



## Independent Scientific Review Panel

for the Northwest Power & Conservation Council  
851 SW 6<sup>th</sup> Avenue, Suite 1100  
Portland, Oregon 97204  
[www.nwcouncil.org/fw/isrp](http://www.nwcouncil.org/fw/isrp)

**Memorandum (ISRP 2015-1)**

**January 28, 2015**

**To:** Phil Rockefeller, Chair, Northwest Power and Conservation Council

**From:** Greg Ruggerone, ISRP Chair

**Subject:** Review of a Revised Proposal for the Yakama Nation's Accord project, Upper Columbia Nutrient Supplementation (#2008-471-00)

### Background

In response to the Council's December 4, 2014 request, the ISRP reviewed a revised proposal for the Yakama Nations' [Upper Columbia Nutrient Supplementation Project](#) (#2008-471-00). The revised proposal is intended to address a condition placed on this project as part of the Council's decision made on the project on May 12, 2010 and a follow-up review by the ISRP in February 2014 ([ISRP 2014-2](#)). In addition to the 2014 ISRP review, the ISRP has reviewed the project three other times ([ISRP 2009-27](#), [2009-50](#), and [2010-8](#)). In the 2014 review, the ISRP found that the project proponents' submittal did not meet scientific review criteria and recommended that any future proposal be presented as a stand-alone study proposal that addresses the ISRP's comments, especially those related to experimental design and statistical analysis.

In response to the last ISRP review, the Yakama Nation developed and submitted the following documents:

- Cover letter (6 pages)
- The previous ISRP review (ISRP 2014-2) augmented by the project sponsor with point-by-point responses to the ISRP's questions and concerns (53 pages)
- Updated proposal narrative - titled *Upper Columbia Natural Production Restoration Project* (81 pages)

Project short description:

*This project evaluates the effects of nutrient addition on natural production of anadromous salmonids in Hancock Spring Natural Laboratory in the Methow River Basin. The project applies a food web approach to quantify energy flow through food webs, using both Trophic Basis of Production modeling and stable isotope analyses. The project also assesses the effects of habitat complexity on efforts to restore natural production. To scale findings, the project formalizes*

*ecological mechanisms of energy routing in a mechanism-based model for understanding restoration effects on ESA-listed fish species.*

The ISRP's review below is organized around the proponents' proposal outline and response to the previous ISRP review.

## **ISRP Recommendation**

### **Does not meet scientific review criteria**

Some of the concerns raised in the previous ISRP review were addressed in the revised proposal and in the point-by-point responses provided by the project proponents. For example, limiting the experimental treatment to analog additions reduced the issue of confounding effects from multiple types of treatments associated with the previous design.

However, the revised proposal still has a number of major flaws:

- 1) The nutrient addition study will still be unreplicated, lacks a non-enrichment control, and suffers from a lack of independence between the restored and unrestored reaches. Reach 2 is immediately downstream of Reach 1. Any activities occurring in Reach 1 could impact conditions in Reach 2, including the addition of analogs to Reach 1 (i.e., Reach 2 will receive enrichment from both the analogs placed in this reach and any organic matter and nutrients transported from Reach 1). Therefore, it would not be possible to attribute any difference in response to enrichment with analogs between Reach 1 and Reach 2 to difference in physical habitat condition. The lack of an unenriched reference site will affect the ability to ascribe observed responses to the treatment rather than temporal effects. A before-after control-impact (BACI) design is needed to disentangle treatment and temporal effects. The lack of replication implies that any conclusions would be specific to this particular location and may not be applicable to other locations.
- 2) The two study reaches appear to be physically very different, even before Reach 1 was restored (see comments below). This further increases concern about the validity of attributing any differences in food web responses to the habitat restoration actions that were undertaken before enrichment began.
- 3) Hancock Springs is an unusual site at the landscape scale, as the proponents acknowledge in the proposal. Therefore, concerns remain about the applicability of the information generated at Hancock Springs to other systems in the Upper Columbia region. Using the Hancock Springs data to parameterize ecosystem models may aid, somewhat, in understanding the extent to which the responses observed at Hancock Springs can be applied to other sites. However, the application of models parameterized with data from a spring stream to systems with the variable flow and thermal regimes typical of non-spring streams will be highly uncertain. It is likely that knowledge gained from this project will be of limited value in improving restoration actions on other small tributaries.

Although a better understanding of food web response to various restoration actions remains a key knowledge gap across the Columbia Basin, the issues listed above indicate that Hancock Springs is not an appropriate location to conduct this type of research. Overall, the proponents have not provided a convincing argument that this project will provide new information that can be used by other researchers or managers.

