



United States Department of the Interior

FISH AND WILDLIFE SERVICE

Mid-Columbia River Fishery Resource Office

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March 21, 1995

Dear Interested Party:

During the spring of 1993 the U.S. Fish and Wildlife Service (Service) released the publication "Production and Habitat of Salmonids in Mid-Columbia River Tributary Streams" authored by Service employee (now retired) James W. Mullan and others. That report elicited considerable comment relative to some of its conclusions, methods, and general adequacy of peer review.

In response to those concerns and our reexamination of the report's content and review process, the Service agreed to initiate a post publication review and release review comments to agencies, tribes and other holders of the report. This letter and attached review comments are meant to fulfill that obligation.

The Service established a group of ten volunteer reviewers from various entities in the Pacific Northwest. Each reviewer was given the complete report and asked to examine the entire document or any portion they felt qualified to review.

Due to various reasons only five of the ten reviewers responded with their comments. Although the Service is disappointed that all reviews were not completed, we believe that any further delay is counterproductive to resolving issues generated by publication of this report. Some may disagree, but the Service considers the completed reviews as adequately capturing the essence of the report's strengths and weaknesses. The Service would like to thank the reviewers for their effort. We recognize that each reviewer took precious time from their busy schedules to assist this process.

The Service plans to print an additional 50 copies to fulfill requests received since the review began. We will attach the peer review comments to any of these reports distributed.

If you have any questions concerning this review please contact Dan Diggs (503-230-5972) or Brian Cates (509-548-7573).

Sincerely,

A handwritten signature in cursive script that reads "Brian C. Cates". The signature is written in dark ink and is positioned above the printed name.

Brian C. Cates
Project Leader



United States Department of the Interior



FISH AND WILDLIFE SERVICE

Lower Columbia River Fishery Resource Office
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Vancouver, Washington 98665

June 8, 1994

MEMORANDUM

TO: Project Leader
Mid-Columbia River Fisheries Resource Office

FROM: Fishery Management Biologist
Lower Columbia River Fisheries Resource Office
Vancouver, Washington

SUBJECT: Critique of the report, "Production and Habitat of Salmonids in Mid-Columbia Tributary Streams"

This report contains a wealth of information; the result of years of research, literature review, and work experience by the major authors. However, a critical review of several sections of the document leads to questions concerning the validity of some of the assumptions and conclusions in the report. I did not have the time to examine the habitat sections of the report in any detail and therefore will offer only a few general comments on those sections. Most of my specific comments or questions concerning this report will be limited to the areas of harvest rates, survival rates, and run size abundance estimates since these are the areas of my work experience. Specific comments and questions will be identified by page number and paragraph and follow the order that they were presented in the report.

Page iv KEY CONCLUSION: Smolts of naturally produced spring chinook salmon were 13 to 100 times as viable as hatchery smolts.

I question some of the values used in the preceding two paragraphs in making their calculations and drawing this conclusion. The report does not explain how hatchery and natural stocks were segregated to arrive at the mean 1967-87 escapements. Was straying and natural spawning of returning hatchery fish accounted for? Hatchery smolt-to-adult survival rates could be underestimated without the benefit of good marking programs in the earlier years to identify straying and natural spawning of hatchery fish. Inriver catch averaging 20% was listed in the report, but harvest rates were quite variable during 1967-87, ranging from 0.2% to 54.8%. A 10% correction for ocean harvest is listed. This is probably excessive since recent CWT and genetic stock identification data suggest rates less than 5% and closer to 1% to 3%. Smolt-to-adult survival rates for naturally produced fish

ranging up to 10.1% seem excessively high, considering high mortality rates resulting from downstream passage problems at mainstem dams that averaged about 70% and approached 95% in some years. An annual cohort run reconstruction methodology would be a better way to develop run sizes if historical age data are available. Even without historical age data, an annual cohort run reconstruction methodology that uses average age composition and annual inriver harvest rates would give a better representation of historical run sizes for the 1967-87 period.

Data, recently summarized for Warm Springs River wild spring chinook, estimate smolt-to-adult survival rates back to the Deschutes River ranging from 1.5% to 4.1% and averaging 2.8% for broodyears 1975-88. Smolt-to-adult survival rates were also developed for Warm Springs Hatchery spring chinook for broodyears 1978-88. Hatchery spring chinook survival ranged from 0.1% to 0.9% and averaged 0.4%. Wild spring chinook survival ranged from 2 to 20 times that of hatchery spring chinook and averaged 10 times that of hatchery spring chinook survival for comparable years.

Expanding the Deschutes River return rates for annual inriver harvest rates, an estimated average ocean harvest rate of 2%, and 5% passage loss of adults per dam results in a revised smolt-to-adult survival rate range of 1.9% to 5.0% and an average survival rate of 3.4% for the 1975-88 broodyear period. The Warm Springs River smolt-to-adult survival rates were calculated on an annual cohort run reconstruction basis using available age data from adult returns and estimates of wild smolt outmigration from smolt trapping in the lower Warm Springs River. The Warm Springs River spring chinook stock has five to seven less mainstem dams to pass as juveniles and one would therefore expect smolt-to-adult survival rates for this wild stock to exceed those of stocks farther upriver.

Page v KEY CONCLUSION: Smolts of naturally produced summer/fall chinook salmon from mid-Columbia River tributaries were 8 to 17 times as viable as 13.8 million tagged hatchery smolts released from lower Columbia River hatcheries.

