W A S H I N G T O N   T R O U T

COMMENTS ON
NOAA FISHERIES; SUSTAINABLE FISHERIES DIVISION

Proposed Evaluation of and Pending Determination on a Resource Management Plan (RMP), Pursuant to the Salmon and Steelhead 4(d) Rule

Submitted May 17, 2004 by
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RMP TITLE: *Puget Sound Comprehensive Chinook Management Plan: Harvest Management Component;* provided by Washington Department of Fish and Wildlife and The Puget Sound Treaty Tribes, April 8, 2004

INTRODUCTION

Washington Trout has reviewed the *Proposed Evaluation and Pending Determination on a Resource Management Plan (RMP), Pursuant to the Salmon and Steelhead 4(d) Rule* (PEPD) prepared by NOAA Fisheries, Sustainable Fisheries Division (#2003/01616; 4-8-04). We have also reviewed the relevant RMP, the *Puget Sound Comprehensive Chinook Management Plan: Harvest Management Component,* submitted to NOAA by the Puget Sound Treaty Tribes and Washington Department of Fish and Wildlife (co-managers). We have also reviewed relevant fisheries-management records and scientific literature pertinent to a review of both the RMP and the PEPD.

Washington Trout does not concur with NOAA's Recommended Pending Determination to grant take authorization to fisheries implemented in accordance with the RMP.

The PEPD should provide a critical evaluation of the proposed harvest management plan for Puget Sound chinook in order to assure the public that potential incidental harmful impacts to the threatened Puget Sound chinook salmon ESU will be held within limits safe enough to justify exempting the state and tribal co-managers from the ESA's Section 9 take prohibition. Such a critical evaluation should include the following, at a minimum:

- Provide a detailed explanation of key terms and concepts employed in the RMP that bear significant weight with regard to the assessment of the risk posed to populations of the listed ESU by the proposed harvest regime, and note where the RMP fails to do this;
- Provide a detailed and critical description and assessment of the key assumptions made by the RMP with respect to the potential impacts on populations of the listed ESU and the ESU as a whole of the proposed harvest regime;
- Clearly describe and characterize the several kinds of risk that the harvest regime may pose to populations of the listed ESU and to the ESU as a whole;
- Characterize relevant and critical uncertainties with appropriate methods and in sufficient detail so that the nature of the uncertainties can be clearly appreciated and the full range of potential harmful impacts on listed populations that may arise as a result of such uncertainties can be determined and the associated risk to listed populations fully characterized;
- Evaluate whether the proposed fishery regime(s) is(are) described in sufficient detail to permit a clear assessment of the extent to which the regime is risk-averse to potential impacts on populations of the listed ESU;
- Clearly describe and explain the extent to which the proposed harvest regime is risk-averse to harmful impacts on individual populations of the listed ESU and the ESU as a whole.
- Require the RMP to employ clearly articulated impact-threshold targets to be attained (or to be avoided), with clearly articulated management actions that will be taken in response when critical thresholds are not attained (or not avoided), and clear time frames for taking corrective actions and for achieving the desired targets of the corrective actions.

In general, we find that the PEPD fails to achieve the standards required of such an evaluation. The PEPD employs important legalistic and technical-biological terms and concepts without ever attempting to explain them or to note their use by the RMP itself. The PEPD fails to provide or to require of the RMP an adequate description or characterization of critical uncertainties involved in modeling chinook salmon population dynamics and harvest regimes that are likely to result in considerable direct and indirect risks to populations of the listed ESU and their recovery.

The PEPD holds that the RMP "would not appreciably reduce the likelihood of survival and recovery of the Puget Sound chinook salmon ESU."(PEPD, p.2.) This is an opaque standard, open to any number of subjective interpretations, including the most minimal. The PEPD lacks any clear attempt to quantify its interpretation of this critical concept and explain how it was applied to the RMP, making it impossible for a reviewer to objectively judge the appropriateness of the determination.

The PEPD introduces factors that it appears to consider extra-biological mitigation for various and specific anticipated risks to the ESU imposed by the RMP, including the notion of a "fair distribution of the burden of conservation" (PEPD, p. 5). It is not at all transparent how the PEPD has attempted to calculate an equitable distribution of conservation burden relative to PS chinook recovery across all stakeholder and user groups. The calculation would have to be extremely complex, requiring objective determinations of each stakeholder's relative responsibility in PS chinook decline, and then factoring and cross referencing a vast array of societal costs and benefits across every stakeholder group for any activity that could impact or be impacted by chinook recovery. It is not clear to Washington Trout that every reasonable calculation of each user group's "fair" portion of conservation burden would inevitably favor the acceptance of all the risks and uncertainties associated with the RMP. Without a clear and justifiable explanation of how NOAA determined the distribution of conservation burden, the application of that distribution in its evaluation of the RMP would necessarily appear to be arbitrary. In fact, the PEPD does not provide any legal justification for accepting or relying on such an apparently subjective and qualitative notion in the context of considering whether or not to exempt an action from the general ESA Section-9(a) take prohibitions. Regardless of how accurately conservation burden can be equitably distributed, or whether it is legally appropriate for NOAA to consider that distribution in granting take authorization, Washington Trout has identified and will discuss below at least one instance where NOAA's application of this concept in the PEPD is likely arbitrary and capricious.

Minimum Fishery Regime

In the discussion on management objectives and performance indicators, the PEPD introduces the RMP concept of a "minimum fisheries regime" that the PEPD characterizes as a "base level… which the fisheries would not go below." While the PEPD admits that the minimum fisheries regime and its associated Low Abundance Thresholds and Critical Exploitation Rate Ceilings are driven by policy rather than biological objectives, it endorses the co-managers "perspective" that the RMP "strikes a balance" between those objectives by addressing "conservation concerns, providing a minimum harvest opportunity, recognizing tribal treaty rights, and by representing a fair distribution of the burden of harvest."

Besides representing a balance steeply tipped toward accommodating the co-managers, it is not clear that all the identified parameters are appropriate or that they have been applied properly. The difficulties in objectively determining and applying an equitable distribution of "conservation burden" are discussed above. It is also not clear, despite the implication of the PEPD, that recognition of tribal treaty rights would necessarily mandate the acceptance of a base level of fisheries that must always be allowed, under any circumstance. This concept explicitly sanctions a continual, "minimum" level of fishing impacts on populations of chinook within the listed ESU under conditions when the co-managers expect these populations to be at very low levels of abundance.

It is of significant concern to Washington Trout that the RMP would propose, and NOAA would accept, that no conceivable circumstance potentially faced by the PS chinook ESU could warrant the complete restriction of fishery impacts on individual Management Units. Such a practice seems intuitively incompatible with both the listed status or the protection and recovery of the ESU. Consequently, one should expect that such a proposed practice would be clearly and carefully described and the reasons believed to justify such a practice carefully detailed. Neither the PEPD nor the RMP accomplish this.

While the RMP describes some details of minimum fisheries regimes by Management Unit, the only condition the PEPD appears to require for take authorization is the imposition of "a critical exploitation rate ceiling" (p.12). The critical exploitation rate ceiling is an upper limit on the southern US (SUS) fishery exploitation rate that would be "imposed" "when spawning escapement for a management unit is projected to fall below its low abundance threshold *or* if

Canadian fisheries make it difficult or impossible to achieve the RMP's rebuilding exploitation rate."(ibid.).

[The connection between the minimum fisheries regime and Canadian fisheries warrants further discussion on NOAA's conservation-burden concept. The relationship of the RMP to Canadian (and Alaskan) fisheries appears to be NOAA's most explicit attempt in the PEPD to "distribute" conservation burden "fairly." For many PS chinook Management Units, Canadian fisheries represent a significant (in some cases majority) percentage of the total impacts, constraining SUS exploitation rates in order to stay within the RMP's Rebuilding Exploitation Rates. When Canadian Fisheries are expected to push total exploitation rates above the RER, the minimum fisheries regime will be implemented even if the RER will still be exceeded. Likewise, if a Management Unit or Population falls below an identified Low Abundance Threshold, the co-managers will still be able to implement a "base level" of fisheries, regardless of Canadian impacts or the total exploitation rate imposed on the Management Unit.

