<u>Present Day Hatchery Practices & Reform</u>

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Harvest abundance and policy are progressively transforming recreational and commercial angling opportunities to a reduced state. But, as license holders and harvesters we are the stakeholders funding most of the science and operations that provide for that harvest. Years ago I attended an Oregon Department of Forestry public meeting in Portland at which the speaker was asked by an audience member what percentage of DOF revenue was derived from timber harvesters to which he replied 90%. A follow up question queried what percentage of the department's operating budget was devoted to facilitating harvest to which he again stated 90%. Seems like the loggers are getting their money's worth. Not so the sports and commercial anglers.

In Oregon approximately 70% of ODFW revenue is derived from license sales, ad valorem tax on commercial harvest, and excise taxes on all matter of angling tackle and accessories from boats to barrel swivels. As a past board member of the Oregon Hatchery Research Center we discussed facility operating budgets annually. 100% of the budget dedicated to fulfilling its mission is derived directly from line items within ODFW. Science outside the mission is funded from other sources. The mission is to study the differences between hatchery origin fish and natural origin fish in order to manage for those differences to facilitate conservation [protecting wild genes and diversity] and harvest. Therefore, that important science is funded from 70-100% by anglers. Are we getting our money's worth from the science or the policy?

To help frame this lengthy report I will share a bit of my background and core beliefs about hatchery management and salmon and steelhead resources in Oregon and Washington. I have been a Lifetime Member of CCA since Gary Loomis first presented it as a solution to secure sustainable commercial harvest methods on the Columbia River about 15 years ago; and have been a Lifetime Member of NW Steelheaders for several years, authoring several articles for their past quarterly magazine. But, it is my interest in the current deployment of hatchery resources and their future jeopardy that fuels my passion for resource advocacy now.

From my work on the OHRC board I still support the notion that science may lead us to the apex of the hatchery/wild conundrum. It requires more effective oversight and allegiance to the mission, but that's a discussion for later. I also believe for the most part staff at ODFW and WDFW are well-informed, intelligent people trying to solve what may be an unsolvable problem, providing sufficient abundance of hatchery stocks to provide the harvest opportunities our stakeholder group ***desires*** [which should now become ***demands***] while protecting wild genetics in the face of federal constraints under the ESA listings.

It is with this background I was recently compelled to read *<u>"A Review of Hatchery Reform Science in Washington State",</u>* published January 23, 2020. This 160-page report was an effort to ascertain if a new hatchery strategy in Washington state is warranted, based on fresh science emerging in the 10+ years since they last adopted recommendations to balance the risks and benefits of alternative hatchery practices from the 2009 **<u>Hatchery Scientific Review Group</u>** [HSRG]. Their focus is Pacific salmon and steelhead hatcheries; and it assesses the *<u>science</u>* of hatchery reform rather than the *<u>implementation</u>* of it. It is a long hard read, but provides a comprehensive primer about hatchery policy, practices and reform. As advocates for sports anglers' desires ne demands we need to speak from an informed position on current practices and the science behind them.

It commences by acknowledging that the economic, sociocultural, treaty and mitigation ***benefits*** of hatcheries have received far less attention than the ecological and genetic ***risks*** of hatcheries in the research; but then goes on to declare that said benefits "are *not* the focus of this paper", thereby perpetuating the bias of prioritizing hatchery risks over hatchery benefits.

The fundamental focus of hatchery reform in the state of Washington and elsewhere has been to manage gene flow between hatchery origin populations [HO] and their companion natural origin populations [NO]. Why is that important? In order to minimize the perceived fitness loss [defined as a reduction in reproductive success or number of offspring], as well as a loss of genetic diversity within and among populations. Fitness decline is due to selection for traits favorable in the hatchery but harmful in the wild where selection ["domestication'] is then expressed when both stocks - hatchery and natural - spawn in the wild.

HSRG devised a method for establishing fitness loss thresholds by creating a risk metric for hatchery operations called pHOS [proportion of hatchery origin spawners]. Its counterpart pNOB [proportion of natural origin fish used in hatchery brood stocks] is not a measure of risk but is often used in combination with pHOS to determine the appropriate size of the hatchery program [number of juveniles released.] Empirical research reveals that relative reproductive success [RRS] or the comparative numbers of offspring between HO/HO pairings, NO/NO pairings and HO/NO pairings show that ***"hatchery programs employing 100% natural origin broodstock can provide demographic conservation benefits while maintaining genetic diversity with minimal or no apparent genetic fitness loss."*** They conclude that reducing pHOS and increasing pNOB should be *"fundamental goals in any hatchery reform management action."* Later in their report they suggest reducing pHOS is the foremost objective of program size management. But, by reading the bolded phrase above you may alternatively infer that increasing pNOB might be a better facilitator of fitness control. For additional insight into the use of pHOS and pNOB as management tools I suggest you review Craig Busack's exceptional work on PNI or proportional natural influence. Craig is recently retired from NOAA fisheries.