Again, I question some of the values in the preceding two paragraphs used in the calculations in drawing this conclusion. Inriver harvest rates for summer chinook varied between 0.0% and 16.7% during the 1967-87 period. An ocean harvest rate of 75% in 1967-84 is excessive for this stock and results in a substantial overestimate of adult production of this stock from the mid-Columbia River tributaries. Further, the early estimates of ocean impacts referred to in the report are not adjusted for adult equivalence, the fact that many of the immature fish harvested in the ocean would have died of natural mortality in any event. The adult equivalent adjustment factor is especially pertinent to fall chinook impacts in the ocean since these fish are harvested over a number of years in the marine environment.

An adult equivalent ocean harvest rate of 75% may have been approached in some years for some tule fall chinook stocks in the lower river during this period, but the Columbia River Technical Advisory Committee estimated adult equivalent ocean harvest rates for upriver bright fall chinook at 45-50% during this period. Ocean harvest rates on upper Columbia River summer chinook were probably lower than those for upriver bright fall chinook because of their return timing to the Columbia River (mid-May through mid-July), which occurs before or during the early portion of major ocean fisheries during their maturing year. (Current S.E. Alaska and Canadian chinook troll fisheries for the summer season begin about July 1.)

Smolt-to-adult survival rates ranging up to 8.0% again appear excessive considering known passage survival problems caused by the hydrosystem and the overestimated ocean fishery impacts on this stock. Current smolt-to-adult survival rates for naturally spawning upriver bright fall chinook from the Hanford Reach are generally less than 1.0% based on analysis of recent coded wire tagging.

In addition, the comparison between the mid-Columbia natural stocks and lower river hatchery stocks is between differential time periods (1967-87 vs 1978-81) and stocks (brights vs tules). We know that ocean and inriver harvest impacts and survival conditions can be quite variable between years, areas, and stocks. (Tules are harvested at a higher rate in more southerly fisheries relative to brights and appear to suffer greater natural mortalities due to El Nino events than brights.)

Page v KEY CONCLUSION: Unlike salmon, hatchery smolts evidently are as viable as naturally produced smolts (mean, 6.4%; range 1.3-14.3%).

Mean survival for steelhead of 6.4% and ranging up to 14.3% again appears excessive for the Columbia River basin during the 1967-87 time period. Were hatchery and wild stocks accurately segregated and were migrating smolts accurately estimated in the analysis?

Page vi KEY CONCLUSION: ... The success of hatchery steelhead, unlike the failed hatchery programs for salmon, ...

"Failed hatchery programs for salmon," is a matter of interpretation. Hatchery production could well have been more successful than it was given credit for in the report, especially, if there was misidentification between hatchery and natural stocks which may have occurred without good marking and sampling programs during the earlier years.

Page vii KEY CONCLUSION: There is no evidence that historical abundance of salmon and steelhead in the Wenatchee, Entiat, and Methow rivers differed markedly from now.

This is a very strong statement that I do not believe is supportable given the lack of truly defensible data concerning historical catches, run sizes, etc. See later comments that refer to Page 29 and Page 30.

Page viii KEY CONCLUSION: Despite some abuse from recent activities of humans, there appears to be little or no net loss of the functional features of mid-Columbia River tributaries.

The sweeping statements concerning mining, grazing, logging, and road construction not being widespread problems in the preceding paragraphs are probably debatable but I have no personal knowledge of the habitat problems. I found the statement, "Rock riprap is used along streams for flood protection, and provides critical habitat for salmonids," to be quite interesting. I had always thought that large woody debris, undercut banks, cobble/gravel substrate, stair-stepped pool/riffles, deep pools, good riparian cover and other natural stream features were more conducive to salmonid habitat. The statements concerning diversion of up to 79% of streamflow for irrigation resulting in no appreciable difference in habitat, and current irrigation withdrawals in the Methow River basin being beneficial to salmonid habitat were very surprising to me. However, I will let others who are more knowledgeable in habitat issues deal with those claims.

Page 24, Bottom Two Paragraphs

See above comments referencing Page iv and Page v. Annual cohort run reconstruction is a much better way to calculate historical run sizes than using dam counts, a set four year brood cycle, and long term average harvest rates.

Page 28, Second Paragraph

Upriver summer/fall chinook typically have a large component of five year olds and some six year olds in the adult returns. Three year olds also make up a significant component of the returns with many of these fish returning as early maturing males. The average age composition for adult upriver bright fall chinook (hatchery and natural combined) during the period 1983-92 was 20.9% age three, 45.7% age four, 31.8% age five, and 1.5% age six fish, respectively. Average age composition for adult hatchery summer chinook in the mid-1970's based on 1974-77 brood Wells Hatchery returns was 16.7% age three, 50.0% age four, 31.3% age five, and 2.1% age six fish, respectively. Age composition can be quite variable for a given year. Calculating all production on a four year brood cycle is not appropriate when 50% or greater of the adult returns are actually of an age other than four year olds. In addition, the impacts of intercepting fisheries need to be accounted for when making the type of spawner-recruitment comparisons that are made here. I do not believe that the last sentence of the paragraph is necessarily supported by the data.

Page 28, Third Paragraph

The ocean harvest rates for summer/fall and spring chinook are overestimated and are not adjusted for adult equivalence. See ocean harvest comments relating to Page iv and Page v.

Page 28, Fourth Paragraph

See comments above for second paragraph concerning summer/fall chinook. A four year brood cycle for spring chinook also may not be appropriate. Average age composition for Leavenworth National Fish Hatchery spring chinook returns for 1980-82 was 2.3% age 3, 33.0% age 4, and 64.7% age 5 fish, respectively. Annual cohort run reconstruction is a much better modeling tool and fishery impacts need to be accounted for in this type of comparison.