The implied argument seems to be that no matter what the status of a Management Unit is, or what total impacts to the Unit are, constraint of SUS fisheries in order to meet the RMP's performance thresholds (or by implication, more biologically appropriate management objectives) would be "unfair," given the temporal and spatial advantages enjoyed by Canadian Fisheries for Puget Sound and other Washington stocks, and the large relative impacts on PS chinook attributable to those fisheries. Both the RMP and the PEPD note that the "management of Canadian fisheries is outside the jurisdiction of the co-managers."

But the PEPD fails to note that the distribution of impacts to PS chinook stocks in Canadian fisheries is not entirely outside the jurisdiction of NOAA Fisheries. ESA considerations re the distribution of harvest impacts would presumably have been no small component of negotiations to finalize the Pacific Salmon Treaty. We expect that NOAA was principally involved in those negotiations, and certainly had opportunity to review the treaty and its implications for SUS fisheries when it prepared its Biological Opinion on the PST, essentially approving the Treaty's Canadian-fisheries provisions. If an equitable allocation of harvest impacts across the whole spatial and temporal distribution of PS chinook during their marine life-history is a legitimate concern, NOAA had the opportunity to address that concern during those processes.

Put simply, NOAA does not have the authority to give away the same fish twice. It cannot approve a specific level of impact for Canadian fisheries, and then try to characterize that level as essentially intractable, in order to allow a bottom threshold of impacts that SUS fisheries will never be required to go below. Even if increased risk to the ESU is justified by an attempt to equitably distribute conservation burden, it is likely an arbitrary and capricious decision to allow it when NOAA has the opportunity (or has passed on the opportunity) to redistribute the conservation burden more equitably without placing added risk on the listed resource.]

The RMP alleges that the minimum fisheries regime would result "in a significant reduction in incidental impacts on listed chinook salmon", while providing "minimally acceptable access" to non-listed salmon species, including non-listed hatchery chinook salmon, for which harvestable surpluses have been identified…"(ibid). No clear evidence is provided that imposition of critical SUS ERs under the circumstances described would result in "significant" reductions in incidental impacts on members of the listed ESU. The description of the various SUS exploitation rates is simply confusing. Nowhere are the rates that are proposed to be in effect under conditions when the minimal fishing regime would be in effect translated into the numbers of listed chinook that would be expected to be caught and to escape given various pre-SUS fishery run sizes and compared to the numbers that would be expected to be caught if the fishery were conducted at the default “recovery” exploitation rates. A comparison of Table 2 with Table 5, for example, fails to make it clear what, if any, the exact change in fishery regimes would be under the minimum regime.

The co-managers further assert that under the minimum fishery regime, additional conservation measures on the part of the southern US fisheries *may* be considered by the co-managers solely at their discretion “where analysis can demonstrate that additional conservation measures in fisheries would contribute substantially to recovery of a management unit…” (ibid). As elsewhere in the RMP and in the PEPD no attempt is made to define or identify what would constitute a “substantial” contribution to recovery. This clearly indicates that there is no firm bottom line to the potential indirect harvest on listed stocks that may occur under the conditions when the minimum fishery regime would be in effect.

This lack of a firm, enforceable bottom line is particularly distressing, given that the minimum fisheries regime, no matter when or why it is imposed, *does not* appear to have a significant impact on mandating reductions in either the total exploitation impacts experienced by Management Units, or even the SUS and Pre-terminal SUS exploitation rates that the RMP does purport to regulate. For instance, Table 5 in the PEPD identifies the most likely anticipated total exploitation rate of 27% for the Skagit Spring Management Unit over the proposed period of the RMP, with an SUS exploitation of 14%, and a likely anticipated escapement of 1,845. The total exploitation is below the RER of 38% and anticipated escapement is well above the Low Abundance Threshold of 576 and slightly below the Upper Management Threshold (anticipated MSY-escapement, see below) of 2000. While this may be to the good (leaving aside questions about why reaching the upper management threshold through relatively small reductions in SUS impacts should be foregone), it seems to suggest that a 14% impact rate on Skagit Springs is accommodating relatively unconstrained SUS fisheries directed at non-listed species and populations, during periods of moderate abundance. This default-rate of impact is significantly lower than the Critical Exploitation Rate Ceiling of 18%. During periods when abundance levels approach or even pass the Low Abundance Threshold from above (apparently to even below some identified but undefined “point of biological instability”), or when the RMP’s RERs are projected to be exceeded, the minimum fisheries regime and its associated Critical Exploitation Rate Ceiling would appear to have the perverse effect of allowing the co-managers to retain take authorization even if they *raised* SUS impacts from 14% to 18%, while doing nothing to ensure any reduction in total impacts! This is at considerable odds with the RMP’s characterization of the minimum fisheries regime as “extraordinary fisheries conservation measures” designed to “minimize” impacts to Management Units from fisheries.

That characterization is also contradicted by NOAA’s Draft Environmental Impact Statement on the RMP in its estimation of changes in fishery effort that would be expected to occur under various scenarios modeled by the co-managers for the next six years. Under the most optimistic scenario, levels of abundance and Alaskan and Canadian fishery regimes would be similar to

2003 (Scenario A). The co-managers estimate total commercial harvests in Puget Sound (all salmon species) of 20.0 million pounds in Scenario A. The worst-case scenario (Scenario D) assumes that the total abundance of adult chinook bound to return to Puget Sound will be 30% lower than in 2003, and Alaskan and Canadian fishery impacts on Puget Sound chinook will be at their legal maximums, conditions that would trigger the RMP's "extraordinary" conservation measures. Under Scenario D, "harvests are predicted to be 97 percent of Scenario A." In terms of recreational fishing activity, 1.4 million angler trips are predicted under Scenario A, and "Scenario D trips are predicted to be 95 percent of Scenario A trips." (Puget Sound Chinook Harvest Resource Management Plan NEPA Draft EIS, Section 4.6.1.1, page 4 – 130.)

NOAA admits that the considered employment of critical exploitation rate ceilings is "driven by policy considerations" and not by biological (i.e., conservation) considerations (ibid). Nowhere, however, are the "policy considerations" referred to or their legal basis explicitly described, explained, and/or justified. This seems particularly inappropriate in view of the fact, admitted by both the RMP and the PEPD that such policy consideration only come into full force when the escapement of a listed population or management unit is anticipated to be at a near-critical level of abundance (below the RMP's proposed low abundance thresholds). It is incumbent on NOAA to explicitly articulate the policy considerations and their legal status and demonstrate that such considerations justify the risk to which listed chinook populations would be subjected under the circumstances in which critical exploitation rates would be put into effect under the RMP.

Discussion of Management Objectives and Indicators

The PEPD holds that the RMP meets the conservation burden in such a way as to "conserve the productivity, abundance, and diversity of all populations within the Puget Sound Chinook Salmon ESU", among other objective (p. 5). The RMP is said to achieve this by employing "biologically-based management objectives that are generally expressed in terms of population-specific exploitation rates or escapement goals", except when circumstances require that the minimum fishing regime applies (p.5). This leads one to expect that the RMP proposes to manage harvest on the basis of the status of individual populations.

However, the substance of the proposed regime suggests that these assertions overstate the extent to which the RMP is supportive of recovery. The majority of listed Puget Sound chinook populations occur in the Sound from the Snohomish Basin north. Twelve of the 22 populations from the Elwah River east home to one of four river basins: Snohomish, Stillaguamish, Skagit, and Nooksack. The proposed (incidental) harvest impacts on all of these populations is to be achieved under the RMP by managing exploitation rates at the management unit level, not at the level of the individual population. Within these four river basins there are five management units: Nooksack, Skagit Summer/Fall chinook, Skagit spring chinook, Stillaguamish, and Snohomish. In none of these management units is the maximum ("recovery") exploitation rate based directly upon (and therefore constrained by) an estimate of the maximum allowable rate sustainable by the weakest component stock.