## **ISRP Review Comments on the Revised Proposal**

Comments are provided below on the specific elements of the proposal and the point-by-point response to the previous ISRP review.

### **Problem statement**

The issue to be addressed by this study is briefly described at the beginning of the proposal. However, some of the material identified issues that could not be addressed through the proposed research. The proponents state that *“The central problem being addressed by this project is the ongoing low level of natural production for ESA-listed fish species in the Upper Columbia basin, despite numerous past and ongoing restoration actions.”* Determining the cause of low levels of natural production cannot be determined from just one site and only looking at food webs. Low levels of natural production by listed fish species may be due to numerous factors unrelated to food web dynamics including overfishing, toxics, genetic introgression from hatchery fish, and other problems. The proponents also state that *“Although significant measures have been implemented to reverse this trend, anadromous returns have not significantly increased into the region (NPCC 2009).”* This statement seems a bit out of date as adult returns of salmonids (especially steelhead) to the Methow River appear to have increased significantly since 2010, based on a recent IMW annual report ([www.usbr.gov/pn/fcrps/rme/methowimw/MethowIMW032013.pdf](http://www.usbr.gov/pn/fcrps/rme/methowimw/MethowIMW032013.pdf)). The data on recent returns should have been used in developing the problem statement.

Finally, the proponents state that *“Low aquatic productivity caused by reduced MDN is a limiting factor for rearing salmon ....”* This is an overly broad statement and has not been proven to be the case everywhere in the Columbia Basin. For spring streams, in particular, this assumption may be incorrect as such systems tend to be (relatively) nutrient enriched compared to other small streams.

### **Information and knowledge gaps**

The proposal states on page 11, *“The information gathered from the proposed research will allow scientists to address multiple knowledge gaps related to the ecological response of nutrient addition across background biotic and abiotic variability. Specifically, the project will: (1) quantify the complex food web response to nutrient addition across two stream reaches (restored/complex and degraded/simplified).”* Although this statement indicates the project will address multiple knowledge gaps, only this one gap is noted. A more complete discussion of the key knowledge gaps, relevant to system response to nutrient addition, would have enhanced the rationale for this project.

### **Understanding food web response to restoration**

The review of the literature related to stream food web response to the addition of salmon carcasses, carcass analogs, or inorganic nutrients is relatively complete. Table 1 provides a useful summation of some of this research. However, this section fails to incorporate some of the more recent elements of food web theory (see McCann 2011, *Food Webs*, Princeton Univ. Press). For example, what makes food webs stable? What makes them productive for some species and not for others? The proponents are correct in pointing out that food web response to various restoration actions has received relatively limited scientific attention in the Columbia River Basin and elsewhere – citing Cross et al. (2013) as support for this statement. Notably, the Cross et al. (2013) study included a relatively comprehensive examination of basic ecological properties of food webs, including food web stability. Incorporating these types of fundamental principles about food web organization into this study could aid in the transfer of study results to other situations.

Another aspect of previous research in this area that was not fully discussed in this section is the role that light availability plays in determining the effect of nutrient addition on bottom-up enhancement of trophic productivity. The fact that Hancock Springs is largely open to direct sunlight along much of its length indicates that light is not likely to limit autotrophic production. However, there are many streams in forested areas of the Columbia Basin where light will play a role in limiting autotrophic production. Some discussion of the role that light plays in food web dynamics should have been included in this section as light availability will be an important factor in the extension of any results from Hancock Springs to other sites. Several publications listed below evaluate this issue and provide information that may be relevant to the Hancock Springs study.

Additional information is required for Figure 1. It is unclear whether the values provided next to each fish species in this figure refer to the standing stock or the % of the total energy consumed by each species. A more comprehensive legend is needed in order to fully understand this figure. In addition, the key citation for Figure 1 (Jorgensen et al. 2013) is not in the references.

The Bellmore et al. (in prep) paper, which describes the ATP model, is central to the research being proposed as it is the primary tool that would be used to analyze the collected data and extend results to other systems. Unfortunately, the article is not published and a draft or summary of this paper was not provided with the proposal package. It was not possible to fully evaluate the applicability of the model to this project without a better understanding of the model structure.

