Comments on the "Peer Review of "Final Report-Instream Flow Assessment Pilot Project (draft 9/9/05)"

By Greg Blair, Larry Lestelle, Kevin Malone, Chip McConnaha, and Lars Mobrand

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We are responding to the review of the Stillaguamish instream flow assessment entitled, "Peer Review of "Final Report- Instream Flow Assessment Pilot Project (draft 9/9/05)". The review was authored by Derek Booth of the University of Washington, Paul McElhany of the National Marine Fisheries Service and William Trush of the firm, McBain and Trush. Although commissioned as a review of the instream flow assessment project conducted by Snohomish County and Northwest Hydraulic Consultants Inc., the bulk of the review is a critique of Ecosystem Diagnosis and Treatment (EDT) method in general. Our comments address inaccuracies and errors in the reviewers' statements regarding EDT. We have not reviewed how EDT was applied to the task in the Stillaguamish.

Over the past decade and more, state, county, city and tribal resource managers across the Pacific Northwest have participated in the development of EDT and have actively used it for watershed planning and most recently for development of recovery plans for ESA listed salmon and steelhead. Their good work and efforts deserve fair and constructive review. Partisan model bashing serves little purpose and leaves the region with more confusion and few options for moving forward. We continue to believe that scientific review is an essential element of salmon recovery that can be done in a collaborative manner designed to produce better tools and techniques.

Contrary to Booth et al., we start from the premise that relating habitat conditions to the needs of salmon is important and useful to decision making affecting the environment. We contend that part of the failure of past salmon and steelhead recovery efforts has been because these efforts only focus on a narrow component of the life history or one or two "limiting factors". Booth et al. suggest that, because science is unable to understand the complexities of salmon life history and their relationship to the environment, integrated modeling efforts, such as EDT, should not be undertaken. This view ignores the knowledge gained through extensive studies on salmon life history in freshwater, estuarine and marine habitats. Furthermore, we contend that recovery planning based on this view will lead to failure as it does not account for the complexities of salmon life history and the need to develop a plan that evaluates all habitats used by the population in question. In addition, the task of the Stillaguamish analysis set by Washington DOE was to evaluate "flows for fish". Hence, any constructive review must compare EDT to other available alternative methods for accomplishing this task.

First, we should clarify the nature of EDT. Models are constructed to solve specific problems. Each one has specific purposes and limitations that must be understood in order to review the model and its results. EDT is a knowledge-based planning model designed to help create a scientifically based working hypothesis regarding the effect of habitat condition on the biological potential of a salmon population. It is mechanistic in that it addresses individual components of fish performance through a library of salmon-habitat relationships gleaned from the scientific literature. EDT assembles the components of the aquatic environments as they relate to salmon into a working hypothesis that serves as the basis for salmon recovery actions. EDT is a description of how these components fit together to determine fish performance that is consistent with the scientific literature. EDT is descriptive and does not attempt to predict fish abundance at some future date or to quantify the uncertainty in its prediction of biological potential. However, knowledge and data limitations are recognized and documented as part of the EDT process.

This description of EDT contrasts with models that use statistical techniques to understand the relationship between two or more variables. Statistical models have the advantages of apparent simplicity and being able to quantitatively characterize uncertainty. However, statistical models do not explain underlying relationships or help assemble the myriad components that make up a restoration strategy (see Hilborn and Mangel 1997; pages 32-33 for additional discussion on this subject). Because of compounding correlations between variables, statistical models need to address the smallest possible number of variables (parsimony).

Our purpose in contrasting the type of model embodied in EDT and statistically based models is not to say that one is better than the other. Instead we stress the need to understand the purpose of a model in order to give it a fair and useful review. It is possible to conclude that a model is inappropriate for a particular problem without condemning the model in general. Based on their comments, it is apparent that Booth et al. are most comfortable with traditional statistical models and viewed EDT through that lens. Not surprisingly, they found it lacking.

Many observers have noted the need for multiple models and approaches in development of fish recovery plans and evaluation of alternative restoration strategies. We endorse this idea and stress that effective habitat management will employ an array of models that address different aspects of the problem. For questions related to probability, statistical models are clearly the tool of choice. However, a statistical model will not help planners understand how salmon performance is related to habitat characteristics in their watershed or to construct a logical restoration strategy based on available knowledge. For this, a planning model such as EDT will be needed. Development of effective strategies for recovery of salmon and their ecosystems will employ many types of tools in their appropriate fashion.

To quote from Kareiva and Mobrand (1999):

"The general philosophy of EDT is well grounded in established scientific principles describing the influence of habitat structure on the characteristics of biological communities. Specifically, EDT assumes that habitat characteristics determine biological performance. Hence, changing habitat through natural events or by human action will have a corresponding effect on biological performance.