Pages 29 and 30

The values in the 1967-87 columns for the Wenatchee, Entiat, and Methow rivers in Table 5 were calculated incorrectly. They are not corrected for adult equivalence of the 62%-71% total downstream mortality rates nor for adult equivalence of ocean fishery impacts of immature fish. Many of the downstream mortalities and fishery mortalities of immature stocks in the ocean would have succumbed to natural mortalities during later life stages and can not be expanded directly to adult production. The Pacific Salmon Commission Joint Chinook Technical Committee uses natural mortality estimates 50% for first year ocean residence, 40% for second year ocean residence, 30% for third year ocean residence, 20% for fourth year ocean residence, and 10% for fifth year ocean residence. These and other data weaknesses seriously call into question the conclusion that current and historical production are essentially the same.

Page 98, First Two Paragraphs

I question the reliability of some of the parameters and procedures used to estimate the egg-to-smolt and smolt-to-adult survival for naturally produced fish. See earlier comments concerning Page iv. I know of no other data that suggest survival rates as high as those presented here for naturally produced spring chinook that must negotiate the numbers of dams that these fish do. As discussed earlier in my comments to Page iv, the recent summarization of Warm Springs River wild spring chinook data probably provides the best information for an upriver stock. Those data suggest an average 1975-88 broodyear survival rate of 3.4% after adjustment for upstream passage losses and ocean and inriver fishery impacts. The reference to spring chinook released in the Deschutes River on Page 102 surviving at 1.63% is about as good as it gets for upriver hatchery spring chinook. These fish are reared in as natural conditions as possible and pass only two dams rather than seven to nine dams. It is interesting to note the difference in estimated smolt-to-adult survival rates presented in Table 20 on Page 101 between the Wenatchee River and the Methow River. There

is a 50% reduction in survival for the Methow River where fish must pass two additional dams. Use of a four year brood cycle, long term harvest rate averages, and projections of smolt outmigration from estimates of egg deposition and egg-to-smolt survival for calculating the abundance and survival rates for natural spring chinook stocks is a poor surrogate for annual cohort run reconstruction and good estimates of smolt production from outmigrant trapping.

Page 98, Fourth Paragraph

In the first place, I am not convinced that 17,400 naturally produced spring chinook is a good estimate for the mid-Columbia River tributaries for the period 1967-87. (See earlier comments.) Secondly, the authors appear to be comparing apples and oranges in this paragraph. Although I did not have the US/Canada document referred to, an examination of other Canadian data suggests that the 19,000 to 31,500 Fraser River stream-type chinook referred to includes only spawning escapement, whereas, the mid-Columbia River estimate includes total production. Ocean and terminal fisheries in the Fraser River would substantially increase the Fraser River estimates for comparable total production.

Page 101

Table 20 estimates smolt-to-adult survival rates for naturally produced spring chinook that appear unreasonably high. I believe there are problems in the 1967-87 average run size estimates and there may also be problems in other parameters such as egg deposition, egg-to-fall fingerling survival, fall fingerling-to-smolt survival, etc.

Page 102, Second Full Paragraph and Last Paragraph

The estimate of 86,000 naturally produced ocean-type chinook for mid-Columbia River tributaries during 1967-87 is not supported by the available data. This analysis assumes all these fish are summer chinook timed in terms of inriver harvest (9%). The ocean harvest rate of 75% during 1967-84 is too high and is not adjusted for adult equivalence. Adult equivalent ocean harvest rates likely averaged less than 45% for summer chinook. Estimated naturally spawning summer/fall chinook smolt-to-adult survival rates are excessive due to the overestimate of run size. (See comments pertaining to Page v.) Using a more reasonable adult equivalent ocean harvest rate of 40-45% results a lower run size estimate and a smolt-to-adult survival rate (near 2%) that is more in line with other contemporary estimates. Certain lower river hatchery facilities such as Spring Creek National Fish Hatchery have had fall chinook survival rates as high as 2.0-4.0% in the early 1970's. Survival has since declined and is generally 1.0% or less.

Page 104, Table 21

The average run size data for 1967-87 are flawed resulting in excessive estimates of summer/fall run migrant-to-adult survival rates. (See previous comments.) Where did the lower end of the range for number of migrants (000) (third row from the bottom of the table) come from for each drainage? They don't seem to be used in the calculation of migrant-to-adult survival. Wild upriver bright fall chinook from the Hanford Reach generally survive at less than 1.0% based on recoveries from recent coded wire tagging. Migrant-to-adult survival for the mid-Columbia River tributary ocean-type chinook during 1967-87 was probably not more than 2.0%.

Page 107, Second and Third Paragraphs

The crudely estimated smolt production at about one quarter million coho, with smolt-to-adult survival at 6.0 to 12.0% is just that (crudely estimated) and likely overestimates the survival rate. Oregon coastal coho, which have a much shorter freshwater corridor to negotiate and no dams for most tributaries, typically survive at about 4.0%. Survival for mid-Columbia River coho which negotiated seven to nine dams would be expected to be considerably less than for the Oregon coastal stocks. The upper end of the steelhead smolt-to-adult survival rate range (1.3-14.3%, mean 6.4%) similarly appears excessive.

As I stated at the beginning, I have basically reserved my comments to the areas of the report that dealt with stock abundance, harvest rates, survival rates, etc. I felt that habitat issues were best left to others who have more expertise in those areas. Sorry I was so slow in getting these comments to you but hope that they are helpful in your review of the document.

Timothy Roth
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November 16, 1993

Mr. Brian C. Cates
U.S. Fish and Wildlife Service
Mid-Columbia River Fishery Resource Office
P.O. Box 549
Leavenworth, WA 98826

Dear Brian;

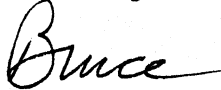
Enclosed is my review of the report "Production and Habitat of Salmonids in Mid-Columbia River Tributary Streams." Due to my background and experience, I have kept my review to Chapter VI "Impacts of Settlement on Production and Habitat."