This reliance on management unit rates contradicts the claim by the RMP and the PEPD that the RMP proposes a harvest management regime in which exploitation rates are restricted by the weakest component population. Absent such a clear restriction, the proposed harvest regime cannot be accurately characterized as population-based management.

The basic harvest regime for a population or management unit is characterized by a combination of exploitation rate ceilings and abundance thresholds. The abundance thresholds function primarily as controls on which exploitation rate ceiling is to be in effect in a given fishing season. There are two exploitation rate ceilings that are related to two abundance levels: "recovery exploitation rates" (RERs), "critical exploitation rates", "upper management thresholds", and "low abundance thresholds". The critical rate applies under the minimal fishery regime describe previously. It is to be operative when the pre-season estimate of the total escapement of a management unit will be below the low abundance threshold "or if Canadian fisheries make it difficult or impossible to achieve the RMP's rebuilding exploitation rate"(p.12). RERs are operative when the pre-season estimate of the total escapement of a population or management unit is expected to be above the low abundance threshold. In the case where escapements are estimated to exceed the upper threshold, directed harvests on listed populations (or management units) may take place to harvest up to the difference between the estimated escapement and the upper threshold, provided that the RER is not exceeded.

Clearly, the extent to which the harvest regime proposed in the RMP is compatible with the survival and recovery of the listed ESU and the populations that compose the ESU depends both on the appropriateness of the threshold levels that function as controls on the harvest rates and the harvest rates themselves. These rates and thresholds should satisfy at least two requirements: (1) they should be estimated in a rigorous manner that fully takes into account the uncertainties involved in the estimation process; (2) in light of the uncertainties involved in (1) the operative rates and thresholds should be chosen to be risk-averse -- that is, they should be chosen so that they are robust to the inherent estimation error. The PEPD and the RMP appear to fail on both of these counts.

Recovery Exploitation Rates

Before discussing our general concerns regarding the RMP RERs, we want to note that Table 2 of the PEPD, which summarizes the relationship between the various management objectives and exploitation rates for each Management Unit, is confusing and potentially misleading. It tends to suggest that reductions in total harvest impacts associated with the Low Abundance Thresholds will be greater than they actually are, and in some cases suggests that total exploitation impacts under the RER regime will be significantly lower that they actually will be.

For instance, The RER for the Skagit Summer/Fall MU is expressed as 50%, applicable when escapement is above the Low Abundance Threshold of 4800. The Critical Exploitation Rate (minimum fisheries regime), triggered by the Low Abundance Threshold, is expressed as 15% SUS (in even years). A lay person or casual reader may infer that total harvest impacts would be required to drop by as much as 35 percentage points if abundances decline to or drop below 4800. However, the RER as expressed in the table represents total harvest impacts from all fisheries, including Canada and Alaska, while the Critical ER as expressed represents only Southern US impacts (Washington Coast and Puget Sound). The table does not at all make clear that Canadian and Alaskan impacts are absolutely unaffected by the Critical ER Ceiling, and that total impacts under the minimum fisheries regime will be reduced only marginally if at all (see discussion on the Minimum Fisheries Regime above).

Compounding the confusing and misleading inferences of Table 2, some of the RERs are expressed as Pre-Terminal SUS and SUS rates, without clearly identifying that the rate does not include impacts from Canadian and Alaskan Fisheries, allowing the suggestion that total harvest-related impacts on those Management Units will be significantly lower than they actually will be. The RER for the Green River MU, applicable above the Low Abundance Threshold of 1800 spawners, is expressed as 15% PTSUS. Again, to a casual reviewer, 15% might appear to be a reasonably conservative impact-limit, possibly biologically appropriate for a "recovery" exploitation rate. However, according to Table 5, the most likely total exploitation impact on the Green MU over the course of the RMP will be 63%, including an SUS impact of 47%. While exploitation impacts will total more than four times the RER expressed in Table 2, and SUS rates will be over three times the RER, impacts to the Green MU will comply with the RMP because Pre-Terminal (PT) SUS rates will be held to 10%.

At worst, this might be characterized as smoke and mirrors. At best, it is less than completely transparent. RERs and Critical ERs should be expressed so that they can be simply compared, so that reductions in total impacts associated with the minimum fisheries regime can be clearly determined. RERs should be expressed so that total harvest-related impacts under the RER regime are clearly quantified for all MUs.

The PEPD includes descriptions of RERs calculated by NMFS and by the RMP (co-managers). The PEPD states that the co-managers "propose that the RMP's rebuilding exploitation rate for the individual management units would *improve the viable status* of the population or populations within that management unit." (page 7, emphasis added). No evidence is provided for such a claim. It is hard to understand how adding harvest mortality to the level of mortality currently experienced by listed populations can *improve* their health!

The PEPD asserts that harvest at or below NMFS-derived RERs -- which are calculated differently than those employed in the RMP but are in general similar (and often greater) than those in the RMP – "will not appreciably reduce the likelihood of rebuilding that population, assuming current environmental conditions based on specific risk criteria" (page 7), apparently contained in a document referred to in the PEPD as "Viable Risk Assessment Procedure (NMFS 2000a)." No details as to the actual assumptions and calculations carried out by NMFS are provided to support this claim. Consequently, it is impossible for the reviewer to know what "specific risk criteria" were employed and to judge their appropriateness.

The title of the document referenced by the PEPD appears to be in error. The citation references the draft RAP document of May 30, 2000. However, this was to have been superceded by a document titled "Viable Risk Assessment Procedure" that employed a harvest model that was believed would be more suitable to the population viability modeling tasks needed to assess harvest impacts on listed salmon populations. This model was not employed by NMFS Sustainable Fisheries Division in its current assessment of the proposed RMP, but rather the original RAP model.

In August 2001, the prestigious Salmon Recovery Science Review Panel held a series of meetings with state, tribal, and NMFS harvest-management personnel to discuss and evaluate harvest-management techniques and practices related to conservation and recovery of listed populations. The following comments on RAP and VRAP are taken from the November 2001 RSRP review of those meetings:

> There is a glaring disconnect between the deterministic yearly allocation models used to set allowable harvests, and their validation with stochastic population dynamics models… Earlier versions of RAP lacked realistic patterns of temporal autocorrelation in environmental stochasticity and simulated population trajectories only over a 25-year time frame. We are encouraged by recent efforts to improve this model (i.e., the VRAP model, which includes different forms of density dependence, depensation, more general patterns of environmental stochasticity, and a longer simulation time frame). However, we do not see any indication of close interaction between the groups setting yearly allowable harvests and those exploring the consequences of these choices on a longer time frame….
>
> We recommend that NMFS carefully reexamine the procedures by which allowable harvests are suggested and approved. The deterministic allocation models used to set allowable harvests each year need to be much more thoroughly tested and validated with long-term stochastic population modeling based on objective PVA criteria. Moreover, we see a need to incorporate the yearly cycle of harvest decisions into a long-term simulation model like VRAP. (RSRP, pages 8-9).

The apparent use of the RAP and not the VRAP, despite the misleading term in the text on page 7, to assess impacts to population viability from harvest under the RMP at the RERs, suggests that the risk assessment procedure employed is inappropriate and not the best that is currently available to NOAA

Population viability analysis is the area of specialization of personnel in the Conservation Biology Division of NOAA's Northwest Fisheries Science Center, not the Sustainable Fisheries Division. The "close interaction between the groups setting yearly allowable harvests and those exploring the consequences of these choices on a longer time frame" (viz., these two divisions at the Science Center) that the RSRP recommended still does not appear too have taken place. In fact, the current NMFS RERs have been determined without any recent input from or collaboration with the Conservation Biology Division, members of which have frequently expressed concern over the appropriateness of several of the proposed RERs, particularly those for the Skagit chinook populations. Consequently, we believe that there is good reason for skepticism about the viability of many of the RERs proposed by NOAA and by the co-managers.