It is disconcerting that initially the Washington agency defined ***"Hatchery Reform"*** as *"institutionalized changes to hatchery programs* ***intended to reduce risk to natural populations,"*** with no mention at all about the importance of sustaining fisheries with these reform campaigns. Though they do subsequently acknowledge that reform is intended to minimize NO risk while sustaining fishing opportunities, you can easily perceive these two goals are not twins. Also, it is important to understand that hatchery reform not only encompasses breeding, rearing and release protocols, but a whole suite of social values and goals [addressed comprehensively by Dr. Robert Lackey in his work on *Science and Salmon Recovery*] that create a hierarchy of core societal imperatives where salmon recovery holds a subordinate position to numerous other social requirements.

WDFW points to the indisputable success of the reintroduction of Coho in the Columbia River basin, causing significant natural spawning [Galbreath et al. 2014], ***"with some evidence for adaptive evolution of hatchery broodstock*** *[Campbell et al. 2017]."* Additional examples of beneficial hatchery supplementation were cited in the Clearwater River [Sharma]; and the Yakima River [Fast et al. 2015], where adult natural origin returns were insufficient pre-supplementation to reach juvenile carrying capacity; yet benefited post supplementation with increased redds, spatial distribution and harvest without distressing wild returns. The Clearwater and the Yakima programs relied exclusively on natural origin broodstock. But, even here the Washington agency cautions us about the value of hatchery supplementation by advising *"this benefit must be carefully balanced against the longer term risk of fitness decline."* In their opinion the ultimate conservation goal is to increase natural origin adult abundance *["in* *generation three and thereafter"]*, rather than merely increasing the number of hatchery origin spawners in the wild *["generation two"]*.

They also note that generation one hatchery origin Johnson Creek [Idaho] Spring Chinook produced 2.5 times more natural origin grand offspring [in generation three] while using 100% natural origin brood in the hatchery than generation one wild fish, as cited in Janowitz-Koch et al. 2019. However, this benefit is diluted in Venditti et al. 2018, which examined 13 supplemented and 14 reference chinook populations before, during and after supplementation in the Clearwater and Salmon basins in Idaho; and discovered that abundance decreased to pre supplementation levels after terminating supplementation.

After reviewing four studies, two of which reveal ***enhanced natural origin abundance*** through hatchery supplementation [Berejikian and Van Doornik 2018, and Janowitz-Koch et al. 20219]; and two that demonstrate ***limited conservation benefits*** to natural origin abundance [Scheurell et al. 2015, and Venditti et al. 2018], WDFW concludes that before relying heavily on the conservation effect of supplementation, a better understanding about the ecological cause [including degraded or inaccessible habitat] of population declines must be achieved. Thus by shifting the focus they diminish the benefit of supplementation, and effectively establish an untested hypothetical barrier to expanding supplementation programs. To some, this may be inferred as anti-hatchery bias?

The preceding was an overview of the benefits of hatchery programs. Next follows a discussion about the risks of those programs.

Genetic Risks:

Because hatchery programs subsidize fisheries that encounter incidental contact [**risk**] with natural populations and impart post-release mortality [though this is deemed to be low impact] to ESA listed stocks in mixed stock fisheries, they ponder various risks that hatcheries play by exposing natural stocks to harvest impacts. They reference two studies [Bendock and Alexandersdottir 1993; Nelson et al. 2005] to estimate post release survival in marked select fisheries, but acknowledge more study is needed to improve estimates of incidental mortality in mark-selective fisheries. Further, not all fisheries are mark-selective. Indeed a relatively small proportion of estimated exploitation rates on unmarked Puget Sound Chinook render this a less than optimal management tool to restrain HO abundance in order to limit NO mortality.

