added Hess et al. 2012 to the references section.

R6 References: R6 referred to a number of studies within their comments, and the references for those studies are included below.

Hard, J. J. 1995. Genetic monitoring of life-history characters in salmon supplementation: problems and opportunities. *American Fisheries Society Symposium*, 15:212-225.

Kalinowski, S.T, D.M. Van Doornik, C.C. Kozfkay, and R.S. Waples. 2012. Genetic diversity in the Snake River sockeye salmon captive broodstock program as estimated from broodstock records. Conservation Genetics 13:1183-1193

Waples, R.S., and J. Drake. 2004. Risk-benefit considerations for marine stock enhancement: a Pacific salmon perspective. pp. 260-306 *in* K.M. Leber, S. Kitada, H.L. Blankenship, & T. Svåsand, eds. Stock Enhancement and Sea Ranching: Developments, Pitfalls and Opportunities. Second Edition, Blackwell, Oxford.

Thank you for these references. We have added the Kalinowski 2012 and Waples 2004 to the references section, in accordance with our response to R6's comments.

Definition of Terms

R1 Comment (p125, lines 2949-2952): With regards to the definition of "Wild," R1 suggests "doing a final check to make sure this definition was adhered to throughout the document."

Yes, good suggestion, we reviewed the entire document for consistency. The term "wild" appears perhaps most frequently in

the discussion of the modeling presented in Appendix 4, which is appropriate because the exercise evaluates trends following initiation of a new hatchery program derived from a wild population.

Tables

R7 Edit (p131): Replace the phrase "Integrated Chinook hatchery programs throughout Washington" with "Integrated Chinook hatchery programs throughout Washington run by WDFW." R7 further comments "It is not clear if this table is intended to only include integrated hatchery programs run by WDFW, or comprehensive of all programs. If the latter, then there are several missing."

We revised the opening phrase of the figure caption to read "A non-comprehensive summary of integrated Chinook hatchery programs operated by WDFW."

R1 Comment (p132): On Christie et al. 2014a and the statement "When spawning in the river, hatchery-origin fish from local or predominantly wild broodstock tend to have lower reproductive success than natural-origin fish; evidence for genetic basis to fitness costs of hatchery propagation" R1 states "It's very important to get this right. This review both supports and is also equivocal. This from the Results section of the paper, "The Wenatchee Chinook and Umpqua coho studies (Theriault et al. 2011; Ford et al. 2012) followed a similar design to the Hood River steelhead study and compared hatchery fish with different degrees of hatchery ancestry. Unlike the Hood River case, neither study found significant differences in RRS between the different types of hatchery fish spawning in the wild, providing no evidence that reduced RRS was due to genetic effects in these studies (Fig. 4). We are still left with evidence for heritable fitness loss for steelhead (Araki et al. 2009), with confirmatory results from Ford et al. 2016. It's important to be clear about this."

Good point. We removed the phrase "evidence for a genetic basis to fitness costs of hatchery propagation" from the implications of Christie et al 2014a. We added "evidence for genetic basis to fitness costs of hatchery propagation, replicating results of Hood Canal steelhead study (Araki 2009)" to the implications of Ford 2016. We agree that it is an important point that evidence for the heritable fitness loss is limited to two studies. We revised the text of the Emerging Science section to read, "A consistent observation of lower RRS of hatchery-origin fish provided basic conceptual support for fitness costs of hatchery propagation (Christie et al. 2014a), though unequivocal evidence for a genetic basis to this pattern remains rare. , and is limited to two studies (Araki et al. 2007; Ford et al. 2016)." We also added a paragraph addressing the scarcity of unequivocal, population-scale empirical evidence for a genetic basis to fitness loss.

R7 Edit (p132): Replace "(Janowitz-Koch et al. 2019) with "(Janowitz-Koch et al. 2019; Hess et al. 2012).

We added Hess 2012 to the table.

R7 Edit (p132): Modify the implication for the (Janowitz-Koch et al. 2019; Hess et al. 2012) reference to read "Hatchery programs with high pNOB and PNI can provide conservation benefits and limit negative fitness effects on wild fish," instead of "Hatchery programs with high PNI can provide conservation benefits."