In January 2001, personnel at the Conservation Biology Division submitted written comments to the Sustainable Fisheries Division, expressing "discomfort with the approach used to establish exploitation rates...." At that time, the CBD noted an apparent failure by the SFD to incorporate "additional, fairly simple ways to improve... establishment of exploitation rates" that had been explored in previous discussions between the Conservation Biology and Sustainable Fisheries Divisions. The CBD comments explicitly cite numerous "technical liabilities" and "biologically counter-intuitive results" with the approach taken by the SFD in setting exploitation rates, many of which parallel the concerns expressed in this review. It is not clear that the issues raised by the Conservation Biology Division in 2001 have been reconciled, and in fact the weaknesses and omissions in the approaches taken to set the RERs in the PEPD appear to be similar to those criticized in the CBD comments.

Apart from these issues, the RERs appear to be closely associated with the upper management escapement thresholds. Below we discuss reasons for believing that estimates of MSH escapement levels are inappropriate conservation targets. Here, we note that we believe that most of the RERs are too high and are inappropriately risky. It is not at all clear that harvests at or near such rates will not appreciably impair the progress of recovery of populations within the listed ESU.

While we do not believe that MSH escapement levels are appropriately risk-averse even when they are accurately estimated and managed for, there is a practical concern for establishing RERs with respect to point estimates of MSH escapement. MSH escapement is located on the ascending limb of the estimated stock-recruit curve at the point at which the slope of the curve is equal to 1. In the neighborhood of this point on typical logistic growth curves the slope of the curve is positive and greater than 1 to the left. Consequently, any error in achieving the true MSH escapement level that results in over-harvest (under-MSH-escapement) will produce escapements that produce recruitments that are markedly below those produced at MSH or higher levels of escapement. In the case of a stock being managed for recovery (rebuilding) such a harvest policy is "knife-edged" in the sense that even slight imprecision in achieving a harvest target (here, an RER) can result in significant under-escapement and significantly reduced subsequent recruitment.

The fact that the RERs are alleged to function as ceilings (though apparently only rough ceilings) does not alter the riskiness of choosing them in close association with estimates of MSH escapement. Lower exploitation rate ceilings associated with regions of estimated stock-recruit curves considerably above MSH escapements and closer to equilibrium abundance (carrying capacity) are needed. Such escapement levels would also support the employment of more robust and sustainable harvest methods associated with threshold harvesting regimes as described by Lande et al. (1997) and previously recommended by the RSRP (RSRP 2001). Critical discussion of such issues is neglected by NOAA in the PEPD.

Upper Management Threshold

According to the PEPD, "[t]he co-managers define the upper management threshold as the 'escapement level associated with optimum productivity (i.e., maximum sustainable harvest…),'" levels "calculated" by the co-managers on the basis of "current habitat conditions." (Page 11). The RMP Upper Management Thresholds "equate to the upper escapement thresholds."(Ibid.). There are several reasons to be critical of these levels, all of which appear to have been neglected or ignored by the PEPD.

There is in fact little real data available to the co-managers or NOAA on which to base firm, robust estimates of either the current carrying capacity (equilibrium abundance) or the "maximum sustainable harvest" escapement level of individual chinook populations in Puget Sound and Hood Canal. Under such circumstances and in view of the current conditions that justify the listing of the ESU, it is extremely risky to attempt to identify a unique harvest management threshold point such as escapement that will achieve maximum sustainable harvest.

Even with the best of data it is extremely risky to attempt to manage harvest regimes to achieve point estimate escapement levels corresponding to MSH unless the harvest management regime is capable of attaining essentially perfect degrees of precision – a virtual impossibility under conditions as complex as harvest regime proposed under the RMP.

As the PEPD notes in discussing “rebuilding exploitation rates” (RERs) on page 10, calculating such rates “ideally requires knowledge of a spawner-recruit relationship based on escapement, age composition, coded-wire tag distribution, environmental parameters, and management error …”. These considerations also apply to attempts to estimate other harvest management targets such as MSH escapement levels. Moreover, to be truly useful in determining a point estimate value for a management target such as escapement that will produce “maximum sustainable harvest”, the data that is employed to estimate a stock-recruit relationship needs to cover a period that spans several generations of chinook (at least 20 years) under stable environmental conditions, wherein the environmental variability exhibits a stationary distribution..

During the past 20 years Puget Sound chinook populations have experienced a variety of destructive impacts to their freshwater environments, large fluctuations in ocean productivity and in patterns of growth and survival associated with ocean productivity, and changes in fishery regimes in both Canada and the southern US, including Puget Sound. These circumstances render any apparent stock-recruit relationship based upon data spanning this period suspect, because the time series of escapements and recruitment is not stationary. Even if there were no errors in estimating escapement and recruitment -- including the confounding effect of unmarked F1 hatchery fish on the spawning grounds throughout Puget Sound and Hood Canal – any estimated stock-recruitment relationship would have a high probability of failing to accurately reflect the true environmental variability affecting the recruitment process for any individual brood year.

The real stock-recruitment relationship of interest is the current one that likely has been in effect only since the late 1990s and for which, therefore, there is at best one or two spawner-recruit data points! No *bone fide* stock-recruit relationship can be determined from this scant data.

This nullifies both the attempt to estimate equilibrium stock size (carrying capacity) from an estimated stock-recruitment relationship and to estimate a hypothetical “optimum” escapement representing maximum sustained harvest from the available data alone. In order to incorporate past data that reflect different environmental regimes and different underlying stock-recruit relations, it would be necessary to attempt correcting for the lack of stationarity in the available stock-recruit time series, and incorporate comparable data from other chinook stocks. Even so, the resulting stock-recruit relationships will contain additional uncertainty associated with the correction, requiring managers to adjust estimated management threshold in a conservative direction, if associated harvest impacts are not to impede the recovery of the listed populations. This approach to estimating stock-recruit relationships does not appear to have been employed or considered by NOAA or the co-managers.

The simple point is that any estimate of a critical management threshold such as the MSH escapement level will inevitably be extremely uncertain. It is extremely risky to employ such an uncertain point estimate as a management target, without at least acknowledging the uncertainty, which in practical terms should mean adjusting the target in a conservative direction relative to the risks associated with the uncertainty. The RMP gives no evidence that such critical uncertainties in the employment of estimated stock-recruit relationships have been fully recognized and adjusted for. In any case, the PEPD fails to raise or discuss any critical considerations of these kinds about the approach taken by the RMP to estimating these escapement reference points and employing them in the proposed harvest management regime.

Another issue regarding the estimation of stock-recruitment relationships that is ignored by the RMP and the PEPD is the impact of past (over-) harvest on aggregate stocks (management units). Hilborn (1985) explained how high harvest rates on mixed stocks composed of populations of varying productivities will alter stock-recruit relationships in misleading ways that can result in harvest rates that will prevent rebuilding to the size of the original aggregate stock (see also, Hilborn & Walters 1992, Chapter 7, pp. 293 ff.). Harvesting the original aggregate stock at high rates will result in the reduction or elimination of component "weak" stocks that are less productive (i.e., that have lower inherent population growth rates at low abundances) than the average of the original aggregate. Such stocks may nonetheless be critically important to the long-term viability and diversity of the larger population unit.

With no spawner and recruit data for the original unfished stock, the stock-recruit data that is acquired following the initiation of fishing will fail to be representative of the original aggregate. Instead the new aggregate stock will appear to be both inherently more productive at low stock sizes than the original aggregate and to have smaller capacity because it is now composed of fewer sub-stocks. In order to rebuild or recover the original stock-complex, harvest rates must, at the very least, not exceed the rate that the weakest component population can sustain, but the available estimated stock-recruit relation will fail to indicate the need for this low rate.