WDFW acknowledges an **asymmetry risk** [their word implying inequality] between conservation benefits to natural origin stocks and harvest opportunity on hatchery origin stocks exists; which is exacerbated by curtailing the latter to protect the former. If, therefore, harvest managers restrict fisheries to meet conservation targets, they may forgo a large scale harvestable abundance and thus create greater asymmetry of benefits. Large scale hatchery production amplifies this asymmetry. It's challenging to follow their intent here, but it seems unlikely they are arguing for larger production programs.

Their final point about the risks imposed by hatcheries as they facilitate fisheries looks at basin harvest plans implemented to achieve harvest and conservation targets, and how the presence of HO stocks spawning in the wild can "mask" [McClure et al. 2003; Johnson et al. 2012] the performance of NO spawners. Again, as hatchery programs scale, this **masking risk** scales.

To describe the **genetic risks** of hatchery propagation, WDFW relies on the following studies: Busack and Currens 1995; Bekkevold et al. 2006; Naish et al. 2008; Glover et al. 2017; Wither et al. 2018.

Two broad categories of genetic effects from hatcheries are loss of diversity within and among populations; and maladaptation or domestication. Both are produced solely by interbreeding between hatchery origin fish and natural origin fish.

The loss of within-population diversity [**risk**] is important to the long term adaptability of salmonids in the face of a changing environment caused by climate transformation and/or human activity [Lande and Shannon 1996; Agashe 2009; Forsman and Wennersten 2016]. Populations are finite and lose variation over time from generation to generation because not all adults contribute to the next generation; but they often replenish that lost variation naturally through mutation and the migration of other populations. Nevertheless, that migration benefit depends on the diversity of the immigrants; and HO immigrants are deemed to lack the diversity needed to support natural adaptability, especially if hatchery spawning partners share a common ancestor [not completely absent in the wild but less frequent than in the hatchery]. Here, it is important to note that "if the subset of adults used as broodstock are not representative of the diversity in the population, then diversity may be lost." In other words, the smaller the sample of adults used as broodstock, the greater the likelihood diversity may be lost. To this author that argues for increasing the proportion of NO fish in the brood. Here, they cite Van Doornik et al. 2011 as uncovering *"little evidence for reduced genetic diversity in supplemented populations."*

Among-population diversity facilitates evolution of the species [Greene et al. 2010; Schindler et al. 2010; Schindler et al. 2015; Braun et al. 2016]. So, the loss of it is considered a **risk** of hatchery supplementation. Here, they rely heavily on scientific references to *genetic homogenization*, *deleterious alleles*, *co-adapted gene complexes*, *outbreeding depression*, and loss of *beneficial allelic combinations through introgression and recombination* to posit that hatchery propagation causes all these bad things. In all candor I am unable to comprehend the biological activities here as they influence among-population genetic diversity, but will accept that this gene flow consequence from hatchery operations can occur when broodstock, eggs or juveniles are transferred between river basins, or when HO stock stray [pHOS] and then interbreed with local NO residents of their new locality. Earlier, it was stated pHOS control is more important than expanding the use of pNOB in controlling lost diversity and fitness of NO stocks during interbreeding. Yet, here they state: *"However, the ultimate impacts of hatcheries on maintaining or increasing genetic homogenization through straying among populations are poorly understood."* Is anyone else alarmed at how courageously they rely on *"poorly understood"* science for policy?

Domestication [**risk**] is a term used for genetic adaptation to, or trait selection in, the hatchery environment, and may lead to fitness decline of offspring from HO/NO pairings in nature [Christie et al. 2012a; White et al. 2013; Hagen et al. 2019]. To assess the impacts of Domestication in HO stocks spawning naturally the relative reproductive success [RRS] of offspring of HO/HO, NO/NO and HO/NO pairings is investigated. Araki et al. 2009 is cited as the sole long term study of RRS in steelhead; and it uncovered *"multi-generational declines in RRS from hatchery exposure that carried over to the natural-origin offspring of HO parents that spawned in the river."* Also, shorter term studies were citied supporting this decline: Araki et al. 2007; Williamson et al. 2010; Theriault et al. 2011; Ford et al. 2016; Janowitz-Koch et al. 2019. But, complicating the merits of these studies for policy decisions is the acknowledgement that *"Interpretation of RRS studies is often made difficult by the unknown degree of hatchery introgression prior to the onset of the study."* There may be little difference in fitness between HO fish and NO fish if the latter's RRS has been effected through generational HO/NO hybrids [introgression] due to straying and/or pHOS [Willoughby and Christie 2017].