I have a few general comments to accompany the specific discussion found in the review. I'd first like to commend the authors for attempting to synthesize information over such a broad scale. Management and research have long suffered by our ignorance of the "big picture." While I view the monograph as a good start, I think it can be improved in many ways. The habitat chapter suffers from little or no data, or studies, to support their findings. I feel the authors draw some very strong conclusions based largely on supposition. Examples of this would include the benefits of irrigation returns to Methow fish populations and the notion that moderate levels of pollution are a benefit to aquatic productivity. The objective approach would be to present these concepts as hypotheses, explicitly stating the potential positive and negative effects. At this point the conclusions are one-sided, generally positive. What the authors have done, though, is pose some very interesting research questions.

The habitat chapter contains very little synthesis of the available information. Each natural and anthropogenic influence is generally perceived as acting individually, instead of in a cumulative, synergistic and additive manner. There is no context for the impacts, either spatially or temporally. Considering the extensive body of literature on cumulative effects, I believe this is a serious omission. I would suggest the authors consider a timeline of the various impacts, when and where they were most prominent, and then a discussion of how these impacts may have influenced habitat production both individually and cumulatively over time.

Thank-you for the opportunity to review this work. I know the author's task was a monumental undertaking. We will need a better understanding of how systems operate at these scales as we move into the future. If there are any further questions, either from yourself or the authors, please don't hesitate to contact me.

Best Regards,

A handwritten signature in cursive script that reads "Bruce".

Bruce A. McIntosh
Stream Ecologist

Review of "Production and Habitat of Salmonids in Mid-Columbia River Tributary Streams"
Chapter VI "Impacts of Settlement on Production and Habitat"

Mining (p. 122)- In general I agree with the authors' comments concerning the effects of mining for minerals and metals. I also know of one other large mine on the Twisp River that was operated around the turn of the century out of a camp called Gilbert (Jim Spotts, Okanogan National Forest, pers. comm.).

In addition, Wissmar et al. (1993) discuss the impacts of **nonmetallic** mining on streams in eastern Washington and Oregon. They conclude that the value (and production) of sand, gravel, gypsum, and limestone has been several times that of precious metals. Historically, gravel was removed from the stream channel and floodplain by dredging. Current practices allow gravel to be removed from outside the zone of low summer flow. Wissmar et al. (1993) provide a good discussion of the impacts of these practices. This information should be added to portray a more accurate representation of the past and present impacts of various types of mining. It is also important to recognize that while the impacts to mining may have been localized, they can have a profound and lasting effect on stream habitat and fish populations. Mining may have affected fish migration (both juvenile and adult) by dewatering and/or blocking sections during critical periods, caused fish kills due to toxic wastes, and created other water quality problems. It is also important to note that where streams have been impacted by mining (i.e., John Day, Grande Ronde, Salmon River Basins), recovery has been minimal in the past 50 years. For a general review of the impacts of mining, the authors should consider Nelson et al. (1991).

Grazing (p. 122-126)- This section begins abruptly with no introduction or background, instead beginning with the specifics of their findings. There is no regional context within which to examine the individual river basins. My recent paper (McIntosh et al., 1993) and Wissmar et al. (1993) contain overviews of grazing histories in eastern Washington and Oregon. During euro-american settlement (mid-1800's to 1930's), cattle and sheep were grazed throughout the major river drainage's in eastern Oregon and Washington. The common practice was to winter livestock in the river bottoms and then trail them to high meadows for the summer. By 1920, the public was alarmed by the poor condition of public rangelands in the Pacific Northwest due to overgrazing. The 1930's brought the beginning of grazing reform (Taylor Grazing Act, 1934). Grazing records show there has been a decline in grazing pressure throughout the eastside, but range conditions remain poor. I have hypothesized in my recent paper that the uplands in the mid-Columbia region may be in a recovering state, due to declining grazing pressure since the 1930's, but riparian zones are still receiving grazing pressure at levels that inhibit recovery. Mullan et al. verified this in their reference to the condition of private rangelands in the Methow Valley.

I also advise caution in using Forest Service evaluations of range condition. These evaluations are of questionable design (they rarely stratify upland from riparian use) and are generally based on little data. Many riparian ecologists would argue current range utilization standards (stubble height, etc.) are inadequate to maintain riparian vegetation, particularly

woody species, let alone bring about recovery. The body of literature is clear concerning the poor condition of rangelands throughout the west (GAO, 1988; Platts, 1991). I see little evidence to the contrary in the Mid-Columbia region.

Logging and Roads (pages 126-128)- Given the minimal data and analysis provided in this section, I find the dismissal of logging and roads as anything but a minor impact an overstatement. My examination of the historical record shows that logging and road construction has been affecting these drainages for some time, though not to the extent of river basins throughout the rest of the Columbia basin.

The historical record indicates that log drives and splash dams were common throughout the Wenatchee basin (Wenatchee National Forest, unpublished historical accounts). While there are no studies of the direct effects of these practices, Sedell et al. (1991) make a strong case for the devastating impact of splash dams and log drives on fish habitat. In addition, logging practices in the early part of this century concentrated on the removal of the large trees in and adjacent to riparian zones. Log jams were also actively removed to ease the transportation of logs. These two practices removed large woody debris (LWD) from the stream channel and reduced the sources for future recruitment. Evidence of these impacts are apparent throughout the riparian zones of eastern Washington and Oregon. An examination of the number and size of stumps in the floodplain and adjacent slopes clarifies this point. The removal of log jams and LWD continued after the era of log drives, in response to fishery biologists concerns that LWD would hinder the migration of fish, and further as the result of large floods (e.g., the removal of LWD by the Army Corp of Engineers after the 1964 flood, and each major flood since then). It has only been in the last 10 years that the importance of LWD to aquatic ecosystems has become understood. These streams are now left with the legacy of 100 years of LWD removal and reduced recruitment due to log drives, splash dams, stream cleaning, and riparian logging.