This circumstance is especially appropriate to the case of Puget Sound chinook, the majority of whose populations have been harvested at high and likely unsustainable rates, throughout most of the 20th century. Even in the absence of the kinds of circumstances described above that violate the basic assumption of a stationary time series of spawners and recruits, high harvest rates in the past will have resulted in stock-recruit data that will produce the kinds of biased parameter-estimates described by Hilborn. These potential biases in stock profiles based upon the estimation of stock-recruitment relationships – appearance of higher productivity at low abundances and smaller carrying capacity – are critical to avoid in managing harvest impacts on ESA-listed populations, especially when recovery targets have yet to be determined!

The PEPD asserts that the RMP establishes upper management thresholds for populations or management units using methods such as "standard spawner-recruit calculations…, empirical observations of relative escapement levels and catches, or Monte Carlo simulations that buffer for error and variability…" (page 11). For the reasons given above, the first two methods are unlikely to be risk-averse or to provide an unbiased and robust estimation stock productivity or capacity. The third method (Monte Carlo simulations) at best only brackets the point estimates of the stock recruit parameters and the associated point estimates of management thresholds such as "maximum sustainable harvest". Moreover, the results of these simulations that are reported are not appropriately risk-averse.

The PEPD reports that "[t]he co-managers' expectations are that application of the RMP's rebuilding exploitation rates will: (1) result in escapement levels that are less than the point of instability no more than five percent more often than if no harvest had occurred over 25 to 40 years; *and* (2) lead to a high (at least 80 percent) probability that spawning escapements will increase in 25 to 40 years to a specified (upper) threshold *or* that the percentage of escapements less than the RMP's low abundance threshold at the end of 25 or 40 years will differ from a no-harvest regime by less than 10 percent…"(page 10).

There is a considerable amount of hedging here in regard to characterizing the risk to recovery that may be posed by implementation of the RMP. In the context of the recovery of populations that are at-risk of serious decline or extinction, an 80 percent probability of increase to some "upper threshold" is not at all risk-averse! If the "upper threshold" is biologically significant for conservation, NOAA should not accept 20% probability of *not* attaining it within four to eight chinook generations! The determination of appropriate probability levels and timeframes for attaining or failing to attain some critical upper or lower threshold should result from a clear and rigorous Population Viability Analysis. This has not been undertaken for the RMP and would, in any case, require the involvement or at least the review of NOAA Conservation Biology Division, which has not been actively engaged in the current review process that has resulted in the PEPD, a fact noted by the RSRP on previous occasions (op. cit.).

Nor is a difference of "less than" 10 percent in the percentage of escapements that are below the "low abundance threshold" between the proposed harvest regime and no harvest insignificant. It is, for example, not at all obvious that NOAA should approve gambling on increasing the frequency with which populations of listed chinook are expected to cross the low abundance threshold from above by nearly 10%. The PEPD should at least provide a critical discussion of the significance of these estimated differences.

The assertion that escapement levels below the "point of instability" are expected to occur no more than 5 percent more often than under no harvest was made in the 2001 RMP. The November 2001 RSRP report commented on such claims as follows: "An inability to detect a difference between harvest and no harvest regimes should not suffice as a justification for harvesting [declining] stocks"(RSRP Report page 13). This point fully applies to the repetition of this same statement in the proposed RMP. The PEPD is simply uncritical of these kinds of assertions.

The full distribution of modeled outcomes should be presented, and modeled harvest-management scenarios compared to one another. It is also necessary to compare the shapes of the distributions and the patterns of overlap of modeled outcomes with respect to critical thresholds of interest in order to fully understand the uncertainties and comparative risks of competing potential management strategies. Then managers can employ Statistical Decision Theory techniques to choose a final management regime. Statistical Decision Theory techniques allow an appropriately transparent process for identifying the most biologically justifiable alternative.

The RMP and the PEPD rely upon questionable and controversial estimates of current habitat capacity to justify estimates of upper management thresholds (MSH escapement levels) and the proposed harvest regime that will largely keep escapements below those estimated threshold levels under all but the most fortuitous of circumstances. On page 36 NOAA relies on Ecosystem Diagnosis and Treatment (EDT) modeling estimates of spawner-recruit functions to argue that "further harvest constraint will not, by itself, effect an increase above the asymptote associated with current productivity, until habitat conditions improve."

EDT has recently received very critical reviews from the Salmon Recovery Science Review Panel and from the Columbia Basin Independent Science Advisory Panel -- which is also in independent scientific advisory panel to NOAA Fisheries. EDT is most appropriate for estimating the potential impact on salmon populations of specific in-basin habitat actions and for prioritizing the undertaking of actions thus identified. It is neither designed, intended, nor suited to estimating stock-recruit relationships, though it is apparently being misused for this purpose. EDT has not produced any peer-reviewed publications in which such stock-recruit analysis of specific salmon populations and corresponding estimates of escapement levels have been reported and evaluated. Nor has EDT as a modeling tool itself been validated through a peer review process. EDT is viewed very critically by the Environmental Conservation Division of NOAA Fisheries' Northwest Science Center.

The structure of EDT's modeling process is underlain by a Beverton-Holt stock-recruit relationship which in practice has a tendency to produce low estimates of total habitat capacity and correspondingly high estimates of density-dependent productivity at very low population sizes. This tendency to underestimate habitat capacity is especially likely to be manifested in the kinds of data-poor situations in which EDT is resorted to for prioritizing habitat actions. This tendency is likely to be exaggerated by the failure of EDT to incorporate and take into account any environmental or demographic stochasticity. EDT and related habitat modeling efforts will only produce robust estimates of habitat capacity if they are first evaluated by being applied to real data for salmon life stages within freshwater under appropriate experimental designs that include control and treatment sites.

Instead of being more rigorously evaluated as a potential tool for habitat-based recovery actions, EDT is being relied on as a surrogate for the collection and utilization of real data, particularly smolt outmigrant data. The best way under current circumstances to improve estimates of salmon productivity in freshwater and of freshwater habitat capacity is to collect smolt outmigration data wherever feasible. This, too, has been noted by the RSRP (Report for the meeting held August 27 – 29, 2001; Section VI. A. page 12). Combined with spawner escapement data and environmental data such as the timing, frequency, and duration of high and low discharge events, smolt data provides a considerably more accurate picture of actual freshwater productivity and capacity than model-based estimates that are not grounded in such real data.

Low Abundance Thresholds

The RMP defines a low abundance threshold as a "spawning escapement level, set intentionally above the point of biological instability, which triggers extraordinary fisheries conservation measures to minimize fishery related impacts and increase spawning escapement (page 63 of the RMP)" (PEPD page 11). This statement is misleading and, in fact, in error when it states that "extraordinary" fishery measures are triggered and that the purpose of such measures is overtly to "increase spawning escapement." As noted above, the pre-season expectation that a Low Abundance Threshold will be crossed from above would trigger *at best* a rather modest reduction of the allowable southern US total or pre-terminal exploitation rate, in many cases would merely “constrain” impacts from SUS fisheries comfortably *above* the levels likely to already be occurring under the default RER fishery regime, and in most cases would not significantly reduce total impact rates on individual MUs.

In fact, SUS exploitation *rates* will generally *increase* when the minimum fishery regime is triggered under circumstances when total abundances are low enough that escapements are projected to be below a population or management unit’s low abundance threshold relative to the circumstance when the regime is triggered due to the total RER being exceeded even though escapements are expected to be above the low abundance threshold. The Skagit Summer/Fall MU has an RER of 50% which is expected to be exceeded (anticipated level 55%) under the period of implementation due to Canadian fishery impacts, thus triggering the minimum fishery regime with abundances expected to be great enough that escapements are expected to be above the low abundance thresholds for the component populations. This is anticipated to result in a SUS total exploitation rate of 16%. When the minimum fishery regime is triggered by the expectation that escapement will be below the low abundance thresholds, the SUS rate can be as high as 18%.