From the WDFW report we read that *“There is a major gap in understanding how growth regimes in production-oriented, high efficiency hatchery programs intended to provide harvest opportunities affect genetic and ecological risks.”* In other words, we don’t know the cause of domestication in hatcheries. From my work in fish advocacy I have personally encountered scientists tasked with discovering this cause who admit they don’t know the answer, and express little likelihood of making that discovery during their journey.

Domestication studies are time consuming and expensive; because it must first be determined which traits are under selection in the hatchery. Then, which of those traits are heritable. Then, which of those heritable traits under selection are associated with RRS. So, just to determine the proper genes to study takes several salmonid generations.

Ecological Risks:

Of interest here is the degree and regularity of ecological interactions between hatchery and natural fish prompted by **competition**, **predation**, **disease**, **facility effects**, and **density dependence**. Studies quoted as revealing ecological risks are Einum and Fleming 2001; Kostow 2009; Tatara and Berejikian 2012. The latter finds that the number of hatchery releases affect the frequency of hatchery/natural competitive encounters for food, rearing space, and spawning space; and further they affect the total hatchery plus natural combined abundance on the habitat carrying capacity. We have studied these ***competition*** mechanisms in freshwater life stages but know little how they are manifested in the marine life stage, though biologists do surmise that *“substantial expansion of hatchery production had the potential to reduce the abundance of natural populations due to finite marine carrying capacity,”* [Peterman 1978; Beamish 1997]. Once again, a poorly understood scientific hypothesis is offered as support to curb hatchery scale.

Regarding ***predation***, biologists acknowledge that salmonids are piscivorous [your word for the day…meaning feeding on fish]; and yearling HO smolts may consume smaller NO subyearlings. The studies cited in this review did not quantify population scale impacts on natural stocks due to predation, nor evaluate it as a risk factor. While we often hear that large hatchery releases will trigger avian, mammal or piscine predation on natural stocks, this study concludes that we really know and understand too little about the short term or long term affects to natural stocks of predation associated with hatchery program releases for it to be a significant consideration for policy.

While ***disease*** avoidance is an ongoing focus of study, to manage for disease as a risk factor in the context of hatchery-wild interactions would require better data about the likelihood and magnitude of injurious outcomes than presently available.

Hatchery facilities certainly can impart effects to the local environment **[risk]** that may be important habitat intrusions. Examples are weirs or diversion dams possibly impeding migration in both directions altering redd distribution and spawning, and causing increased injury or mortality due to handling. Also, facility effluents may impact nutrients and stream temperatures downstream of the discharge point. While, facility effects are regulated by federal, state and local jurisdictions “***empirical assessments of population-scale impacts to natural populations are generally rare***,” making it difficult to generalize their impact. Yet, this report states these effects are not inconsequential; which demonstrates another example of unsubstantiated conjecture possibly influencing program scale.

An important admission revealed in this report is that population-scale studies of hatchery effects "often cannot separate ecological vs. genetic mechanisms" and they cite the divergent conclusions drawn in the Kostow and Zhou [2006] study and the Courter et al. [2019] work. The former published a negative relationship between NO productivity in the Clackamas OR River winter steelhead population and the abundance of summer steelhead HO spawners and concluded the combined hatchery plus natural population exceeded the basin's carrying capacity; and led to ecological competition between the two stocks leading to the observed reduced NO productivity. They wisely disregarded genetic effects because data shows there was minimal interbreeding between the two stocks. Courter et al., however, used an updated Clackamas dataset, a different model and new comparisons to neighboring populations to contradict the earlier study, by concluding poor ocean survival conditions during the period of Clackamas River hatchery accrual was undiscovered by Kostow and Zhou [2006] leading to their flawed conclusion.

It is difficult to separate hatchery program effects from climate-scale environmental factors influencing NO productivity because they both change slowly over time, thus limiting valid comparisons between years. Therefore, large scale experiments where abrupt changes in hatchery management are manipulated are needed to fully understand hatchery influences. They cite Jones et al. [2018] as an example of a large scale manipulative study. And, significantly it reports evidence of *"increased natural origin abundance and diversified spawn timing in a coastal Oregon Coho salmon population two generations after the termination of Coho salmon hatchery releases."* It is often suggested by informed sports anglers [Jack Smith a well-respected Oregon fishing guide comes to mind] that greater use of population level experiments is indispensable to better understand HO/NO interactions and comprehend the impacts of hatchery programs. We hear, however, they are much more difficult to execute and therefore less frequently employed than case by case studies.