Although road densities in the mid-Columbia are low (65% of the Wenatchee and Okanogan Forests are under wilderness or roadless status) compared to other parts of the Columbia basin (e.g., western Washington and Oregon, eastern Oregon), the authors fail to consider the spatial context of road development. In most developed drainages, roads (and often railroads) have been built up the valley bottoms, typically next to the stream. This causes the stream to be constrained, limiting lateral movement and reducing the streams potential to interact with the floodplain/riparian vegetation. The interactions between water, stream channels, and riparian zones are the processes that have shaped and maintained these habitats over time. In addition, road construction reduces shade by canopy removal and create surfaces that are very effective at moving sediment and water to stream channels. These effects can alter the natural temperature, hydraulic, and sediment regimes of the stream (In reality, these road networks become extensions of the drainage network).

The authors are correct in pointing out that the heaviest logging has come in the last decade. With historic impacts to stream channels and riparian zones, along with intensive logging over the past 10 years, the stage may be set for the cumulative effect of these

activities to begin influencing fish habitat at large-scales; only time will tell.

Wildfire (page 128-129)- While the authors concentrate on the potential negative effects of fire, they fail to consider the positive influences. Aquatic ecosystems throughout the Pacific Northwest evolved under the influences of fire, including periodic large-scale fires. Fires, and associated floods, are a source of large woody debris, spawning gravel, and nutrients to aquatic ecosystems. In using the Entiat fires as an example, the authors fail to consider the impact of human intervention in exacerbating the effects of fire/flood events. After the first flood, much of the LWD was removed from the river to prevent debris jams from mobilizing and destroying downstream structures (i.e., bridges, homes)(Ken MacDonald, Fish Biologist, Wenatchee National Forest, pers. comm.). What wasn't understood at the time was the role that LWD plays in buffering the effects of floods. LWD decreases water velocities, accumulates mobilized debris, and stores sediments. The "recovery" of the Entiat most likely would have occurred much faster without human intervention.

Sedimentation (pages 129-131)- Fine sediments are not the only potential sediment problems in streams. Where the bed is highly mobile due to bedload transport, pools can be filled and redds can be destroyed, often on an annual basis. This may be a more common problem in higher gradient streams than fine sediments, as they are generally more readily exported. While our survey work in the mid-Columbia region suggests that sediment problems are not widespread (McIntosh et. al., 1993), I find little data in Mullan et. al. to justify their conclusions. I would also be very cautious in interpreting Forest Service sediment data, as sediment models often have high degrees of error and are rarely calibrated to regional, let alone local conditions.

Stream Stability and Riparian Vegetation: (pages 131-136)- I would suggest combining this section with the previous section on sedimentation. As I noted in my comments for the "Logging and Roads" and "Wildfire" sections, the authors have failed to fully consider the long-term impacts of log drives, splash dams, stream cleaning, stream channelization, and settlement on riparian vegetation and channel stability. Since settlement began, humans have been affecting riparian zones. LWD was removed from streams for log drives and "flood control," and trees were harvested, first for furniture and firewood (especially softwoods because they were easy to work with hand tools), then for logs. In addition, the human response to flooding has been to "stabilize" channels through rip-rap and levees, making the consequences of future floods much worse. The 1993 flooding of the Mississippi clearly illustrates the failure of this strategy.

I also disagree with the authors assertions on page 135, paragraph two, concerning channel obstructions. LWD has played a major role in large, high gradient rivers. A visit to the Chiwawa River in the Wenatchee basin would clarify this. The reason LWD doesn't play a major role in many of these systems is for the reasons I noted in the previous paragraph. An example would be the Twisp River in the Methow basin. The Corps of Engineers pulled much of the LWD out of the system in the 1970's after a flood (Jim Spotts, Fish Biologist, Okanogan National Forest, pers. comm.). While the Twisp currently has a

functionally intact riparian zone, recruitment and retention of LWD in the channel to historic levels will take time, especially in these high gradient systems (high transport rates). I would also argue that the function of the mainstem portions of the Methow and Wenatchee rivers have been fundamentally altered by human impacts, the Methow being a good example. While the floodplain retains a significant portion of its floodplain forest (cottonwoods), the river has limited lateral mobility in many areas due to channelization and road construction. The complexity and productivity of these floodplains were shaped and maintained by large floods, not destroyed. It is time to stop viewing floods as disasters and recognize that they are the major process that shape and maintain the floodplain habitat that fish and humans depend on. Floods become disasters when humans intervene. The human response to flooding is at best a short-term solution and generally becomes a long-term liability. As Aldo Leopold said "the first rule of intelligent tinkering is to keep all the parts." Streams and floodplains that are functionally intact are capable of handling floods, the problem in the Pacific Northwest is that few streams are in this condition.

Stream Alteration (pg. 137)- The authors have concluded that "stream alteration" has been minimal, due to little documented riprap. This conclusion fails to recognize there are other forms of "stream alteration." Stream alteration can be caused by the removal of riparian vegetation, the blocking and/or isolation of secondary or main channels, and the trampling of streambanks by livestock. Where roads, railroad grades, and powerlines have been built next to streams or within the floodplain, the potential of these streams has been reduced. Since the beginning of euro-american settlement, streams throughout this region have been "altered." The historic impacts I have discussed in the previous sections confirm that the streams of today are far different than they were in the past.

