

## Response to Blair et al.

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### Overview

We appreciate the interest that our review of the Shared Strategy document, “Final Report – Instream Flow Assessment Pilot Project” has generated. The region has long recognized the importance of a science-based approach to salmon conservation, but efforts to move in that direction have been isolated at best. Although studies, reports, and memos that reach very different conclusions may seem like an inefficient way to move forward, that is exactly the process by which scientific enquiry makes sustained progress. We consider the Blair et al. commentary, “Comments on the “Peer Review...”” in this light and hope that it will continue to stimulate thoughtful discussion on this topic of great importance to the region.

Our review of the Instream Flow Assessment Pilot Project was exactly that—a review of the pilot project. Our review focused so strongly on EDT because that is what the pilot project emphasized, almost to the exclusion of other approaches. As the primary tool that was used to predict the biological impact of flow alteration, understanding the functioning of this model was essential for evaluating the pilot project and its potential applicability elsewhere in the region. Although there is a need for an evaluation of EDT, we did not conduct a complete review of the model, and we specifically stated “A complete peer review of EDT is outside the scope of our review, just as it was outside the scope of the Draft Report.”

It is important to note that we did not say EDT should never be used. Rather, we said that it should not be used for the application in the pilot study, at least until the model had been thoroughly reviewed (and, by implication, until those issues raised in such a review have been addressed). This remains our recommendation; as such, it is hardly a radical suggestion.

Blair et al. stated, “Partisan model bashing serves little purpose...” We agree with the statement but disagree with the implicitly articulated belief that our review is “partisan model bashing.” Blair et al. offer no evidence of partisanship and we have no alternative model that we champion. A negative review is not necessarily “bashing,” and we have no sense of that here.

Blair et al. also stated, “...any constructive review must compare EDT to other available alternative methods for accomplishing this task.” Again, our “review” was of the methodology of the pilot project, not of EDT. Model comparison *per se* was not the task of the review team, nor was using a fish-population-prediction model the “task” of the pilot study. Rather, the pilot study was charged with providing management

recommendations with regard to flow modifications as a result of urban development, and we were asked to evaluate the science in the pilot study and comment on whether the methods in the study should be applied elsewhere. Indeed, a thorough comparison of alternative methods might have been an appropriate task for the pilot study itself (this was noted indirectly in our review). We also did provide a number of “next steps” that we *do* consider effective, feasible, and defensible—that is our working definition of “constructive,” and that they do not include further application of EDT (or any other such model) at this time does not lessen their credibility.

### **Peer Review**

Blair et al. stated, “We submit that EDT is the most documented, the most thoroughly reviewed and the most commonly understood tool used in salmon recovery planning in the Pacific Northwest” [original emphasis]. However, Blair et al. do not cite a single review of the model. In support of their claim that EDT has been widely reviewed, Blair et al. noted (as did our review) that EDT has been widely used. However, “users” of a model, by definition, can NOT provide an *independent* review of that model. In peer review, one can not review one’s own work. Any sample of model users will not include those who may have considered using the model but rejected it for some reason, and so the population of EDT users is an inherently biased (i.e. non-representative) subset of scientific opinion. A good peer review should include an unbiased sampling of qualified scientific opinion from those with no stake in the review results. Shared Strategy should be credited with conducting a peer review of at least an *application* of EDT, even if not of the model itself; the present discussion serves (if nothing else) to demonstrate just how unusual such reviews have been.

Blair et al. does cite one validation report (Rawding 2004). This report, which was neither peer reviewed nor a review of EDT, explored only one aspect of the model and never provided (nor intended to provide) sufficient information to conclude whether the application of EDT in the pilot study was appropriate.

Blair et al. cited ongoing NMFS work on sensitivity analysis of the EDT model as constituting a review of EDT. One of the authors of the pilot project review (Paul McElhany) is a principle investigator on the NMFS sensitivity analysis project (along with co-PI Ashley Steel). The NMFS sensitivity analysis project, however, has not produced an independent peer review of EDT and has no plans to produce such a peer review. Of course, the results of the sensitivity analysis would be useful information for consideration by an independent peer review team. None of these activities, however, constitute a “thorough review” of the model, and we remind the region’s salmon managers that rarely have any fish-habitat models undergone such scrutiny.

Blair et al. stated that EDT is “...the most commonly understood tool...”. Given that documentation of the mathematical algorithms in the model was not even available until very recently, it seems that the depth of that common understanding would be highly

variable and often limited. This is probably true of other such models, also, but that does not invalidate our concern for this particular application.

We stand firmly by our conclusions regarding peer review – “A complete peer review of the EDT model would be essential to evaluate the quality of this study. We strongly recommend against further application of this model until such a peer review has been completed. EDT may or may not provide useful, credible outputs, but it does not pass even the lowest standards for a scientific peer-reviewed framework, and it does not meet the standards for ‘Best Available Science.’” Our recommendation was (and remains) that until these shortcomings are rectified, alternative means should be explored to accomplish management objectives in watersheds like those of the pilot study.