*Ambrose, H.E., M.A. Wilzbach, and K.W. Cummins. 2004. Periphyton response to increased light and salmon carcass introduction in northern California streams. Journal of the North American Benthological Society 23: 701–712.*

*Gregory, S.V. 1980. Effects of light, nutrients, and grazers on periphyton communities in streams. PhD Dissertation, Oregon State University, Corvallis.*

*McCann K.S. and Rooney N. 2009. The more food webs change, the more they stay the same. Philosophical Transactions of the Royal Society B 364: 1789-1801.*

*McCann K.S. 2011. Food Webs. Princeton University Press, Princeton, New Jersey.*

*Wilzbach, M.A., B.C. Harvey, J.L. White and R.J. Nakamoto. 2005. Effects of riparian canopy opening and carcass addition on the abundance and growth of resident salmonids. Canadian Journal of Fisheries and Aquatic Sciences 62: 58–67.*

### **Technical background**

The technical background provided in the proposal is generally sufficient although some citations for several key papers were not provided and some information seemed a bit out of date. For example, salmon and steelhead escapement values provided in this section are pre-2010. As noted earlier in these comments, abundance of adult salmon and steelhead in the Methow River has increased significantly since 2010 (IMW Annual Report; <http://www.usbr.gov/pn/fcrps/rme/methowimw/MethowIMW032013.pdf>). The more recent escapement information should have been included in this section of the proposal.

### **Project Information - significance to regional programs and relation to other projects**

One of the ISRP's major concerns with this proposal is the non-representative nature of the Hancock Springs site. The significance of this research to recovery actions identified in various regional plans would be better represented if there were a more complete understanding of how common spring creeks are in the Upper Columbia region. The point-by-point response to the previous ISRP review did indicate that such a survey could be conducted in the future but is not planned as a component of this study. It is true that the improved understanding of food web response to trophic enhancement is of general value across the Columbia Basin. But the relevance of the results from Hancock Springs would be largely limited to sites that share similar characteristics. Use of models to extend Hancock Springs results to other systems may be helpful in this regard. But given the unusual features of spring creeks, there will be a high level of uncertainty with the application of model results to systems other than spring creeks.

The proposal lists six BiOp attributes that are addressed by this project:

- 1) Tributary habitat restoration, including new information on evaluating and prioritizing projects to achieve survival and other benefits
- 2) Improve juvenile and adult survival by protecting and enhancing habitat
- 3) Implement habitat projects that address limiting factors
- 4) Protect or restore riparian buffers, particularly those that function as thermal refugia
- 5) Remove barriers to fish passage into thermal refugia
- 6) Identify cool-water refugia
- 7) Protect groundwater systems and restore them where possible

However, it is not clear how the proposed work at Hancock Springs will address items 4 through 6. No component of this study will address riparian buffers nor is this study planning to assess responses to barrier removal. Although the cool water in Hancock Springs does offer thermal refuge, this study is not identifying thermal refugia. Further explanation is necessary.

The proposal indicates that it will address certain elements from the Fish and Wildlife Program's 2000 objectives (Section 10.C.3. "Objectives of the 2000 Fish and Wildlife Program"). Given that the Program has been updated numerous times in the last 15 years, it is surprising that this version of the Program is cited here. It would be more appropriate to use the 2014

objectives (or even the 2009 objectives) of the Program rather than the 2000 objectives. Was there a reason why the 2000 objectives were cited in this section?

The proposal states on page 26 that this project will address three of the Northwest Power and Conservation Council's 2003 Mainstem Amendment plan objectives, including the following: *"the protection and enhancement of mainstem habitat, including spawning, rearing, resting and migration areas for salmon and steelhead and resident salmonids and other fish."* It is unclear how the Hancock Springs research will provide any information relevant to *"the protection and enhancement of mainstem habitat."* If successful, this research could provide a better understanding of food web response to nutrient enhancement in small spring creeks in the Upper Columbia region, but the extension of these results to mainstem habitats would be highly suspect.

The proposal indicates that linkages with researchers involved in other nutrient addition projects have been established. However, how knowledge gained at these other projects has been incorporated into the study at Hancock Springs was not described. For example, in what ways has the Kootenai River fertilization project informed the strategies of this project? In what ways have Hancock Springs personnel been involved with the BC fertilization projects? More information should have been provided about the linkages between the Hancock Springs study and nutrient enhancement research being conducted elsewhere.

Insufficient detail was provided about the relationship between the Hancock Springs study and other efforts assessing system response to nutrient enhancement occurring in the Upper Columbia region. Briefly mentioned in the proposal were the Upper Columbia Nutrient Enhancement Project (BPA 200847100) and a WSU food web analysis project. If USGS, BOR, and WSU have a comprehensive investigation of food web dynamics ongoing throughout the area, why is the Hancock Springs study necessary? What will it contribute to the larger investigation? Given that these projects are occurring in the same area and with some of the same researchers as those associated with the Hancock Springs study, a description of how these efforts fit together would have been useful.