Good suggestion, change made.

R7 Edit (p132): Modify the implication for the Waters et al. 2015 reference to read "Integrated broodstock management with high pNOB and PNI limits divergence of hatchery from natural populations" instead of "Integrated broodstock management limits divergence of hatchery from natural populations."

Good suggestion, change made.

R7 Edit (p132): Replace "inimize" with "minimize" within the implication listed for Willoughby and Christie 2017.

Change made.

VE Edit (p133): Replace "supplemention" with "supplementation" for the implication of the Venditti et al. 2018 reference.

Change made.

Figures

R4 comments (p135): R4 highlights "A) unmarked Chinook salmon (N = 15 stocks), B) marked Chinook salmon (N = 15), C" and states "Important figure to show the level of exploitation. Surprised it so consistently high on unmarked fish…really high for chinook. Worth indicating that the bars are medians or means?"

We added a description of the lines, boxes and whiskers plus points to the figure caption.

R4 comments (p136): R4 highlights the word "investigation" at the end of the figure text and asks "Could you overlap with the zone of management? Presumably management done at the watershed scale? Would show low precision and high potential impact."

This is an interesting idea, and we considered modifying the figure. Hatchery management decisions likely span the range of

geographic scales. Some initiatives, such as increasing hatchery production to serve as killer whale prey, likely have a large geographic scale, while other decisions (e.g., specific release site) are made more locally. In the end, we decided not to modify the figure to avoid added complexity, and maintain a tight focus on the informational inference from research studies. Regardless, we agree with the reviewer that comparing the spatial scale of biological processes and management has value.

R7 Comment (p137): With regards to Figure 3, R7 states "Good to see higher values of PNI included in this figure. An important question for many integrated programs is: how high must pNOB be in order to maintain high mean relative fitness? This is a step closer to addressing that question."

Thank you for the confirmation.

Appendix 1: Puget Sound Chinook Salmon Demographics

R2 Comment (p140): With respect to the sentence "We provide a full accounting of adult demographics including harvest rates, total hatchery-origin plus natural-origin return to the river, total abundance of naturally spawning Chinook salmon, and the proportion of naturally spawning Chinook salmon produced in hatcheries (pHOS)," R2 asks if "Would be helpful to include a definition of harvest rate. For example, is this an adult equivalent exploitation rate that equals the reduction in the terminal run due to all fisheries on all ages coastwide including bycatch? Or the proportion of maturing run harvested directly in Puget Sound fisheries? Or something else?"

We added two sentences describing the harvest rates as inclusive of adult equivalents of immature salmon and incidental (bycatch) mortality.

R2 Comment (p140): With regards to the use of "non-natural" in the sentence "We also include non-natural hatchery stocks that are not associated with a historical population of Chinook salmon," R2 suggests "deleting or using a more neutral term like 'independent.'"

Good suggestion, change made.

R2 Comment (p140): On the statement "Because fisheries are typically constrained only by impacts to natural populations, non-natural harvest rates were not available for hatchery stocks" R2 asks "Is this really true? Aren't there CWT groups associated with those releases that could be used to calculate exploitation rates?"

We dug a little deeper into the harvest estimates, and the reviewer is correct: exploitation rates are estimated for independent hatchery populations not associated with natural

populations. However, our intent was to focus on harvest of natural populations and have presented those numbers in the table. Marked hatchery stocks associated with natural populations would have a different harvest rate than their associated natural populations due to retention in mark-selective fisheries. We make that point in Figure 1. To avoid confusion in our simple, concise demographic review, we chose not to present exploitation rates on any hatchery stocks, including the independent hatchery stocks not associated with natural populations, to ensure all harvest rates represent only impacts of natural populations. We have revised the text of the appendix accordingly, including explicitly stating that we only present harvest rates for natural populations.

R2 Edit (p140): Replace statement "Because fisheries are typically constrained only by impacts to natural populations, non-natural harvest rates were not available for hatchery stocks" with "Because fisheries are typically constrained only by impacts to natural populations, harvest rates were not available for hatchery stocks not associated with a natural population."