A second philosophical point is that EDT recognizes that the scientific basis for decisions regarding the future of the Columbia River cannot and will not be limited to statistically "proven" knowledge. While statistically based information is important, scientists are increasingly aware that the complex and dynamic nature of ecological systems is currently, and perhaps always will be, imperfectly captured in statistical *relationships. Prudent management must take advantage of all available information. For this reason, EDT uses both statistically-based and heuristic knowledge."*

In this response, we will address four general categories of charges made by Booth et al. First, they conclude that EDT has not received adequate scientific review, second, they fear that EDT provides a false sense of precision to decision makers, third they find EDT overly complicated and "over-parameterized" and finally, they address certain specific points that they feel are in error. We will address each of these four points in this overview. We also attach more detailed comments that relate to the specifics of their last point.

1. Peer review of EDT

Critical review and examination of models and conclusions are essential parts of the scientific process and effective natural resource management. However, Booth et al. state that EDT "does not pass even the lowest standards for a scientific peer-reviewed framework…" This is simply nonsense. We submit that <u>EDT is the most documented, the most thoroughly reviewed and the most commonly understood tool used in salmon recovery in the Pacific Northwest</u>. We make this statement on the basis of the following facts:

a) The results of EDT have been validated repeatedly by hundreds of state, county, city and tribal biologists that have applied the model in nearly every salmon-bearing stream in Puget Sound and the Columbia Basin. No other tool has the track record for utility or validation enjoyed by EDT. Users of EDT first check the predicted abundance of salmon in their stream against their observations of fish abundance. This is followed by more detailed examination of results for specific life stages and stream reaches against the knowledge of local biologists based on years of observation. While even the most seasoned field biologist usually finds new insights from an EDT analysis, in nearly every case the results of EDT are consistent with observed behavior of salmon—no one would use it if it did not. The results of validating EDT against observed behavior of salmon is documented in numerous EDT-based reports. We reference the numerous Columbia River subbasin plans prepared by state, tribal and local planners for the Northwest Power and Conservation Council, most of which are based on EDT analysis. Dan Rawding (2004) of the Washington Department of Fish and Wildlife has completed the most formal validation of EDT results to date. For streams where sufficient empirical data was available to construct a spawner-recruit relationship, Rawding found the EDT estimate of production parameters to be remarkably accurate. Managers and planners would not consistently return to EDT if it was not useful, or if better tools were available. While, this review process may be less formalized than that required for academic advancement, it is equally scientific and passes any test for common sense and practicality.

b) Over the last decade, EDT has been the subject of numerous scientific reviews conducted at watershed and broader regional scales. EDT is currently undergoing extensive review by the U.S. Bureau of Reclamation, the Washington Department of Fish and Wildlife and the National Marine Fisheries Service. We know of no other model in the region that has had the level of review afforded EDT. The current review involves tens of thousands of runs of the model over a range of conditions. We have participated in these reviews to answer questions and provide documentation needed to understand every computational component of EDT. We have provided hardware and software support. NMFS was provided with their own working version of the EDT model. We note that the second author the Booth et al. review has been a full participant in the NMFS review and we are honestly mystified and discouraged by the disconnect between this review and the collaborative efforts we've participated in with NMFS.

c) Documentation for EDT is exhaustive and freely available to an extent not seen in any other model used in salmon recovery planning. The EDT library (www.mobrand.com) contains complete descriptions of EDT logic, algorithms and attributes. As part of the above mentioned review of EDT by NMFS we added to the existing documentation of all algorithms. All species rating rules in EDT are freely available. We created, and provide in the EDT online library, software to explore the implications of each habitat-rating rule in EDT. Each attribute used in EDT is thoroughly documented as to its relevance to salmon performance. A partial bibliography of EDT documentation, applications and papers is provided as an attachment to these comments.

Booth et al. conclude that EDT does not meet standards for "best available science" because of lack of peer review and publication record. Examination of the record will show, in fact, that EDT meets the criteria for "best available science" to a far greater degree than does any other model currently applied to salmon recovery in the Pacific Northwest, including those developed and used by federal salmon recovery scientists.

Booth et al. make a valid point in arguing for use of OpenSource code in models and we hope that the region can move toward this more collaborative approach to software development. EDT was developed before notions of OpenSource software became widespread. In fact, none of the models currently used in salmon recovery in the Pacific Northwest meet this standard at the present time. We believe the idea has merit and we have encouraged development of EDT applications by others. It is our intention to move toward OpenSource standards in future EDT development.

It should be noted however, that in an OpenSource environment it is the users of the model that will determine its usefulness, not academicians. As noted previously, the model is being tested and used throughout the fisheries community. Any anomalies in rules, assumptions, and documentation are being sent to us for correction and refinement.

2. False precision

Booth et al. feel that EDT delivers a false sense of precision because results are point estimates ("to the fish") and no quantification of precision is provided. All models provide point estimates, often to absurd levels of precision—these are computers after all! This is not a failing of the model. It is up to the user to decide an appropriate level of significant figures. Any model, EDT included, needs to be used with judgment and knowledge of its limitations. We have found most users of EDT to be sufficiently savvy to use EDT results responsibly and professionally. It is not a statistical model so quantification of uncertainty in EDT is not appropriate. However, we have provided the capability within the model for a qualitative assessment of precision of the data and functional relationships.