Broodstock Management – reducing the risk of fitness loss from domestication.

Eighteen pages of their 160 page report dealt with broodstock management. Many studies are cited as foundational to this topic, but it is the model developed in Ford [2002] that established the core genetic components of the HSRG "Standards and Principles" in 2014. It modeled the mean relative fitness of a wild population affected by a hatchery population by using several formulas, ultimately arriving at Equation 4 where PNI = pNOB ÷ (pNOB) + (pHOS). PNI is dubbed Proportionate Natural Influence, and it has become the primary metric and management goal for integrated programs, where two diverse [natural and hatchery] populations are managed as a single unit with designed gene flow from one to other. pNOB represents gene flow from the wild into the hatchery, and pHOS represents the gene flow from the hatchery into the wild.

It is important to understand here the difference between ***segregated*** hatchery programs and ***integrated*** hatchery programs. The intent with the former is to establish two separate populations with zero or at least limited gene flow between them. Here pNOB is zero, and pHOS is limited by controlling spatial and temporal interactions and straying. The latter represents the opposite strategy from segregation where the goal now is to create a single population that exists in both environments [hatchery and wild]. Risk is mitigated by achieving a specific PNI. If the goal is conservation, most hatcheries employ integrated broodstock programs. If the objective is harvest, segregated broodstock programs prevail.

Using the Ford model, HSRG then established broodstock management goals where **PNI = 50%** is the fundamental target because it is the line between greater hatchery influence [PNI<.50] and greater natural influence [PNI>.50]. They found that to maintain this **target goal of PNI = .50**, in integrated programs the use of pNOB is relatively inefficient when pHOS exceeds 30%. Again, integrated programs are primarily focused on conservation [some call it *preservation* of wild genes]. But, in segregated programs used for harvest objectives and where the use of wild broodstock is 0, then a target PNI of 50% is threatened with pHOS as low as 5% because of the absence of wild genes in the broodstock.

As explained above, mean relative fitness decline is related to pHOS, pNOB and PNI. And, while it responds to changes in PNI, according to mathematical manipulation, the extent of the response is more profound with changes in pHOS than in pNOB, though the greatest impact is achieved when you decrease the former and concurrently increase the latter. In seasons of low recruitment [offspring] of natural spawning populations, the use of pNOB is constrained.

Where there is significant difference between the hatchery environment and the wild environment, HOS [hatchery origin spawners] demonstrate low fitness, and create few natural origin recruits. Therefore, under conditions of environmental variance the longer a population relies on HOS [i.e. where pHOS remains high] the less likelihood that population will generate NOS [natural origin spawners] even when degraded habitat is restored [i.e. dam removal]. There is hope, however, because while hatchery and wild environments diverge greatest when strictly hatchery origin parents are used as brood and with possible inbreeding, and those resulting offspring are then conveyed to foreign watersheds for release, we are observing a diminishing [though not zero] use of these practices today.

Waters et al. [2015] is cited as the best comparison of integrated vs. segregated hatchery management strategies. In their study of Yakama River Spring Chinook they found that integrated programs generally have a higher number of breeders [broodstock] than segregated programs. This is consequential because Allendorf et al. [2013] divulges that large populations tend to retain greater genetic diversity than small populations, and therefore by conclusion hatchery programs spawning a larger number of fish will tend to conserve more genetic diversity than ones spawning fewer fish.

-HATCHERY REFORM-

Hatchery reform is prompted by the need to reduce selective differences between the hatchery and natural environments, control pHOS, implement greater use of pNOB, sustain genetic diversity and design more effective monitoring programs. On this latter point I recall at a planning meeting for the Oregon Coastal Multispecies Management Plan it was expressed by senior ODFW management that funding monitoring was a perpetual problem, and like "deferred maintenance," was relegated to a subordinate position in the operational budget. So, the greater a program's effectiveness depends on monitoring the less likely that program will achieve its intended goal.

To assess current institutional changes to hatchery programs WDFW admits empirical observations of hatchery reform are rare, and "*addresses concepts rather than case studies*." Since the early 2000's guidance for hatchery reform has been mostly the responsibility of the HSRG; and their recommendations were the foundation of WDFW policy reforms adopted in 2009 as Commission Policy C-3169.