Even though the expected total SUS landed catch from the MU would be lower under the latter circumstance, this is irrelevant to the impact on the MU and its component stocks that will result from the harvest regime proposed in the RMP. In this case the minimum fishery regime would impose a higher exploitation rate on a stock with an expected escapement below the low abundance threshold, driving the expected escapement even lower. Assuming *ex hypothesis* that the low abundance threshold is, in the words of the RMP, “above the point of biological instability” the minimum fishery regime will drive the escapement closer to the point of instability, and perhaps even below that point. This hardly qualifies as the taking of an extraordinary measure to “minimize fishery related impacts and increase spawning escapement.”

Moreover, the fishing level proposed under the minimum fisheries regime – which forms the foundation around which the entire fishing regime proposed in the RMP is designed – will be the minimum fishery level in every year of the RMP regardless of the projected escapement. Under the RMP this regime will, in fact, become the baseline regime. It is simply misleading to characterize a baseline fishing level as a level at which “extraordinary conservation measures” are in effect.

The biological relevance of the low abundance thresholds is also of concern. The low abundance thresholds are themselves defined with reference to “the point of biological instability.” The implication appears to be that the Low Abundance Thresholds are actually an intermediate threshold set conservatively above some identified lower, critical point of abundance. Yet neither the RMP nor the PEPD clearly define the “point of biological instability” or provide a clear quantitative explanation of how the proposed Low Abundance Threshold levels are determined. Nor do they provide any evidence that the Low Abundance Thresholds are set far enough above putative points of biological instability to provide a pre-cautionary and properly risk-averse margin of safety when they are crossed from above.

If the Low Abundance Thresholds are intermediate above the point of instability, and the Critical ER Ceilings are designed to slow or avoid the potential for the population to reach that point, the distance between the two thresholds would appear to be relevant to evaluating the appropriateness and likely efficacy of the minimum fisheries regime. It also seems counterintuitive that if the Low Abundance Thresholds were intermediate, no management action would be planned or required if the population did indeed reach the point of instability.

Page 65 of the RMP itself defines the point of instability as “that level of abundance (i.e., spawning escapement) that incurs substantial risk to genetic integrity, or exposes the population to depensatory mortality factors.” As with other critical terms employed in the RMP and the PEPD, no explanation is provided or even attempted regarding what is meant by a “substantial” risk or – more to the point – how such a level of risk is determined. We believe that it is simply incumbent on the co-managers in the RMP and NOAA in the PEPD to explicitly describe how such levels of risk are determined and the conditions by which the level of risk is measured.

Inspection of the low abundance thresholds themselves raises concern that they are not chosen in an adequately risk-averse manner with respect either to the recovery of populations within the ESU or with respect to the provision of an adequate margin of safety -- in the context of chinook harvest management -- above a lower level of abundance of concern. The characterization in the PEPD of how the co-managers define low abundance thresholds confirms the impression that the levels are chosen subjectively and primarily for the convenience of characterizing a (minimum) fishery regime that will secure a pre-judged desirable minimum level of harvest regardless of stock condition.

For example, one criteria employed to identify a low abundance threshold was “the lowest escapement level with a greater than one return per spawner ratio”. This is ambiguous. Does it mean that the level chosen is the lowest single point in the record at which a return of greater than one was observed -- but other points in the record that were equal to or even somewhat greater than that level had returns of less than one? Or, does it mean that such a point was chosen provided that no other point of comparable or greater escapement levels ever had a recruitment less than one? In either case, given the variability in salmon recruitment that is typically characteristic of even a healthy wild salmon stock, it is difficult to view this method as risk-averse, given that it implies that there are stock- or management unit-specific escapements of record greater than the level chosen that also exhibited recruitment levels not much greater than one. It is also relevant at what point in a period of record recruitments of greater than one were observed at candidate Low-Abundance-Threshold target levels.

In any case, we believe it is incumbent on the PEPD to critically consider these kinds of issues regarding the choice of Low Abundance Threshold escapement levels for harvest-management controls, and to require the RMP to adequately define such critical terms as “the point of biological instability” and “substantial risk to genetic integrity,” and provide appropriate measurable criteria for determining when critical levels are being approached or crossed.

Critical Exploitation Rate Ceiling

Most of our concerns regarding the Critical ER Ceilings have been elaborated in previous sections, most notably in the discussions on the Minimum Fisheries Regime and the Low

Abundance Thresholds. We will here restate and clarify some of those concerns, and comment on some specific statements in the PEPD's discussion of the Critical ER Ceilings.

We are distressed that the determination of an exploitation-rate ceiling intended as response to crossing a critical-abundance threshold from above would be based on policy objectives rather than biological considerations. It does not seem at all intuitive that a management decision of such potential import and associated risk could qualify for take authorization without proposing some coherent biological justification. At the very least, identification and a thorough discussion of the pertinent policy objectives and their relationships to ESA-compliance would be necessary. However, the PEPD does not attempt to analyze or even discuss the unnamed "policy considerations," except to note that they have "primarily driven" the development of the ceilings.

The relative appropriateness of the Ceilings notwithstanding, our largest concern is the disconnect between the descriptions of the Critical ER Ceilings and their apparent actual effects on impact rates. Characterizations of the Ceilings and their association with the Low Abundance Thresholds are confusing and often misleading.

For instance, The PEPD appears to endorse the co-managers proposition that the Critical ER Ceilings will result in "significant" reductions in harvest impacts on PS chinook, while providing something called "minimally acceptable access" to the fisheries that are imposing nearly all the impacts. No discussion is offered on how a minimally acceptable level of access was determined, who determined it, or why. As discussed above, it is not at all apparent that the Critical ER Ceilings are capable of effecting anything more than marginal reductions in overall impacts to most MUs under most circumstances. In fact, the ceilings would not appear to constrain SUS impacts for many MUs below levels likely to occur under default fishing conditions when escapements are above the Low Abundance Thresholds. The default fishing impacts appear in fact to represent the minimum access the co-managers will accept, and this seems to be the likely policy objective driving the development of the Critical ER Ceilings, propositions of significantly reduced impacts notwithstanding.

The association of the Critical ER Ceilings with RERs and the Low Abundance Thresholds creates the implication of a two-tiered harvest regime for each MU, with separate impact-rate schedules above and below the thresholds. However, there is little suggestion that the provisions of the RMP would necessarily effect any significant difference in overall impacts on an MU, no matter what level of abundance it reaches, or whether or not Critical ER Ceilings are imposed.

Regarding the Critical ER Ceilings (and possibly, if only by inference, "minimally acceptable access") the PEPD appears to paraphrase and make a remarkably uncritical endorsement of the co-managers "position" that any necessary "further resource protection" must come from, among other things, reducing impacts in Alaskan and Canadian fisheries. To bring this point home, the PEPD goes on to note terms of the Pacific Salmon Treaty that allow potential increases in Canadian fishing activity likely to impact PS chinook, the range of impacts to Puget Sound MUs imposed by Canadian fisheries, and the fact that "the management of Canadian fisheries is outside the jurisdiction of the co-managers." The PEPD appears to be offering extra-biological justifications for the Critical ER Ceilings, and making a coarse attempt at distributing conservation-burden equitably. However, as noted in more detail in the discussion on the minimum fisheries regime, the PEPD fails to note that NOAA does have some influence and jurisdiction over terms of the PST and by extension impacts from Canadian fisheries. NOAA has the responsibility to acknowledge its previous determinations regarding the allocation of allowable impacts to a listed ESU; it certainly can not use the relative "equity" of those previous determinations as the primary justification for accepting increased risk to the same listed population in a subsequent determination.

Other Issues of Concern

The Range of Variability in Chinook Productivity is not Fully Considered

The PEPD uncritically accepts the likely range of abundances of adult chinook returns during the six-year RMP implementation period chosen by the co-managers for their modeling of the impacts of implementing the RMP. The 2003 modeled abundance was used to estimate the high end of the range. The lower range of modeled returns chosen was a "30 percent reduction in the 2003 abundance". This is entirely too optimistic in view of the range of variability in both freshwater (egg-to-smolt) and ocean (smolt-to-adult-return) survival rates experienced by Puget Sound chinook stocks over the past decade and a half. Both freshwater and saltwater survival rates can range well beyond 30%.