Booth et al. find fish numbers an unsatisfactory metric for assessing habitat potential. However, we find that the ability of EDT to assess habitat in biologically meaningful terms to be one of its greatest assets. It would be simple in EDT to create a 0-1 index of habitat potential as is used in many Habitat Suitability Index models. The difficulty with this is that an index provides no basis for validation. For almost any user of the EDT the first check on model utility is to compare the predicted fish performance under the current habitat condition with observed performance. EDT is the only model we are aware of that creates a biologically verifiable index of habitat condition. Despite their dismissal of model validation using fish numbers, Booth et al. attempt to critique the model based on their comparison of EDT predicted fish performance to their own estimate of fish performance. While we believe their "validation" to be incorrect, the fact that the type of output provided by EDT allows us to have this discussion illustrates the value of the feature of EDT. We address errors in the computations of Booth et al. directly in the attached comments.

3. Over parameterization

Booth et al. find EDT to be overly complicated and to include too many variables for most of which, they assert, we have no data. They conclude that, "In attempting to include every possible functional relationship, the model greatly outstrips the available empirical information for habitat attributes and function relationships between habitat and fish performance." We couldn't disagree more. EDT defines salmon performance on the basis of 16 survival attributes that are rigorously defined by "rules" that relate fish survival to 46 environmental attributes. The EDT environmental attributes include such things as large wood, sediment, temperature and channel form-- attributes that are found in nearly any standardized habitat assessment protocol including those in use by the State of Washington, the State of Oregon and the U.S. Forest Service. If we do not know, with some certainty, that these attributes are important to fish performance, why does the region devote time and money to collecting so much data about them? The functional relationships between these parameters and salmon survival are well documented in hundreds of scientific reports and papers (see Lestelle et al. 2004b for a discussion of the literature relative to EDT). If, despite this, we cannot make conclusions about how factors such as large wood, sediment, temperature and flow relate to fish performance, how can we advocate major habitat restoration efforts to recover salmon? True, our knowledge lacks the precision or statistical power desired by some, yet the chimera of scientific certainty regarding behavior of complex ecological systems is simply not to be had. We believe that the region has information upon which to make intelligent conclusions regarding the causes of salmon decline and to make informed decisions regarding restoration priorities. EDT has been developed and used by the fishery managers to organize and document that information and create a logical basis for habitat restoration.

Booth et al. note that most variability in EDT results can usually be explained by a small subset of the environmental attributes. This will come as no surprise to any user of EDT. Indeed, it is what you would expect—large wood, flow, temperature and sediment are broadly recognized habitat limitation in nearly all Pacific Northwest streams. What EDT provides is a way to move beyond this truism to create finer scale conclusions, relate habitat change to fish performance and compare alternative habitat restoration strategies. The 46 attributes in EDT can be thought of as a "lexicon" of words to describe the environment of salmon—likely a paltry number in reality. Even as every word in our vocabulary is not needed to express any one thought, every attribute in EDT is not needed to describe the important aspects of any single stream. Yet which words in our EDT vocabulary do we discard? We generally do not know, *a priori*, which attributes control fish performance in each reach and are often surprised when the analysis provides new insights on key limiting factors. We have concluded that the best approach is to describe the environment as best we can using available information and then assess the relative importance of each attribute. The result is a ranking within a watershed of the commonly measured attributes of fish performance.

For example, a thorough reach-by-reach review of all 46 attributes in the Cowlitz River resulted in the conclusion that disease (*C. shasta*) could be a major factor limiting Chinook and steelhead production. Although the disease was present historically, the construction of the Cowlitz Hatchery Complex had apparently increased disease load in the lower Cowlitz River. As a result of the hypothesis formulated through EDT, studies were initiated to determine mortality rates on juveniles exposed to the disease for various time frames. Results indicated that for susceptible species, mortality or infection rates were as high as 100% (Harza 2000).

Booth et al., citing McElheny and Steel, in preparation, note that EDT is very sensitive to assumptions regarding the species benchmarks. They describe the benchmarks as a "hidden" part of the model. In fact, the dependence of EDT on the benchmarks is a well-documented feature of EDT (Lestelle et al. 2004a). The benchmarks define the maximum possible survival for each life stage and are set for each species based on review of the scientific literature. EDT uses the species-habitat rules to "downgrade" the species benchmarks to reflect habitat conditions in the target stream. This procedure ensures that productivity and capacity values computed for each life history segment are bounded by the biological limits of the species and scaled consistently across time, space, and life stage. Consequently it is to be expected that EDT results can change if the benchmarks are changed. Survival could be "dialed-in" simply by manipulating the benchmarks. To prevent this, the benchmarks are integral parts of the EDT species rules and are a protected, but fully disclosed part of the model.