Given this context, I find the authors' suggestion that riprap provides habitat equal to, or better than natural habitat a very dangerous conclusion. The question becomes "relative to what?". Relative to non-riprapped habitat, or relative to what was available historically. Past research (Cederholm and Koski, 1977; Chapman and Knudsen, 1980) concluded that channelization **reduced** habitat quality (cover, woody debris) and quantity. If Mullan et. al. conclusions are carried to the extreme, fish habitat would be enhanced if the **entire** stream was riprapped. I'm sure this is not what the authors meant. Nonetheless, they draw some strong conclusions based on little data and no context.

Agriculture/Irrigation (pages 137-142)- The authors state that very small percentages (1-3%) of the watersheds are farmed; a more pertinent question might be what portions of the floodplain and adjacent terraces are farmed/irrigated. This would provide more insight into the extent of irrigation and the impacts of agriculture on the floodplain/stream channel (channelization, removal of riparian vegetation, etc.). In considering the influence of irrigation returns on groundwater and streamflows, the authors failed to consider some fundamental issues. As a "rule of thumb," at a **maximum**, 50% of irrigation flow is returned to the "system" as groundwater or irrigation return, most of the flow is lost to evaporation and plant growth (Jon Rhodes, Hydrologist, Columbia River Intertribal Fish Commission, Portland, Oregon, pers. comm.). This results in a **reduction** in streamflow,

not an **increase**, as the authors suggest. Besides reduced water quantity, the discussion fails to consider water quality. Irrigation returns are warmer than ambient streamflow and groundwater, besides carrying and concentrating pollutants (pesticides, fertilizers, silt, salts, etc.). Refer to Wissmar et al. (1993) for a more thorough discussion of this issue.

Contaminants (pages 142-144)- I have trouble with the suggestion that pollution has increased fish production, possibly as much as 25-50%, according to the authors. These conclusions have been drawn from little data and no formal studies. There are many problems with "nutrient supplementation," such as whether the nutrients are available in usable forms, and whether they influence portions of the food web that would increase fish production. I would advise great caution in suggesting that pollution is beneficial to aquatic environments, no matter how minimal it may be.

Dams and Diversions (pages 145-147)- The question of irrigation diversions providing suitable habitat is open to debate. Again, these are issues of context, "relative to what?" If there is little or no off-channel habitat available, irrigation diversions may be the only choice. Given a choice, I believe juvenile salmonids would choose naturally occurring habitats. They offer a diversity of habitat types and complexity not common in irrigation diversions. At best, irrigation diversions are ecological slums as compared to natural habitats. We have traded highly dynamic and complex off-channel habitats for ditches. There is also the question of movement. The ability of fish to return to mainstem habitat in unscreened diversions is largely determined by the availability of water, which is controlled by the irrigator. Screening is an effective method (97-99% according to the authors) for preventing fish from becoming trapped.

Impoundments (pages 147-148)- Refer to comments in section on "Logging and Roads" and "Stream Stability and Riparian Vegetation."

Predation (pages 148-151)- I agree with the authors that human predation has, and continues, to be an influence on native fishes. Humans have also altered physical and biotic processes. The practice of releasing large quantities of catchable trout not only increases the harvest of non-target species (i.e., salmon and steelhead), but alters the balance of fish communities. In addition, they provide direct competition for salmon and steelhead. The key point in all these discussions of biotic impacts is that human intervention (abiotic and biotic) has altered the balance of nature in a manner that is often most favorable to non-native and less common species. All these factors act in a cumulative manner to reduce survival for native species throughout their life-cycles.

Genetic Alteration and Loss (pages 151-156)- no comment, beyond my expertise.

Hatcheries (pages 156-157)- no comment, beyond my expertise.

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EASTERN

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CHENEY • SPOKANE

September 28, 1993

Mr. Brian C. Cates
Fish and Wildlife Service
Mid-Columbia River Fishery Resource Office
P.O. Box 549
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Dear Brian:

Thank you for the opportunity to review the document entitled "Production and Habitat of Salmonids in Mid-Columbia River Tributary Streams," by James Mullan, et al. (1992). You should be advised that Jim and I discussed some of the ideas contained in this volume prior to publication, specifically those relating to historic and prehistoric Indian fisheries and salmonid use in the Mid-Columbia, and I reviewed a draft version of Appendix G by Mullan and Williams. While I have strong opinions on other sections of this document, I will restrict my remarks to the information in Appendix G as it is the only content I am professionally qualified to address. In that I have reviewed this information previously, I will keep my remarks brief. The research presented in Appendix G is an extremely important and timely addition to a topic that has been hotly debated in anthropological/archaeological circles for some time and I am pleased to see it included in this volume.

First let me say that the authors have treated historic and ethnographic information pertaining to Indian population sizes and the subsistence cycle of Mid-Columbia peoples in a fair and forthright manner. Having said this, it should be obvious to all readers that other researchers can (and I'm sure will) provide revised estimates of Native salmon use simply by placing greater weight on different ethnographic sources and recalculating numbers, ranging from average fish weights and run numbers to human group sizes, that, as clearly stated, are rather crude estimates of historic and prehistoric reality. For this reason, I am naturally suspicious of any who would argue forcefully against the conclusions reached by the authors. It should also be understood that in today's litigious environment, such formulations will be used and debated by many who have little knowledge of or interest in the research methodologies employed. That the conclusions reached by Mullan and Williams are sound and accurately reflect available information sources should in no way imply that they are the final and "correct" estimates of Native uses of salmonids and I'm confident this was not the authors' intent.

