

## **SCIENTIFIC REVIEW PANEL COMMENTS ON “A REVIEW OF HATCHERY REFORM SCIENCE IN WASHINGTON STATE”**

### **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.”

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.

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.
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.”

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.”

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.”

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

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

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

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

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

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

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

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

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.”

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.”

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.”

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

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.”

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.”

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

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.”

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).”

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.”

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

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?”

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

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

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.

## **Introduction**

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

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.”

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

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

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

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

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

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

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.”

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?”

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

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?”

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

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

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.”

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

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.”

## **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?

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

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.”

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.”

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?

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.

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

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.”

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 read the splash page for the main gist. Final notice in the Federal Register [here](#).”

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.”

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.”

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

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

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

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

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).”

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)”

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.”

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

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

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).”

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

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 salmon in the Yakima River,” after the sentence ending in the words “stable natural-origin returns.”

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.

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

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

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

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

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).”

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.



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

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

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).”

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

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).”

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

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.

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.”

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

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

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

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.”

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

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.”

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

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.

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

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.”

R8 Edit (p24, line 529): “than”

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.”

## **Risk of Hatcheries**

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

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.

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

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.”

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.”

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?

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

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.”

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.”

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  $N_e$  has increased with  $N$ ?”

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?

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?

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).”

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.”

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.”

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

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

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.”

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.”

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.

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.

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).”

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.”

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

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.”

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*) psmolts to rebuild wild populations in Oregon coastal streams. *Canadian Journal of Fisheries and Aquatic Sciences* 43:2443-2449.

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

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

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

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

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

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

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.

R8 Edit (p37, line 797): “ultimate”

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

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.”

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

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.”

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.

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.”

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.”

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

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

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.”

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

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

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

R3, R8 Edit (p45, line 977): Replace “proportion” with “proportion.”

R8 Edit (p46, line 992): “negatively”

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

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.”

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

R8 Edit (p47, line 1009): “al.’s”

R8 edit (p47, line 1016): “Alaska”

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

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

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

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

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

## **Hatchery Reform**

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

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

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

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

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

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

R1 Comment (p52, line 1117): On the word “wild,” R1 asks “‘Natural’ to be consistent with your glossary?”

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)?”

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

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.

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

R2 Edit (p54, line 1166): Replace “populations’ ” with “population’s.”

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.”

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 Elwha, 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.”

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

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

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

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.”

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

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

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

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

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

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

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.”

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.”

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



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.”

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.”

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.”

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.”

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.”

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?”

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

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.

R8 Comment (p71, line 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.

R8 Comment (p72, line 1542): Hmm... 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?

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

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

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?"

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

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."

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

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

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

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

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..."

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

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

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

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

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.

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?”

R8 Edit (p81, line 1730): transfers were

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

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

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

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?”

R8 Edit (p83, line 1778): “timing...is”

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.”

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

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.”

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.

R2 Edit (p85, line 1833): Insert “are” after “However, they.”

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

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

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

R3 Edit (p86, line 1850): Replace “The Salmonid..” with “In addition, ‘The Salmonid...”

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

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).”

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.”

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.”

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

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

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?”

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.”

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

## **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.”

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.”

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)."

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)."

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)."

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

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."

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)."

## **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."

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.

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

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)."

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.”

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

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.”

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.

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?

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

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

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.”

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).”

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.”

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.”

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

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.”

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.’”

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.”

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.”

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.”

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.”

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...”

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

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).

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

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. Sva sand, eds. *Stock Enhancement and Sea Ranching: Developments, Pitfalls and Opportunities*. Second Edition, Blackwell, Oxford.

## **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.”



## 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.”

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.”

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

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.”

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.”

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

VE Edit (p133): Replace “supplementation” with “supplemental” for the implication of the Venditti et al. 2018 reference.

## 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?”

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.”

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.”

## **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?”

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.’”

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?”

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.”

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

## **Appendix 2: Hatchery Effect Parameter Effect Described**

No comments.

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

No comments.

### **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?”

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?”

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?”

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?”

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.”