Sensitivity analysis of EDT must be approached intelligently with an understanding for the biological and geomorphic linkages that exist within stream ecosystems. Sensitivity analysis is not a simple mathematical exercise of varying parameters within EDT at random. For instance, combinations of benchmark parameters that produce estimates of species productivity and abundance that are not supported by the scientific literature should be eliminated from the analysis. In a similar vein, many environmental attributes in EDT are linked—flow and channel form, for example.

Unless these linkages are accounted for, sensitivity analysis will falsely characterize the sensitivity of EDT.

4. Specific points

Booth et al. make several points regarding specifics of EDT. We disagree with their conclusions and believe they reflect a superficial understanding of EDT, coho salmon biology and the Stillaguamish River. However, this type of discussion, perhaps in a more constructive tenor, is exactly the type of review that is needed for the region to refine and develop tools like EDT as a means to evaluate the impacts of environmental change in salmon performance.

Two specific points raised by the reviewers merit special attention because they illustrate points discussed earlier. The first is their assertion of technical errors in the definition of certain attribute rating in EDT including especially the rating definitions for turbidity or suspended sediment as indexed by SEV. In our attached detailed comments we argue that Booth et al. are incorrect in their interpretation of how best to apply SEV in general and how it is used in EDT specifically. The important point here is that the assumptions in EDT, as exemplified by Turbidity and SEV, are clearly laid out and documented such that useful technical discussion could lead to refinement of how the region handles an important attribute like turbidity. We invite the reviewers to join in a constructive effort to review and refine the attributes and rules used in EDT.

A second specific point raised by Booth et al. is their conclusion that the results of EDT are in error. Specifically, they conclude that the estimated historic or intrinsic potential of coho in Church Creek, a tributary of the Stillaguamish River, is unreasonably high and provide their own estimate of historic potential as proof. We noted earlier the irony of this line of reasoning given their previous assertion that the use of fish numbers was an inappropriate metric for watershed evaluation. However, we use this discussion as a prime example of the strength of EDT and why its use of fish numbers is so valuable. Regardless of our opinion regarding the conclusions of Booth et al. on the potential of Church Creek to support coho salmon, EDT allows that discussion to occur in a way that would not be possible with any other metric for habitat potential. The EDT index of habitat potential can be validated, or invalidated, by comparison to empirical numbers just as Booth et al. attempt to do. Others can judge the strength of their conclusions relative to those of EDT and our own EDT-independent assessment.

The estimated pre-development abundance of coho in Church Creek from EDT is 1,500 adults. Booth et al. conclude that EDT significantly overestimates abundance and cite as evidence their opinion regarding realistic abundance in a comparable northern California under pristine conditions to be only 300-500 fish. We believe that a better comparison is found closer to home and provide evidence from Snow Creek a stream of similar size that flows into Discovery Bay near Sequim, Washington. Although far from pristine, the stream is relatively intact and WDFW has studied the stream for some years providing ample empirical data on coho production. In the attached comments, we use information from Snow Creek to show that the estimated intrinsic potential of coho in Church Creek is quite realistic for pre-development streams in Puget Sound.

In conclusion, we thank Booth et al. for their review and appreciate the opportunity to respond to their comments. While we suggest that there are more constructive approaches that will better serve the region's interest, such reviews serve to provoke thought. They also suggest fruitful areas for future model development—the use of OpenSource standards and the need for a broader review of the EDT rules and attributes, for example. A planning model such as EDT needs to continually incorporate new information, ideas and technology. It is our hope that ongoing efforts to study EDT will lead to improvements in the tool and enhance the ability of the region to devise effective plans for habitat management and salmon recovery. We invite the reviewers to enter into constructive dialogue with the aim of helping the region devise effective tools to guide salmon recovery.

Attachment 1. Detailed responses to specifics of the Booth et al review

1. EDT parameterization

Booth et al. assert that the rules developed and applied in EDT are poorly parameterized. To make their point, they focus their criticism on one subset of rules—those that address the effects of suspended sediment (SS) on fish performance. These rules are formulated using the Severity of Ill Effects (SEV) scale derived from Newcombe and Jensen (1996). The SEV, as it is referred to, is based on an extensive meta-analysis of 80 published and documented reports on the effects of SS on fish performance. The work of Newcombe and Jensen is the most comprehensive quantitative synthesis ever done on the effects of SS on fish.

Booth et al. draw two conclusions with regard to how the SEV is applied in EDT: 1) that the SEV scale is miscalculated in EDT and 2) that the rules that then apply the scale are not accurately formulated. They conclude that the resulting rules (hence modeling results) are clearly wrong. They assert from this example that "EDT seems poorly equipped, given what we know, for handling cumulative watershed effects from fine sediment."

These criticisms warrant careful examination.