### **Study area and design**

As noted above, the ISRP believes that the design of the proposed study has serious deficiencies that will be difficult to address. The proposed experiment will be conducted without an appropriate reference site. The two study reaches cannot be considered independent as any activity that occurs in Reach 1 has the potential to impact conditions in Reach 2.

The unusual nature of Hancock Springs also remains a concern. It is imperative that the proponents conduct an extensive analysis of the Upper Columbia Province to identify the number of spring creek sites and their potential for restoration before embarking on a long-term and detailed investigation of food web dynamics in a single spring-stream. In essence, the proponents are putting the "cart before the horse" by conducting detailed studies before they know the spatial scope of the issue.

The fact that no habitat data are available for the two study reaches prior to the application of the restoration treatment on Reach 1 also presents a problem. The information provided in the

proposal seems to indicate that Reaches 1 and 2 may have been dissimilar prior to the application of the habitat restoration on Reach 1. This fact is suggested by some unexpected differences in habitat parameters between Reach 1 (restored) and Reach 2 (unrestored). Reach 1 has a much lower proportion of cobble and gravel substrate than Reach 2, suggesting that Reach 1 has a much higher proportion of the bed covered by fine sediment. This condition would seem to indicate higher quality substrate for salmonids in the unrestored reach. In addition, large wood is more than four times as abundant in Reach 2 than Reach 1, despite the fact that one of the restoration actions on Reach 1 was the addition of large wood to the channel. It also appears that Hancock Springs gains considerable flow as it moves through the study area. Therefore, Reach 2 will experience consistently higher flow than Reach 1. Finally, Figures 3 and 5 indicate that the riparian vegetation along Reach 2 appears to be much denser and taller than that along Reach 1. These types of inherent differences in site characteristics between the study reaches would make it very difficult to conclude anything about the interaction between nutrient enhancement and restoration of physical habitat.

There was some inconsistency in the description of the frequency of sampling in the text. Page 35 indicates that 8 observations will be made annually for 3 years; a total of 24. However, the previous section (Page 33) indicates 6 observations per year for 6 years (3 pre-treatment and 3 post-treatment) for a total of 36. This discrepancy is clarified in Table 6, which indicates that invertebrates will be sampled 8 times annually and the other parameters 6 times. The text should be altered to accurately reflect the sampling frequency.

### **Methods and reporting**

Although the ISRP believes the problems with the fundamental design of the proposed Hancock Springs study are very serious, many of the sampling methods proposed for the study are appropriate. The comments and suggestions relative to the proposed sampling methods provided below may be of value in the development of any future study proposal for examining the effectiveness of nutrient enhancement.

#### Carcass Analog Applications:

The rationale for selecting a carcass analog loading rate might benefit from some additional scrutiny. The paragraph describing the determination of appropriate loading rate (page 35) includes some citation errors and implicitly makes the assumption that salmon carcasses and carcass analogs are the same thing. This section states that Bilby et al. (1996) applied analogs at the rate of 0.03 g/m<sup>2</sup>. This paper did not describe a nutrient enhancement experiment but simply quantified the level of MDN in various components of a stream where large numbers of coho salmon spawned. Bilby et al. (1998) did describe a carcass addition experiment (but not with carcass analogs) in which the application rate was around 0.5 g/m<sup>2</sup>, not 0.03 g/m<sup>2</sup>. The discussion also notes that Wipfli et al. (2003) suggest that streams are in a state of nutrient deprivation if carcass loading rates are below 1.9 kg/m<sup>2</sup>. Given this, it is surprising that a loading rate of 0.15 g/m<sup>2</sup> is selected. The WDFW Stream Habitat Guidelines is cited as the rationale for selecting this loading value. But the fact that this suggested loading rate differs so drastically from that proposed in Wipfli et al. (2003) should cause some concern. Another factor to consider is that these suggested loading rates are for carcasses, not analogs. Nutrients and organic matter provided by 0.15 g/m<sup>2</sup> salmon carcasses and 0.15 g/m<sup>2</sup> analogs are different. Analog have lower water content and differ in nutrient concentration from natural carcasses.

Assuming these materials are equivalent is incorrect. The project proponents also might consider selecting an application rate well above those proposed in the literature. The project objectives cannot be met unless the system responds to the nutrient addition. Applying a sufficiently intense treatment to ensure system response is an approach that has been suggested by Carpenter et al. (1995) for ecosystem-scale experiments. However, a more thorough consideration of the treatment loading rate, concentrating on available information from studies that have used analogs, is required.