On the Skagit River, for example, egg-to-smolt survival rates of wild 0+ chinook measured by WDFW ranged from 1.2% to 16.5% during the 1990s (Seiler, Neuhauser, and Kishimoto 2001). These rates are strongly correlated with river discharge during the egg incubation period and largely independent of spawner density and estimated egg deposition. The lowest survival in the record is 1.2% associated with record-for-the-period discharge of 142000 cfs on November 25, 1990. Until October 2003, there have been no comparable events since 1995. Between 1996 and 1999 spawner densities ranged three-fold (5395 to 15695) and estimated egg-to-smolt survival rates ranged narrowly between 12.7% and 16.5%. In October 2003, after the peak period of chinook spawning in the Skagit and Sauk Rivers, a severe high flow event occurred of comparable magnitude to the November 1990 flood. The overwhelming majority of the 2003 spawning has likely been lost and basin-wide average egg-to-smolt survival for this cohort can be expected to be in the neighborhood of 1% or lower. This alone will result in a return in 2007 and 2008 much lower than 30% below the 2003 return.

The modeling assumptions of the RMP are not risk-averse to this kind of environmental variability. The PEPD is remiss in failing to recognize this and in failing to require that the co-managers adopt more risk-averse modeling assumptions in estimating the likely impacts on listed chinook of the implementation of the RMP.

Negative Impacts of Hatchery Chinook on Natural-Origin Chinook are Ignored, Misinterpreted, or Inappropriately Accepted

In its discussion of likely impacts of the RMP on specific populations and management units in the ESU, the PEPD ignores or misinterprets the potential negative impact of hatchery-origin spawners on natural-origin chinook. We instance the discussion of the North Fork Nooksack River Population on pp. 28ff.

On page 28 the PEPD states that under Limit 6 of the 4(d)Rule the criterion for allowable fishing mortality impacts on populations at or below their critical thresholds requires that such impacts “must not appreciably increase genetic and demographic risks facing the population, and does not preclude achievement of viable function…”.[1] The PEPD then notes that the North Fork Nooksack population is below both its RMP low abundance threshold (1000) and NMFS critical abundance threshold (200). The latter is a dangerously low level of abundance, especially when the legacy of recent and continuing hatchery spawning in the wild clouds the estimation of the abundance of natural-origin spawners as is clearly the case in the North Fork Nooksack. The 1999 – 2003 four-year average natural-origin spawning escapement is 180. This population has been severely depressed since at least 1989 (PEPD Table 6, page 19).

Despite the clear evidence of persistent low abundance of natural-origin spawners, the PEPD attempts to justify continued harvest impacts on this population under the RMP by counting the F1 hatchery-origin spawners from the Kendall Creek Hatchery. This hatchery stock was indeed listed by NOAA Fisheries as essential to the recovery of the North Fork population. But that does not automatically excuse NOAA from critically evaluating the impact of the high percentage of F1 hatchery spawners on the natural-origin stock that is the object of recovery. No geneticist in NMFS’ Conservation Biology Division to our knowledge has asserted that Kendall Creek Hatchery chinook are functionally identical to the original North Fork Nooksack natural-origin stock from which the hatchery stock was founded, nor do they believe that the present hatchery stock can simply replace the remaining wild population.

However, by counting the F1 hatchery fish spawning in the wild the 1999 – 2003 four-year average escapement increases from 180 to 3438. This reflects an extreme percentage of hatchery-origin fish spawning in the wild, in the neighborhood of 95%! No pure supplementation project whose sole purpose is the rebuilding of a critically depressed naturally-spawning salmon population recommends this kind of swamping of the natural spawning population with F1 hatchery-origin fish.

The Hatchery Science Review Group, for instance, recommends that an average of 10 to 20% natural-origin adults be incorporated into the hatchery broodstock each year and that hatchery-origin spawners constitute a smaller percentage of natural spawners annually than the percentage of natural-origin adults incorporated annually as broodstock. (HSRG 2004, Appendix B, pp. B7-8). And the HSRG’s are the most liberal of guidelines suggested by any of the recent critical reviews of hatchery-wild salmon interactions (see, for example, the ISAB 2003).

The PEPD states that the co-managers “are applying operational techniques that decrease the likelihood for divergence of the hatchery population from the extant natural population.” (page 29). The Hatchery Genetic Management Plan for the Kendall Creek hatchery recently submitted to NMFS by WDFW fails to specify any such requirements for incorporating natural-origin

[1] We note that the latter standard is extraordinarily vague inasmuch as one can imagine that any population might under circumstances of rare good luck rebound from very low abundance levels given enough time; hence, it is not clear what depression of a population “below their critical thresholds” would be precluded by this condition. This highlights a general failing of the PEPD and the RMP: the failure provide clear, measurable threshold levels of impact to harvest actions and to attach a specific time frame to requirements to attain some threshold standard. Language such as the above “not preclude achievement of viable function…” is meaningless without a clear specification of what measurably constitutes “attainment of viable function” and without the specification of the length of time by which such function must be attained.

spawners into the broodstock collected for the hatchery.[2] Neither information provided by the HGMP nor any data from appropriate studies indicates that the Kendall Creek hatchery stock is not likely to experience some degree of domestication selection that – among other factors -- will cause it to diverge from its founder wild stock and to have reduced fitness when spawning in the wild.

The high hatchery spawning overlap rate can be expected to cause a significant erosion of any remaining fitness differential favoring the natural-origin fish that are the object of ESA protection and recovery. Yet, the PEPD asserts that the Kendall Creek hatchery stock "retains the genetic characteristics of the wild population" (page 28). No evidence is provided for such an un-plausible assertion! To the contrary, what is very likely to occur is that the "wild" population will soon no longer exist after having been subjected to such high introgression from the hatchery stock, so that the assertion that the hatchery stock retains the genetic characteristics of the wild population will be but an awkward way of stating that the natural-spawning population has become genetically un-differentiable from the hatchery stock.

At the very least, the PEPD should seek and subsequently reference an appropriate and current genetics assessment from the Conservation Biology Division. Once again, the PEPD evidences a lack of regular interaction between the Sustainable Fisheries and the Conservation Biology Divisions at the NMFS Science Center – a clear waste of valuable intellectual resources, resources intended to help ensure the most effective and ultimately most equitable management possible of public natural resources..

In the later discussion of the Nooksack population under compliance with (6)Section (b)(4)(i)(D) of Limit 6, page 66, the PEPD alleges that the high levels of hatchery-origin spawners "are expected to buffer harvest-induced genetic and demographic risks to the natural-origin North Fork Nooksack River population". Yet it is these and other hatchery fish that are the objects of chinook harvest.

As throughout the PEPD recent increases in escapements (marginal at best in the case of the natural-origin Nooksack population) are attributed in whole or in part to the shift to exploitation-rate harvest management in the late 1990s, ignoring or downplaying the role of reductions in the targeting of chinook in Canadian Fisheries and significant improvement in ocean survival rates of Puget Sound chinook during this same period. In the case of the North Fork Nooksack population, however, NMFS notes that the abundance of natural-origin returns has failed to mirror this increase while the return of hatchery-origin fish has. From a conservation perspective, one might draw the extremely plausible conclusion that the extraordinarily high rate of hatchery-origin spawning in the wild has so introgressed the remaining wild population as to have depressed the fitness of the natural-origin population. Instead, the PEPD asserts that

> natural-origin recruitment will not increase much beyond [current levels] unless constraints limiting marine, freshwater, and estuary survival are alleviated. Augmentation

[2] We also note that this and other state as well as tribal HGMPs for Puget Sound chinook hatcheries have yet to be approved by NMFS and that, therefore, none of these hatchery programs is operating with 4(d) take coverage. This pertinent fact is never mentioned in the PEPD, a clear failure on NMFS part in its evaluation of the RMP and its depiction of the regulatory landscape associated with the harvest of Puget Sound chinook.