Before doing so, some background on the SEV is helpful here. The SEV scale (Table 1) is derived from a synthesis of many studies on the effects of SS on various fish species and life stages. The SEV index is calculated from the two aspects of the dose of SS that fish experience, i.e., the concentration of SS that fish are exposed to and the duration (in hours) of the exposure. The index is computed with parameters specific to species and life stage. In river environments, fish can be exposed to elevated SS concentrations for extended periods of time where fine sediment is constantly entering the river over many continuous days—even weeks, or for much shorter periods during storm events. Runoff during storm events often results in elevated SS, a condition made worse under many land use practices. In the first case, where SS is elevated continuously, duration of exposure is easily known—it is simply the total time that fish are present within the area where high SS occurs. In the second case, exposure can occur over multiple storm events. In the Pacific Northwest, several storm events can occur within a single month such that SS might become elevated, followed by a period of clear water, followed again by elevated SS, and so on. In this case, what is the appropriate duration of exposure to apply, i.e., should the SEV be calculated using exposure during a single event (continuous, uninterrupted exposure only) or should it be made considering the total exposure over multiple events (including only periods of elevated SS)? Newcombe and Jensen (1996) did not explicitly address this question in their paper.

Booth et al. assert that they know the answer to the question and it is not the one applied in EDT. In EDT, the duration that is applied in calculating the SEV is the total number of hours during a month when fish would experience the elevated SS, regardless of whether there are intervening periods of clear water. Booth et al. find this to be a serious error. They state emphatically: "SEV is calculated from the number of continuous hours that the threshold suspended sediment concentration is exceeded. This makes a very big difference in the SEV outcome: the EDT analysis would overestimate SEV and therefore overestimate turbidity effects on salmon populations…" As if to denigrate how it is done in EDT, they add: "Any thorough peer review of EDT likely would have recognized this error already." They imply that this error is so obvious as to raise questions about other items contained in EDT.

Our response to Booth et al. on this point is that they are mistaken. We have several reasons for stating this. First, even a casual review of the types of studies included by Newcombe and Jensen (see Appendix Table A-1 in their paper) illustrates this. The durations of exposure of the many studies applied vary widely and come from many types of observations on suspended sediment. While it is true that many of the studies are controlled experiments where SS concentration and duration are prescribed (with uninterrupted exposure), many other studies apply observations from natural systems where concentration and corresponding exposures would necessarily be variable. Some of those studies involve situations where multiple exposures (as described above) would occur over relatively short periods such as a month.

Second, the SEV scale itself (Table 1) implies that it is meant to be applied in situations where the SS concentrations would not necessarily be continuous and uninterrupted. For instance, index values of 8 and 9 can be associated with reduced feeding success, reduced growth rates, reduced fish density and poor condition. These effects have been reported in field studies where exposures occur over multiple elevated SS events.

Table 1. Scale of Severity (SEV) index of ill effects associated with suspended sediment. From Newcombe and Jensen (1996).

SEV	Description of effect
	Nil effect
0	No behavioral effects
	Behavioral effects
1	Alarm reaction
2	Abandonment of cover
3	Avoidance response
	Sublethal effects
4	Short-term reduction in feeding rates; short term reduction in feeding success
5	Minor physiological stress; increase in rate of coughing; increased respiration rate
6	Moderate physiological stress
7	Moderate habitat degradation; impaired homing
8	Indications of major physiological stress; long-term reduction in feeding rate; long-term reduction in feeding success; poor condition

Lethal and paralethal effects	
9	Reduced growth rate; reduced fish density delayed hatching; reduced fish density
10	0-20% mortality; increased predation; moderate to severe habitat degradation
11	>20 – 40% mortality
12	>40 – 60% mortality
13	>60 – 80% mortality
14	>80 – 100% mortality

Third, Newcombe and Jensen (1996) state that exposure to elevated SS can cause fine particles of sediment to be taken into various tissues of a fish, like the spleen, leading to various ill effects on fish health. The process by which this happens is phagocytosis, where the envelopment of fine particles by cells of the fish's gill and gut transports the particles into the fish's body. The spleens of fish can become mineralized by this process. By its nature, this process is not one that occurs during single exposures, it occurs over multiple exposures to SS. The SEV scale is intended to help address this.

Fourth, and most importantly, the SEV author himself, Charles Newcombe, states that exposure should be calculated in the manner done in EDT. Larry Lestelle recently discussed this matter with him by telephone.[1] Dr. Newcombe stated that multiple exposures should be handled by summing the total exposure to a level of SS over some relevant period of time, such as a month, regardless of whether there are intervening periods of clear water. He stated that he applied this very method when he did his synthesis of the many studies. He stated that to not account for multiple exposures like this would be to <u>underestimate</u> the effect of SS. He expressed surprise that this would not readily be done by someone applying the SEV because of the types of studies included in his analysis, though he admitted that he has been asked about this on many occasions. He further stated that to calculate exposure duration in this matter and apply it in the SEV would provide a reasonable estimate of the <u>cumulative effect</u> of SS.

Dr. Newcombe further described how consideration of multiple exposures should take into account whether fish have sufficient time to heal or recover from exposure to an elevated SS event. Use of a month in the manner applied in EDT would be very appropriate, he said. Gill tissue damaged by elevated SS in one storm event would not have sufficient time to fully recover even when clear water follows if another storm occurs soon thereafter, he explained. In this case, the appropriate total exposure to apply in the SEV would be the number of hours exposed during the first event plus the number in the second event. If more events occur, then these durations of exposure would also be added.