If there is a weakness in this presentation, it is, in my opinion, in the brevity of the introduction to the topic. Most readers of this volume will not understand fully the problems associated with the basic data categories employed in this study. They are so important in part due to the fact that many would attempt to extend the conclusions reached by the authors from the immediate post-contact period into prehistory. It should be stressed that not only is this not possible, the estimates of salmonid usage provided in this (or any other similar) study is relevant to no more than approximately 100 years of native history. It would be terribly unfortunate if this fact was not understood by either the lay or professional reader. Although the authors reference some of the problems associated with

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calculating estimates of this type, there are so many problems involved in this process that it would have been advisable to spend more time in reviewing them for this presentation. A few of these problems can be summarized as follows:

1. The processes that transformed post-contact cultures (e.g., epidemic diseases, Euro-American encroachment, introduction of the horse) had a profound affect on all aspects of native life including subsistence practices. Fish probably were over-exploited during this period as part of the widespread transformation of cultures in this region since they were easily exploited. All members (males, females, old, and young) could participate in the capture and processing of fish on a routine basis. Thus, the concentrations of native peoples at major fishing locales on the larger rivers brought them into contact with Euro-Americans and their goods thereby furthering the transformation process.

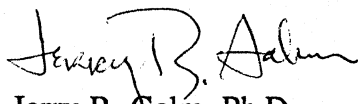
2. Village sizes increased as a result of the reformulation of groups decimated by epidemic diseases and from a general cultural transformation that included introduction of the horse. Villages of the sizes mentioned in this study or the ethnographic record have yet to be documented in archaeological sites dating to prehistory, including those of the last 500 years or so. The concentration of such villages around demonstrated fisheries is understandable for a people increasingly being cut off from other traditional food resources.

3. Like the village size issue mentioned above, the importance of fish in prehistory is a topic that remains clouded in the archaeological record. While few would argue that salmonids had little importance in subsistence pursuits for at least the last few thousand years of prehistory, empirical evidence of salmon use in the archaeological record is spotty at best. When compared to cultures in prehistory from around the world who have been known to have depended on fish for survival, the archaeological record in our region contains too many gaps for any researcher to ignore.

These and other related issues are concerns affecting both the paradigm underlying the present analysis and the diverse ways in which readers are likely to use these data. Accordingly, it would have been appropriate to spend a little more time in spelling out assumptions and the nature of the database before proceeding. Despite this criticism, I would repeat that the authors have done a thorough job of presenting and analyzing the information at hand. Their analysis is certainly reproducible for those interested in pursuing this topic and will no doubt foster much productive dialog in the archaeological community.

Please do not hesitate to contact me if I can be of further assistance in this review process.

Sincerely,


Jerry R. Galm, Ph.D.
Director

Phillip R. Mundy, PhD

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January 5, 1995

Mr. Brian Cates, Project Leader
U.S. Fish and Wildlife Service
Mid-Columbia River Fisheries Resource Office
P.O. Box 549
Leavenworth, WA 98826

RE: Peer review comments on Mullan, J.W., K.R. Williams, G. Rhodus, T.W. Hillman, and J.D. McIntyre. 1992. Production and habitat of salmonids in mid-Columbia River tributary streams. U.S. Fish and Wildlife Service, Monograph I.

Dear Mr. Cates:

These comments are provided in response to your request. As you and I have discussed, since the document was already in final form when I first saw it, I do not feel it is appropriate to give the same type of detailed comments that I would give in the peer review of a manuscript submitted for consideration for publication in a scientific journal. My comments are the equivalent of a "book review" that I would write for a scientific audience.

Mullan et al. have drawn together a wealth of basic scientific observations on the biology, physiography, climate, and other types of information critical to understanding the production of salmonid fishes within mid-Columbia river tributary streams. The data are presented in a logical progression of well organized tables, graphs and photographs which any researcher in the field of salmon biology will find both useful and easy to use. The data are drawn from historical and contemporary literature sources, and they include original observations gathered by the authors. The authors deserve high praise for a well organized and lucid compendium of basic data relevant to the production of salmonids in this geographic area.

Visionary scientific editors usually counsel the inclusion of as much primary data in a paper as the space will permit, since it is a well known principle of scientific writing that conclusions and hypotheses live far shorter lives than the observations on which they are based. Mullan et al. is an excellent example of this principle, for many of their conclusions, interpretations and hypotheses are unlikely to survive any sort of scrutiny. There is a pronounced tendency in certain sections to confuse speculation with hypothesis formulation, and to incorrectly cite literature sources in support of hypotheses. I am not qualified to

examine the validity of each and every hypothesis contained in this lengthy publication, nor am I about to attempt to identify each instance where I believe concepts have not been adequately supported by data or appropriate literature cites. I can present some examples of serious faults from my own areas of experience to give the reader some idea of what level of skepticism to apply in working with this volume.

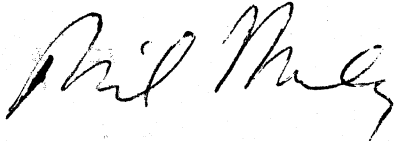
Mullan et al. (1992) has mischaracterized the important historical work of Craig and Hacker in several instances. For example, "Overfishing of preferred chinook stocks forced commercial fishermen to turn to steelhead in the 1880's (Craig and Hacker 1940)." (p. H-290). This characterization is inaccurate in a number of regards. While Craig and Hacker found lack of availability of spring and summer chinook stocks during this time to cause the fishery to move toward late summer and fall chinook stocks, there was no intentional switch to target steelhead as stated by Mullan et al. (1992). Further, Craig and Hacker found the effects of overfishing to be one of only several *possible* causes for the decline of Columbia spring and summer chinook stocks which caused shifts in the dates of fishing. The decline in markets and associated fishing effort, and the loss of spawning and rearing habitat were two other possible causes of decline cited by Craig and Hacker that were not reported by Mullan et al. (1992).