We revised this passage based on our response to the previous comment,and have avoided use of the term "non-natural hatchery stocks."

R2 Edit (p141): Replace "non-natural" with "independent" in the sentence "Approximately 40% of the total adult return was associated with independent hatchery stocks"

We revised this sentence to read "Approximately 40% of the total adult return was associated with independent hatchery stocks not associated with a natural population."

Appendix 2: Hatchery Effect Parameter Effect Described

No comments received from the scientific panel.

Appendix 3: Comparison of pHOS and pNOB Broodstock Management Options Across a Range of Parameter Values

No comments received from the scientific panel.

Appendix 4-The Demographic Model

R7 Comment (p152): On scenario 3, R7 states and asks "I appreciate the additional scenarios that were added to the revised draft. This scenario 2 is helpful to begin addressing the question for integrated programs with high pNOB. However, the drop to pNOB of 0.5 after the first generation makes results less helpful. Please consider a scenario

that addresses the question: how high must pNOB be in order to maintain high mean relative fitness for integrated hatchery programs that start with pNOB=1.0 and pHOS does not change?"

This is an excellent question, and an important discussion, but beyond the scope of this report. As we stated at the beginning of Appendix 4, the demographic model needs to be rigorously tested and reviewed before it can be used beyond the general exploratory results presented in this report. Nevertheless, prompted by R7's question, we did compare Scenario 2 with a pNOB = 0.6, as presented in this report, to a scenario with pNOB = 1.0. The results do not differ much: fitness improves from 0.70 to 0.81, and natural recruitment from 590 to 686, but there would be fewer fish available for harvest, with hatchery recruitment declining from 3420 to 1434. These results assume that pHOS can be controlled, but if pHOS cannot be controlled, even if pNOB = 1.0, the results are near zero wild fitness, low natural recruitment, and high a hatchery recruitment. The model is also quite sensitive to "hatchery effect" *sensu* Appendix 2.

R7 Comment (p152): With regards to the statement "Scenario 3B provided a slight improvement over baseline conditions, with increased natural recruitment, increased pNOB and decreased pHOS" R7 asks "What about effective pHOS?"

See several comments above in "hatchery reform" section concerning effective pHOS.

R7 Comment (p152): With regards to the statement "Finally, Scenario 3C provided recovery for the natural spawning population by severely reducing pHOS" R7 asks "If effective pHOS is low, would this achieve the same effect?"

See several comments above in "hatchery reform" section concerning effective pHOS.

R7 Comment (p155): With regards to Figure A4-1, R7 states and asks "This scenario is helpful to begin addressing the question for integrated programs with high pNOB. However, the drop to pNOB of 0.5 after the first generation makes results less helpful. Please consider a scenario that addresses the question: how high must pNOB be in order to maintain high mean relative fitness for integrated hatchery programs that start with very high pNOB?"

This is a similar question asked above (first question under Appendix 4), and our response here is essentially the same as that above. However, here, there is no change in the results from a pNOB = 0.5 to a pNOB = 1.0: fitness remains nearly zero, natural recruitment roughly 30, and hatchery recruitment near

the maximum of 15,000. High pHOS rules the day. One point may require some explanation. The model starts with targeted pHOS and pNOB in generation 2. By definition, in generation 1, pNOB = 1.0 and pHOS = 0.0 because there are no hatchery-origin recruits in generation 1. In generation 2 and beyond, pHOS can increase above the target value to prevent extirpation. pNOB is maintained at the target value as long as it does not remove more than 80% of the natural-origin population. However, if the population drops below 500, the model limits the number of natural-origin fish removed to no more than 20% of its population. In other words, we can set target pHOS and pNOB, but unlike the Ford (2002) phenotypic model with fixed pNOB and pHOS, the model here determines pHOS and pNOB.

VE Comment (p155): Check subject verb agreement for the sentence "Natural and hatchery recruitment curves is total recruitment from the wild and hatchery environments, respectively."

We revised "is" to "are" in this sentence.