The second point—that the rules that incorporate the SEV are inaccurately formulated—is apparently based on a view of Booth et al. that the rules <u>underestimate</u> the

[1] / Charles Newcombe is an Ecosystems Specialist with the Standards, Monitoring, and Reporting Section of the Ministry of Environment, Province of British Columbia. Phone number is 250 387-9552. Email address is Charles.Newcombe@gov.bc.ca

effects of suspended sediment. They give no evidence for this conclusion other than, it seems, their own authority. (It is puzzling to us that their conclusion that the EDT method will underestimate the effects of SS is given on the heels of their previous statement that the EDT approach to calculating the SEV would <u>overestimate</u> effects.)

They state: “Unfortunately, Figure 5-9 does not account for cumulative non-lethal effects on juvenile salmonids. Rather, the rules curve does not begin to seriously affect juvenile survival until SEV values directly kill juveniles, scaled as approximately 2.5 in EDT. We know this is not correct, based on numerous studies of deleterious, but non-lethal, effects of fine sediment on fish.”

We are uncertain how they arrived at their conclusion since they provided no substantiating evidence. We can only guess it is due to their lack of understanding as to how the rules operate and how EDT does, in fact, address cumulative effects. With regard to the effect of the rules that incorporate the SEV, they were formulated to line up with the SEV scale (Table 1) to the best of our understanding. That is not to say we achieved that objective perfectly—perhaps not—but it is an easy matter to refine the rules if a compelling argument is presented. We provide some examples here for the reader to compare the results of the rules to the SEV scale. The examples shown apply to juvenile subyearling coho. They show levels of mortality that would occur under various combinations of SS and water temperature (Table 2). Water temperature is incorporated into the rules as a modifier on the effect that suspended sediment has on survival. Newcombe and Jensen (1996) stated: “Ill effects are greater in seasonably warm water than would be the case for the same fishes in seasonably cold water.” (This temperature effect is not a direct temperature effect per se but rather how it acts to modify the effect of SS.)

Table 2. Scale of Severity (SEV) index of ill effects associated with suspended sediment. From Newcombe and Jensen (1996).

Attribute	Attribute rating															
Suspended sediment	0	1	1	1	1.5	1.5	1.5	2	2	2	2.5	2.5	2.5	3	3	3
Water temperature	0	0	2	3	0	2	3	0	2	3	0	2	3	0	2	3
Mortality (%)	0%	1%	2%	4%	3%	7%	10%	10%	13%	22%	22%	32%	41%	44%	58%	71%

As quoted above, Booth et al. had concluded that the effect of SS in the EDT rules does not result in mortality until a SS rating of approximately 2.5 is achieved. The examples clearly show that this is not the case.[2] When the SS rating reaches 2.5, mortality within the life stage has become very substantial, ranging between approximately 20-40%, depending on water temperature. This mortality is due entirely to SS (with temperature as a modifier), which would have a dramatic effect on overall life stage

[2] / To fully understand the examples shown, the reader needs to refer to EDT documentation to understand attribute ratings (on a 0-4 scale) and their definitions as applied to different attributes. It suffices to say here that an attribute of 0 is benign to fish survival while a rating of 4 generally results in a very significant effect on survival during a life stage. Hence mortality generally increases as an attribute’s rating goes from 0 to 4.

survival and performance over the life cycle considering other mortality sources. At lower SS ratings, mortality also occurs and it should not be dismissed as trivial. For example, at a rating of 1.5 mortality is projected to range between 3-10% depending on temperature. This range of effect, when combined with all sources of freshwater mortality, then combined with marine mortality, can have a very real consequence to performance over the entire life cycle.

These examples show that the SS ratings used in EDT align very closely with the SEV scale, illustrated in Table 3. It should be noted that we assumed that water temperature would modify the effect of SS so that at higher temperatures mortality would be at or near the top end of the range of mortality defined by the SEV scale. Low water temperatures would serve to keep mortalities at the low end of the ranges for SEV index values. In applying temperature in this way, the effects of SS produced by the rules remain consistent with the ranges of mortality for different SEV index values given by Newcombe and Jensen.[3]

Table 3. SEV index values and corresponding EDT ratings for suspended sediment (SS) ratings used in rule formulation.