The work of Craig and Hacker was further mischaracterized on page G-253 where their estimate of total annual aboriginal salmon consumption was described by Mullan et al. as a, "house of cards". No objective criticism of the methods, data, or literature sources of Craig and Hacker were given to substantiate this pejorative label. Rather, the fact that historical sources have differed in estimating aboriginal consumption was equated by Mullan et al. (p. G-254) to unreliability. Such dismissal of important historical sources by innuendo has no place in scientific writing, in my opinion. Further, given that the geographic reference frame of Craig and Hacker's estimate is far broader than that of Mullan et al., it is unclear why Mullan et al. needed to dismiss it at all. Later on, Mullan et al. rely upon the daily per capita aboriginal salmon consumption estimate of Craig and Hacker (p. G-263 to top G-264). Since the daily estimate is a cornerstone of Craig and Hacker's total annual estimate, the "house of cards" pejorative is even less acceptable in a scientific work, since their estimate is rejected when convenient, and then subsequently rehabilitated when that becomes convenient.

A third and final example of why I advise approaching Mullan et al. with a higher than average level of scientific skepticism is found on page H-307, "The dependance [sic] on hatchery fish carries genetic risks (Goodman 1990; Hilborn 1991)." Without questioning the actual validity of this statement, I note that Mr. Goodman is not a geneticist, and that Goodman 1990 is a law review article, that Dr. Hilborn is a population dynamicist, and that Hilborn 1991 is an "opinion piece" article in a fisheries magazine which contains no data which could be used to evaluate genetic risks. To cite "authorities" in such a manner is irresponsible, to put it mildly. I also note that the limited amount of empirical data which is available in support of the genetic risks assertion is contained in publications which were not even cited in this section.

Given the increasingly desperate and expensive nature of the struggle to save what remains of the Columbia River basin's salmon populations, it is not surprising to me that so many scientists were more than a little concerned at the publication of Mullan et al. by the U.S. Fish and Wildlife Service. Even though the first amendment guarantees us all the right to express any opinion short of shouting, "fire" in a crowded room, I believe the expression of opinion by a scientist carries with it much heavier responsibilities. These responsibilities are particularly heavy for those who work with salmon in the Columbia basin. Expression of scientific opinion in the tradition of western culture requires a carefully established empirical base which includes all relevant observations. Put simply, a hypothesis is an explanation which is consistent with all the observations. While an absence of observations invites speculation, speculation should be left outside of scientific publication. In those areas I felt competent to evaluate, there was too often a tendency for the conclusions of Mullan et al. to far outstrip the empirical base, and for the content of the literature cited to be far afield from the hypotheses they were supposed to substantiate. These lapses detract from an otherwise very important compendium of data on mid-Columbia salmon production, and they left me very much disappointed in the document as a whole.

Sincerely,

A handwritten signature in dark ink, appearing to read "Phil Mundy", written in a cursive style.

Phillip R. Mundy, PhD

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Mr. Brian Cates
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Dear Brian:

Thank you for the opportunity to review the report "Production and Habitat of Salmonids in Mid-Columbia River Tributary Streams" by Mullan *et al.*. I have limited my review to the sections which addressed losses attributable to dams. Within this context there is very little comment I can make, but I offer the following.

In general, I find the key conclusions section (p. iv through viii) difficult to follow, confusing and in some instances based on faulty logic. For example:

page vi, the first key conclusion implies that wild steelhead have failed to sustain themselves because of the successful hatchery steelhead program. This conclusion is not supported by the text.

page vi, the fourth key conclusion is unclear as to what the authors intend. It appears that they imply that dams may not be detrimental to salmon since the overall survival of spring chinook from the Mid-Columbia is similar to that for lower River hatchery fall chinook below the dams. The authors do not state under what environmental conditions, for what years etc. There are several reasons why fish do not survive, and simply comparing stocks from different geographic locations is not appropriate. The fact that many types of development kill fish does not reduce the impact of the hydrosystem.

page vii, the second key conclusion states that "irrigation, at least at current levels in the Methow River basin, may be more beneficial than detrimental to salmonid habitat because of its positive influence on groundwater". This statement is a rather bold conclusion given that the authors do not address the additional impact due to dewatering, or lowering water levels when air temperatures are high, which leads to increasing stream temperatures. They also do not address the consequences of adding additional nutrient load or pesticide accumulation in the groundwater. The authors are reaching for a conclusion without the benefit of data or analysis.

On page 145 the authors imply that the need for fishway improvements at Dryden and Tumwater may have been exaggerated because the numbers above the projects did not increase after the fishways were modernized. Numbers of fish to a specific area will be dependent on total population numbers. The utility of an adult facility is determined by the reduction in delay, timing and fish condition. Once again the authors present information without the data, and use it to make conclusions. Adult passage numbers above the project incorporate the adult facilities as well as the combined impact of juvenile and adult migration conditions in the mainstem. The authors present no basis for their conclusion.

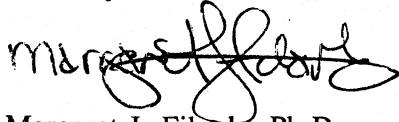
In addition, I am concerned regarding the author's contention that Mid-Columbia Reservoirs would be suitable as exploitable salmon habitat, page 160. Given the fact that these stream channels have silted-in as a result of being impounded leaving little suitable spawning habitat I am unsure what the authors



are suggesting. In addition, reservoir temperatures are higher and the effect of impoundments on increasing in-river migration time is well documented. Together these factors negatively affect the survival of salmonids as a result of impoundment, rather than presenting exploitable habitat.. Again, the conclusion is more conjecture than fact.

In summary, the document presents several conclusions that cannot be substantiated with the data or analyses presented. The overall significance of the report is questionable.

Sincerely,

A handwritten signature in black ink, appearing to read "Margaret J. Filardo", with a stylized, flowing script.

Margaret J. Filardo, Ph.D.