> of these natural-origin spawners on the natural spawning areas of the North Fork Nooksack River, with the addition of hatchery-origin spawners will continue to test the natural production potential of the system at higher escapement levels. (page 66).

It is moot twice-over whether such alleged “probing” of natural habitat capacity is relevant to recovery: first, any such increase is likely to be incompletely demonstrated if the majority of the spawners and their progeny that are doing the probing are less fit than their wild counterparts; and, second, by the time that habitat capacity has increased, there may no longer be a viable naturally-spawning chinook population remaining to utilize it.

The PEPD chooses to make the only interpretation of the escapement data that suits both continued high levels of production from the Kendall Creek hatchery that support such an unsustainable level of hatchery-origin (F1) spawners on the natural spawning grounds and that supports the current level of SUS harvest.

Moreover, and most importantly, all of the issues regarding production at the Kendall Creek Hatchery and the impacts from that production are the subjects of the currently pending NOAA evaluation of the Kendall Creek Hatchery and Genetic Management Plan and Puget Sound Chinook Hatchery RMP. The Kendall Creek Hatchery is currently operating without ESA take authorization. The PEPD’s assertions that the Kendall Creek hatchery population “retains the genetic characteristics of the wild population,” or that hatchery production at Kendall Creek “buffers genetic and demographic risks” to wild NF Noocksack chinook are precisely the assertions that NOAA has yet to make any determination over. The PEPD cites the co-managers’ application of “operational techniques that decrease the likelihood for divergence of the hatchery population from the extant natural population.” The NOAA evaluation of those very operational techniques is ongoing and a determination on their likely efficacy is still pending. NOAA cannot credibly make determinations on how a hatchery population can be used to mediate risks to a wild population from harvest impacts before it has completed its evaluation of the associated risks and authorized the operational techniques of the relevant hatchery program. Evaluation and approval of the relevant HGMP is the responsibility of the Protected Species Division, not the Sustainable Fisheries Division. In effect, the PEPD is making judgments that appear to be outside the scope of its responsibility or authority.

Lack of Clarity in Describing how the RMP Recognizes “Viable” and “Critical” Concepts
In the discussion on Criterion 4 for take-limit 4 of the 4(d) Rule, the PEPD notes that fishery management actions must “(1) recognize significant differences in risk associated with viable and critical population threshold states, and (2) respond accordingly to minimize long-term risks to population persistence.” It is difficult to reconcile these criteria with our comments re the apparent lack of any significant reduction in overall impact rates likely to be effected by the imposition of the Critical ER Ceilings under most circumstances.

The PEPD further elaborates that harvest actions should be designed so that: populations currently at or above the viable threshold (Upper Management Threshold) are maintained at or above that level; risks to populations currently between the Upper Management Threshold and the Low Abundance Threshold (critical threshold) or below the Low Abundance Threshold are not appreciably increased, and the populations can attain viable status. A straightforward and to this review a perfectly reasonable interpretation of these criteria might lead one to expect rather simply that populations currently meeting the Upper Management Threshold would have harvest impacts constrained so that they would continue to meet that level of escapement, and that harvest impacts would not be allowed to prevent populations below the upper threshold from meeting that level of escapement.

However, that does not reconcile with Table 3, outlining the likely ranges of exploitation rates and escapements for each MU over the six-year course of the RMP. The table appears to suggest that for several MUs in some years, the margin of SUS impacts create an elective opportunity to either achieve or forego escapement at or above the Upper Management Threshold and that in some years that opportunity will be foregone.

The PEPD offers considerable discussion on how and why the suggestion is apparently irrelevant, and how the RMP is by rather complicated formula meeting the above criteria, as illustrated in Table 9. The discussion and the table are frankly confusing. Clarification on these issues would be helpful.

Lack of Consistency Between PEPD and Co-Managers' 2004 Harvest Plan
Washington Trout received and reviewed information from WDFW regarding the co-managers's 2004 fishing plan, outlining model predictions of expected impacts and escapements for all PS chinook MUs. Several of the exploitation-rate and escapement predictions fall well outside the range of likely impacts and escapements described in Table 3 of the PEPD. For instance, WDFW predicts 2004 escapement for Skagit Summer/Falls at 19929, with a total ER of 38%. Table 3 in the PEPD shows no escapements for Skagit Summer/Fall above 11,633, and no total ER below 48%. WDFW predicts a 29% total ER (eight points above the RER) and escapement of 9341 for the Snohomish MU in 2004, but the PEPD lists for the Snohomish a range of anticipated total ERs from 19%-23% and no escapement higher than 5,073.

We do not know the source of these inconsistencies, but would appreciate clarification.

Conclusion
Washington Trout does not concur with NOAA's Recommended Pending Determination to grant take authorization to fisheries implemented in accordance with the RMP. We generally find that the Proposed Evaluation fails to make a compelling case for accepting the levels of risk and uncertainty associated with the RMP. The PEPD proposes to authorize a significant level of take of PS chinook. The public has the right to expect a thorough and transparent discussion and evaluation of all the factors used to develop the RMP and to evaluate its relative impact on the recovery of PS chinook. The PEPD generally fails to meet that standard.

The PEPD uncritically and in many cases inappropriately accepts and endorses several controversial assertions by the co-managers, without any documented support and in many cases without any discussion whatsoever. Most notably, the PEPD explicitly sanctions a "minimum fisheries regime," a "base level" that fisheries will not drop below, under conditions when the co-managers expect these populations to be at critically low levels of abundance. It is of significant concern that NOAA would accept that no conceivable circumstance potentially faced by the PS chinook ESU could warrant the complete restriction of fishery impacts on individual

Management Units, without sufficiently detailed and clear discussion of the justifications for such a practice. As it is we are only informed that the thresholds and exploitation rates associated with the minimum fisheries regime are primarily driven by "policy consideration."

Discussions of analyses are more complex than seems necessary, often to the point of being tortured. Tables are misleading. Important terms and concepts are defined in vague, ambiguous, qualitative terms, if at all. The lack of simple, measurable performance indicators and clear numerical descriptions of anticipated impacts to MUs under the various exploitation-rate regimes is striking. The PEPD endorses or makes unsupported and often inappropriate assumptions about habitat capacity and chinook productivity in setting and evaluating abundance thresholds and exploitation rates. It fails to appropriately acknowledge or address the weaknesses and uncertainties inherent in its modeling and other analyses.

The consistent lack of clarity and directness creates an inescapable impression of obfuscation. The PEPD tries to create a suggestion that the RMP employs adaptive management principles, but it lacks any actual adaptation in its proposed management responses. Thresholds may be crossed, and exploitation-rate ceilings imposed or lifted, but it appears that neither overall impacts nor fishing activities will ultimately be significantly affected one way or the other.

It is hard to resist the impression that the co-managers modeled expected impacts to individual MUs from "minimally acceptable access" to various desired fisheries directed at non-listed species and stocks, and developed thresholds and ERs that those impacts would fit comfortably within. In many regards, the RMP represents little more than a commitment to not "direct" harvest at listed PS chinook MUs unless they are predicted to meet or exceed MSH escapement. It is not clear that this represents any particular change from pre-listing harvest management, other than a revision downward of some MSH thresholds.

The PEPD fails to make a compelling extra-biological case for accepting potentially unacceptable levels of risk in the RMP. Reconciling an equitable distribution of conservation burden is likely irrelevant, impossible to achieve with any accuracy, or available to NOAA through other avenues that do not require imposing extra or undue risk on the PS chinook ESU.

Washington Trout respectfully recommends that NOAA Fisheries substantively revise the PEPD, requesting additional information and appropriate changes in the RMP from the co-managers before a final determination on take authorization is developed.

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