<table>
<tr><th>SEV</th><th>Description of effect</th><th>EDT SS rating</th></tr>
<tr><td colspan="2">Nil effect</td><td rowspan="8">0</td></tr>
<tr><td>0</td><td>No behavioral effects</td></tr>
<tr><td colspan="2">Behavioral effects</td></tr>
<tr><td>1</td><td>Alarm reaction</td></tr>
<tr><td>2</td><td>Abandonment of cover</td></tr>
<tr><td>3</td><td>Avoidance response</td></tr>
<tr><td colspan="2">Sublethal effects</td></tr>
<tr><td>4</td><td>Short-term reduction in feeding rates; short term reduction in feeding success</td></tr>
<tr><td>5</td><td>Minor physiological stress; increase in rate of coughing;</td><td rowspan="3">1</td></tr>
<tr><td>6</td><td>Moderate physiological stress</td></tr>
<tr><td>7</td><td>Impaired homing</td></tr>
<tr><td>8</td><td>Indications of major physiological stress; long-term reduction in feeding rate; long-term reduction in feeding success; poor condition</td><td rowspan="4">2</td></tr>
<tr><td colspan="2">Lethal and paralethal effects</td></tr>
<tr><td>9</td><td>Reduced growth rate; reduced fish density</td></tr>
<tr><td>10</td><td>0-20% mortality; increased predation</td></tr>
<tr><td>11</td><td>>20 – 40% mortality</td><td rowspan="2">3</td></tr>
<tr><td>12</td><td>>40 – 60% mortality</td></tr>
<tr><td>13</td><td>>60 – 80% mortality</td><td rowspan="2">4</td></tr>
<tr><td>14</td><td>>80 – 100% mortality</td></tr>
</table>

[3] / This short discussion on the rules is by necessity here much abbreviated. Interested readers are encouraged to review the primary documents that describe the rule structure and its application to gain a more complete understanding.

The matter of cumulative effects within EDT is a related topic that Booth et al. misinterpret. One of the strengths of EDT, in fact, is to provide a better understanding of cumulative effects. We are unaware of any other model that addresses this matter as well as EDT. One small example of this is seen in Table 2, where water temperature operates synergistically to affect the potential overall impact of SS. This is a cumulative effect. Further, the cumulative effect of sediment is captured in the rules through the different ways that fine sediment can affect survival in each freshwater life stage. The rules provide a way of addressing the effects of sediment entrained within the substrate, overlaid on top of the substrate, and as shown in Table 2, suspended in the water column. Effects of these various aspects of fine sediment impacts vary by life stage—collectively, their effects on population performance are cumulative.

Perhaps the greatest strength of EDT in addressing cumulative effects is how it deals with the mosaic of environmental conditions (including sediment condition)—both spatially and temporally—within a river system. The EDT model uses simulated life history pathways that reflect how a fish population is exposed to various environmental effects in all of its life stages and across their geographic distribution within a watershed. It captures the temporal variation of how some attributes, like SS and temperature, vary seasonally. The result is that the cumulative effects are incorporated in a major way through EDT. We are baffled by Booth et al.'s assertion that EDT is poorly equipped to address cumulative effects.

2. Accuracy of model results and validation

Booth et al. are concerned that use of the EDT model produces unrealistic and inflated salmon population estimates under the various scenarios. They refer to "extraordinarily (i.e., implausibly) high fish performance predictions." This, it appears, is a major reason for them to be so severely critical of the model's use. For this reason, closer examination of their criticism is warranted.

Booth et al. believe that the model's estimate of 1,500 adult coho as the historic spawner abundance in Church Creek is implausible. For comparison, they state that a pristine watershed of similar size in Northern California might sustain 300-500 adults. They go on to state that a number like 300-500 would be more than enough spawners to saturate the pristine rearing habitat of Church Creek, hence it would be unreasonable to believe that more than this number of adults could be produced. They also attempt to make some simple extrapolations of survival and adults produced to show how unrealistic are the model's estimates.

We find their argument to be confused and to demonstrate a profound lack of understanding about important issues that affect the model's output. These include coho population dynamics, coho habitat potential, coho life history, application of the Beverton-Holt stock-production relationship, inputs to the EDT model, and how Church Creek was characterized for the purpose of modeling.

Is an estimate of 1,500 adult coho unrealistic for the historic period?[4] We are uncertain why Booth et al. draw a comparison to an unknown stream in Northern California, which has little relevance to the question. Far more appropriate comparisons can be made using data collected in the Puget Sound region, where life histories, climatic effects, and marine survival will be similar. One useful comparison can be made to Snow Creek, a stream that flows into Discovery Bay southeast of Sequim. The Washington Department of Fish and Wildlife has maintained a salmon monitoring site on the stream since the mid 1970s. By the time monitoring began in this stream, the watershed's forest had already been harvested and extensive changes had occurred to the stream's habitat. The stream was far from being pristine in the mid 1970s. Still, data collected there are useful for our comparison.

We compare empirical data collected in Snow Creek to modeling input and output applied in the Church Creek analysis. This allows us to simply examine how reasonable are outputs from the EDT model given a certain set of inputs; we make no judgment on how closely those inputs actually match conditions in Church Creek since that is beyond the scope of our comments.