PROGRAM SIZE

The size of a hatchery program refers to the total number of juvenile fish released from the hatchery on an annual basis. Release numbers in turn regulate - via smolt-to-adult returns - the numbers of returning hatchery origin adults. And, of all the variables in the natural [freshwater plus marine] and hatchery environments and the life cycle stages of anadromous salmonids, the one variable policy makers and managers rely on most heavily to control the genetic and ecological risk imposed on natural populations by hatchery fish is **program size**.

They trust an empirical assessment of habitat carrying capacity to determine if the aggregate hatchery plus natural abundance exceeds it and if so what the biological consequences are for natural populations. Further, they propose that to best consider program size impacts it is necessary to evaluate not an individual program but the combination of multiple programs in a region. This is justified, they argue, because of the early marine phase of the fish life cycle, and aggregated effects of multiple programs in the ocean as offspring from several hatchery programs share common rearing habitat. One may see this as another example of poorly substantiated hypothesis influencing to an unwarranted degree policy Vis a Vis program size.

Controlling pHOS is generally achieved via the use of weirs, dams, mark-selective fisheries and ***curtailing program size*** [number of juveniles released]. I worry this rigid reliance on pHOS as a management tool constrains hatchery program size in a way that may not be consistent with our long term ability to use natural origin fish in broodstocks due to their distinct path to decline; especially after reading in this WDFW report how they portray weirs, dams and mark-selective fisheries as inefficient controllers of pHOS. By discounting three of the four regulators of pHOS, we can understand **their emphasis on hatchery program size for conservation**. But, as we reviewed in our discussion on broodstock management the use of pHOS as the primary metric to measure hatchery operations risk is imprecise at best. A highly placed ODFW staff member, when I asked him to please show me the science that validates 10% pHOS as the target risk goal responded thusly, "I can't, because that science doesn't exist. It is random, but generally accepted among the scientific and management communities." This is further evidence of the imprecision of pHOS when measured and used as a risk metric. Yet, it seems to be the primary control lever in establishing program size and hence the abundance of hatchery fish released and returned for harvest.

Sports anglers recognize an institutional bias against hatchery fish. I personally had another highly placed Oregon agency manager tell me *"If another hatchery fish was never produced it wouldn't hurt my feelings."* You may interpret the following WDFW statement as however it best fits your understanding of hatchery management partiality: *"Given the outsized importance of hatchery program size for conferring both ecological and genetic risks, we suggest a **rigorous justification for program size** is essential for implementing scientifically defensible hatchery programs."* We'll visit that phrase "scientifically defensible hatchery programs" shortly.

REARING STRATEGIES

A number of studies is cited to show the importance of creating a more natural like environment in the hatchery through water source [well versus surface], type of rearing structure, feeding and growth regime, rearing density, size-at-age release protocols, seasonal fluctuations in temperatures and food availability to better mimic natural conditions. In particular, Dittman et al. 2015 reveals that exposing HO fish to waters from the preferred spawning destination [surface or river water source mentioned above] during incubation demonstrates a better practice than offsite acclimation for homing cues.

RELEASE STRATEGIES

When, where and how hatchery fish are released pose genetic and ecological risks to natural populations. Encouragingly, release locations are being altered to eliminate out of basin releases. To reduce predation, releases are also being repositioned downstream from Chinook spawning habitat. The opportunities for ecological interactions between hatchery populations with natural origin fish are lower the shorter distance from release point to the confluence with a larger river or estuary. Further, concentrating hatchery releases in locations where no historic native populations were present is thought to reduce genetic risks of hatchery programs.

Acclimation ponds are used in conservation programs to encourage augmented adult spawning in areas where more natural spawning is desired, while also diminishing out of basin straying [Flagg et al. 2000; Dittman et al. 2010.]

Several studies are shown to produce conflicting results of the utility of volitional [timed] releases for survival or homing, where hatchery offspring emigrate from the hatchery voluntarily rather than by involuntary releases. As with the timing of release, the effect of selecting for size at release is also ambiguous to the degree to establish verified protocols.

MASS MARKING

The treatment of mass marking of all juveniles released from a hatchery in this study was limited, other than to state it is important to facilitate marked selective fisheries, estimate pHOS for controlling exchanges between hatchery and natural populations, and manage broodstock by controlling pNOB.

DISEASE MANAGEMENT

Risk metrics for disease associated with hatcheries are quite limited, because our knowledge is imperfect regarding pathogen transmission rates, host densities and rates of shedding infectious particles. They admit developing monitoring programs, which always seem to hold a subordinate position in operational budgets [my edit here], will be important in order to better evaluate disease dynamics in the context of hatchery-wild interactions.