Table 4 compares habitat areas and key habitats between Snow and Church creeks. The data for Snow Creek is taken from WDW (undated) and Johnson and Cooper (1991). The streams are quite comparable in size. Notably, Snow Creek has a much lower percentage of pool habitat than was used in the Church Creek analysis. Because juvenile coho rely so heavily on pool habitat for both summer and winter rearing, we are most interested in how much of this habitat exists in both streams. Snow Creek contains an average of about 17,000 m^2 of pool habitat at summer low flow while the historic Church Creek (model input) had slightly more, about 18,500 m^2. It is well known that carrying capacity of typical Western Washington coho streams is largely driven by the amount of summer and winter rearing habitat (i.e., pool habitat) and not by spawning and egg incubation habitat (Sandercock 1991; Quinn 2005).

Table 4. Quantity of stream habitat in Church and Snow creeks. Data for Church Cr. are from model input. Data for Snow Creek are from WDW (undated) and Johnson and Cooper (1991).

Habitat quantity	Church Cr	Snow Cr
Total stream area m^2	41,939	56,806
Percent pools	44%	30%
Pool area m^2	18,463	17,041

[4] / Whenever we speak of an estimate of a population size as generated by the model, the reader should keep in mind that this estimate represents a modeling output generated with static input. The estimate is meant to be an approximation of the average abundance that would occur over a period of years during a single regime of climate and ocean conditions. It is recognized that year-to-year variation in actual abundance could be quite high. The modeling estimate is intended to represent an average value over some period of years.

Table 5 compares empirical data on fish abundance in Snow Creek to model output for Church Creek for its historic condition. The table also provides an estimate of smolt carrying capacity for Snow Creek based on an analysis of Ricker and Beverton-Holt stock-production relationships for that stream. This allows for a direct comparison to be made to the output for Church Creek, which is also based on parameter estimates for a stock-production relationship. The data summary and corresponding analysis for Snow Creek are found in Lestelle et al. (1993a). It is important to recognize that the smolt capacity estimate for Snow Creek is based on an analysis of that stream's observed smolt counts, while the smolt capacity for Church Creek is based on an analysis of that stream's habitat using the EDT model—not on actual smolt counts for that stream.

Table 5. Comparison of observed and estimated production levels of coho salmon in Snow Creek and Church Creek. See text.

Snow Cr	
Ave smolt yield (1976-1984)	7,274
Ave spawners (1976-1987)	542
Total adults at 60% harv rate	1,355
Smolt capacity:	
- Ricker equation	8,300
- B-H equation	9,700
Church Cr (modeled)	
Smolt capacity	8,900
Ave smolt yield	8,600
Ave spawners (no harvest)	1,440

The average observed smolt yield for Snow Creek is based on the years where production was fairly stable (brood years 1976-1984)—there was a sharp drop-off in subsequent years and these were excluded (reasons for change in production are discussed in Lestelle et al. 1993a). The average observed spawner escapement (up to and using the last year of smolts included in the smolt average) for the stream was about 540 fish. Applying a conservative estimate of the total harvest rate of 60% in these years would equate to an average number of adult recruits of about 1,400 fish.

The comparison in Table 5 demonstrates that the model's estimates for Church Creek are reasonable. However, there are two factors worth mentioning that would tend to favor a higher rate of production in Church Creek than in Snow Creek. Smolts produced in Snow Creek likely have a slightly lower marine survival than those produced in streams of the main body of Puget Sound (like the Stillaguamish River). Streams that enter directly into the Strait of Juan de Fuca or near there (such as Snow Creek) appear to have somewhat lower marine survival (e.g., see Lestelle et al. 1993a). The second factor to note is that coho juveniles produced in Church Creek would benefit by the presence of additional rearing habitat (particularly during winter) located downstream in the lower most reaches of the Stillaguamish River. This would tend to add capacity to the Church Creek population. In contrast, Snow Creek fish have no comparable habitat downstream because the stream enters directly into Discovery Bay. It is well known that coho populations can benefit significantly by having spawning streams that enter into larger mainstem rivers that provide opportunities for off-channel pond and slough rearing (Peterson and Reid 1984; Lestelle et al. 1993b). This aspect of coho life history is captured in the EDT model.

This comparison of the pristine condition in Church Creek based on modeling to empirical data for Snow Creek in a somewhat degraded condition suggests therefore that the Church Creek abundance estimate might be conservative. The estimate is certainly consistent with what a stream this size is capable of producing within the Puget Sound region.

Why do Booth et al. draw an erroneous conclusion on this matter? It appears to be due to several reasons. They state that there are only three miles (page 8) of habitat in Church Creek, when both in reality and in the modeling there is much more. They state that a 3% marine survival is applied, when in fact modeling for the historic condition was done using a value closer to 16%. Puget Sound coho survival varies widely depending on climate and ocean regimes. During periods of favorable ocean conditions, such as existed in the 1970s and 1980s, marine survival averages between 16-20%. (Survival for Snow Creek fish appears to be somewhat less.) They misapplied benchmark values from the EDT model to some simple computations they made to illustrate juvenile production potential. And finally, they failed to recognize the value of habitat within the mainstem Stillaguamish River located downstream of Church Creek.

Attachment 2: Partial Bibliography and Reference List for Ecosystem Diagnosis and Treatment

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