ADAPTIVE MANAGEMENT

In order to implement adaptive management protocols, vigorous *monitoring* systems are needed to assess the ecological risks of hatchery management strategies. Yet, the study states *"we are not well positioned for this"*. Further, while Hatchery Genetic Management Plans [HGMP's] and NOAA biops [biological opinions] are the regulatory documents guiding hatchery management, *"in most cases they lack clearly articulated monitoring and evaluation plans for understanding and controlling hatchery risks."* Yet, it is precisely these monitoring and evaluation programs they stipulate *"are essential components of scientifically defensible hatchery programs."* I wonder if they are implying what we have currently are scientifically indefensible hatchery programs due to this lack of budgeted and implemented monitoring and evaluation.

-EMERGING SCIENCE-

This might be considered the Washington agency's summary section, as they recap here the science published since the 2009 HSRG recommendations for hatchery program management which they used as the foundation for Washington state policy. The modeling work of Baskett and Waples 2013 and the empirical studies of Christie et al. 2014a; Waters et al. 2015; Ford et al. 2016; Janowitz-Koch et al.

2019 all supported the HSRG recommendation to emphasize pHOS, pNOB and PNI in managing broodstock.

Lower RRS of hatchery origin stocks provides the conceptual support to stress the fitness costs of hatchery propagation [Christie et al. 2014a], though unequivocal evidence why this is "remains rare" [Araki et al. 2007; Ford et al. 2016]. However, while it is likely the lower RRS of HO steelhead is based on genetics [Ford et al. 2016]; the lower RRS of HO chinook studied in Hughes and Murdoch 2017 demonstrated little evidence this is genetic based, and rather caused instead by their poor spawning habitat. Once again, hatchery programs making full use [i.e. 100%] of wild broodstock revealed little divergence in RRS from their associated natural populations [Waters et al. 2015] with relatively low fitness costs [Janowitz-Koch et al. 2019].

Whereas case studies of Hood River, OR steelhead [Araki et al. 2007] and Wenatchee River steelhead [Ford et al. 2012] found population scale evidence of genetic based RRS deficiency, Wenatchee River chinook [Ford et al. 2012] and Umpqua River coho [Theriault et al. 2011} did not. But, they postulate the long term investment in research - at least three generations - needed to test the genetic component of fitness loss may explain those instances where they don't find it.

While Jones et al. 2018 demonstrates the favorable response of natural populations to major reductions in hatchery propagation, Courter et al. 2019 illustrates hatchery decreases are not a remedy in all situations.

-CONCLUSIONS-

Earlier we reference the importance of considering societal values in addition to hatchery protocols when evaluating reform alternatives. WDFW, to their credit, begin their concluding remarks with the reminder that present day hatchery literature impelling policy is dominated by biological studies; but the social, political and legal values of fisheries also need to be deliberated to derive a more precise risk/benefit model for hatcheries. Especially noted is the social value of fisheries which are subsidized by hatcheries, versus natural populations unmolested by fishing, to better define risk tolerance thresholds.

Hatchery reform alone cannot affect meaningful salmon recovery. Unless those factors causing the original and continuing decline in these stocks are identified and restructured, recovery is unattainable. While both HSRG and WDFW acknowledge the merits of an *"all H strategy"* [habitat, hydropower, hatchery, harvest], in their opinion "this goal has rarely been realized." But, recovery is unlikely without significant reform to all four.

Scientific research demonstrating demographic and genetic benefits from certain hatchery protocols are typically done on small scale conservation hatchery programs. And resultant reform actions receiving either conceptual or empirical support from those studies are *"difficult or impossible to implement in large scale production hatchery programs intended to provide harvest opportunities."* For example utilizing high pNOB despite low natural population forecasts becomes progressively more challenging as programs scale upwards. Further, they admit it is *"difficult to know with any degree of accuracy"* if practices based on these types of studies will achieve their intended goals.

Long term empirical studies about the efficacy of hatchery reform principles are what is needed, but they *"are absent in Washington State."* Yet HSRG bases their policy metrics on broodstock and escapement management on this incomplete support for their recommendations. Though reducing pHOS and increasing pNOB to achieve fitness gains in wild populations are corroborated by the research, they assert reducing pHOS, while more difficult to achieve, yields greater control over fitness loss than increasing pNOB, but this is evaluated via mathematical modeling and is *"not well established empirically."* Here, they conclude that specific PNI, pNOB and pHOS thresholds recommended by HSRG are not substantiated by differing population performances, "and thus are subjective," yet nonetheless are considered crucial to broodstock management on an official scale.

I purposely omitted mention of HSRG's *phased* approach to hatchery reform because the resultant matrix of life phases and program objectives requires designating those programs as a cross between integration, segregation, conservation, and abundance with additional phase qualifiers. But, I mention it here because I believe WDFW's comments on this approach reveals a more obtuse agency inclination toward conservation than even the federal constraints. NOAA recommended creating no PNI and pHOS goals for first two recovery phases of *preservation* and *recolonization*. The state agency recommends as reform, however, implementing those metrics to sustain wild fitness and recovery during these phases; stressing their desire for avoiding interaction on recovering wild populations by use of hatchery harvest programs. This is anti-hatchery bias, and it indefensibly perceives that wild populations are recovering.

The future of hatchery management depends on augmented funding; because what is needed in their view is a stand-alone monitoring and adaptive management plan for each hatchery program, ones that quantify risks and benefits and connect hatchery performance metrics to operational changes. Also, landscape-level replicated long-term manipulative before and after experiments are necessary. Without those, they argue, assessment of hatchery reform effectiveness is weakened. Scientists use surrogates such as RRS that in their words *"cannot be scaled"* to replicate population properties. And, because of these conceptual limits the efficacy of hatchery reform activities cannot be directly tested.

WDFW suggests that not enough time has elapsed since the 2009 HSRG recommendations were executed in order to evaluate performance that requires several generations to accumulate. If increased PNI can reverse decades of heritable fitness loss from undesirable introgression of hatchery origin populations, they suggest it may take just as long to observe improved performance from increased fitness.

Another limitation to hatchery reform is the potential magnitude of ecological impacts [i.e. competition and predation] to wild stocks, which are not very well understood and usually not measured or evaluated. Genetic studies have abounded in effort and investment with associated risk metrics such as pHOS established; but the demographic imbalance between HO and NO populations is unsubstantiated and is subjected to no such ecological performance targets. Also, marine ecosystem carrying capacity is rarely weighed when establishing hatchery program size, but could be essential to developing management strategy reforms.

Rather than give clear guidance for hatchery reform per se, they suggest a more generic need for better science, which requires a level of funding I doubt can be attained. As they stated earlier the ***benefits*** of hatcheries are less prominent compared to the ***risks*** of hatchery programs.

If you have followed along to this point, congratulations. I realize even my capsualization of this study is a hard read. As stakeholders funding the majority of science and operations we need to make our voice for abundance much louder than it presently is. What develops as we review this study is that population based manipulative studies are vital for understanding the impacts of hatchery reform proposals. I lack confidence they will be budgeted or implemented. Further, poorly understood concepts such as marine environment carrying capacity, and imprecise risk metrics such as pHOS and RRS are relied on substantively for policy.

The intersection of pHOS and program size is the driver for hatchery abundance and hence recreational and commercial harvest opportunities. The conceptual flaws with this dependence, in my opinion, stems from their disregard for the other constraints to pHOS such as weirs and dams at hatchery sites and the use of marked selective fisheries; as well as the expressed difficulty to measure this metric leading to its further imprecision as a management goal. This work summarizes an abundance of science researching a full pallet of hatchery impediments to wild diversity and fitness, yet boldly states the single best management lever to constrain such impacts is program size. Than why fund the other studies?

It is time for a paradigm shift to create best practices hatchery origin spawners in order to facilitate greater abundance or program size for harvest. We don't require more science for that. We know how to do it. Greater use of natural origin brood stocks to achieve 100% wild brood even in years forecast for low returns is the starting point. If we agree with end of century population forecasts for cold water anadromous salmonids we recognize they are on a path to decline – some postulate extirpation – that cannot be reversed. Using the term *recovery* to defend conservation over fisheries seems disingenuous because the most hoped for goal for wild stocks is merely to slow down this progression to elimination. And, by using wild parents in the hatchery we create returning adults that in a preponderance of the science demonstrates they can spawn in the wild with either natural origin mates or other hatchery origin partners with *no diminishing impacts to fitness or diversity*. We need to make unabridged use wild genes right now while we still have them rather than preserve them for the doubtful future of their *recovery*.