An additional problem arises as a result of the differences in channel condition between Reach 1 and 2. The channel in Reach 1 was narrowed considerably as a result of the habitat restoration treatment, considerably reducing wetted surface area relative to Reach 2. If carcass analogs are to be added at 0.15 kg/m<sup>2</sup> rather than on a water volume basis, it could substantially alter the intensity of the treatment between the two reaches; the wide, shallow channel of Reach 2 would receive considerably more analog material per unit of volume than Reach 1. It would be worthwhile to consider using water volume, rather than wetted surface area, in determining reach-specific application rates. Some baseline information on background nutrient concentrations also would be useful in establishing loading rate; no water chemistry information was provided in the proposal.

Finally, the time of year when the analogs will be placed in the stream was not mentioned in the proposal. If the analogs are to be added at a time consistent with Chinook salmon spawning (September-November) it seems that considerable information could be lost because the measurement season is noted as extending from March to October. If analogs are to be added during the autumn, some sampling through the winter would be imperative to capture potential water chemistry and food web responses.

*Carpenter, S. R., S. W. Chisholm, C. J. Krebs, D. W. Schindler, and R. F. Wright. 1995. Ecosystem experiments. Science 269:324-327.*

#### Periphyton

It is not clear how the periphyton taxonomic evaluation contributes to the objectives of the study. What does algal diversity have to do with standing stock or production?

The reliance on cobble-scrubbing to assess algal biomass may not fully capture this parameter. There may be considerable periphyton biomass associated with organic matter (e.g., FPOM, CPOM, wood) and with sediments. Therefore, biomass per unit area on cobble surfaces cannot be reliably expanded to an estimate of algal biomass per unit area of wetted channel. A representative cross section of substrates would need to be sampled to generate an accurate estimate of algal biomass. Further, the methods do not explain how autotrophic biomass will be distinguished from detrital mass.

#### Invertebrates

A major deficiency in the invertebrate sampling scheme is the failure to include an estimate of invertebrate drift. Drift is utilized heavily by feeding salmonids and may be the primary source of food in many systems. Therefore, drift may give a much better indication of invertebrates



available to the fish than benthic samples. Ideally, both benthic invertebrate production and biomass in the drift would be measured.

Benthic invertebrate samples will be collected using a Hess sampler with a mesh size of 1000  $\mu\text{m}$ . This mesh size is relatively coarse and may lead to underrepresentation of smaller invertebrates. A substantial proportion of the diet of juvenile fish, in particular, may consist of organisms smaller than 1000  $\mu\text{m}$ . For this reason, mesh sizes of 500 or 250  $\mu\text{m}$  are typically employed in this type of sampling. It might be worthwhile to consider using a finer mesh on the sampler.

### Fish Community

It is not clear how (or if) fish movement between Reaches 1 and 2 will be measured. Movement of fish between the study reaches and between Hancock Springs and the Methow River will affect estimates of biomass, growth, and production. Figure 5 indicates that PIT tag arrays are located at the bottom of Reaches 1 and 2, but there is no discussion in the proposal about tagging fish or how the PIT tag data will be used to help address project objectives. The tagged fish would be valuable in quantifying both individual growth rate and emigration rate of fish. If the PIT tag arrays are intended to help address the effects of fish movement on growth, production, and biomass, the procedure by which this would be accomplished should have been described.

The section describing the methods for collecting gut contents (page 39) states "*Gut contents will be collected from the five dominant species, with samples distributed haphazardly among the four size classes.*" Fish size can have a considerable impact on the food items they ingest. If there are differences among study reaches, or among study dates, in the size of fish included in the gut content analysis differences in diet due to fish size could be interpreted as a treatment response. The influence of fish size on diet should be accounted for in analysis of the gut content data. In addition, fish size classes also should be used in generating estimates of growth rate.

### Organic Matter Dynamics

The methods proposed to assess organic matter inputs and processing in the proposal are incomplete. A major deficiency is the failure to consider dissolved (DOM) and fine particulate (FPOM) organic matter. In terms of food web dynamics, these types of organic matter can be much more important than CPOM (and possibly periphyton) and cannot be overlooked. FPOM and DOM delivered to stream channels from phreatic groundwater can constitute a major source of organic matter input to streams, especially springs (for example, see Naiman et al. 1987).

It is not clear how the artificial leaf and stick transport experiments will be used in assessment of food web dynamics. The proponents correctly point out that "*A stream's capacity for efficient and effective use of inputs can be determined by measuring its capacity for retention.*" However, the dowel and water-proof paper pieces used in the proposed retention experiments will only provide a rough indication of system retentiveness for leaves and small pieces of wood. These experiments will provide no indication of DOM or FPOM retention. Additionally,

the dowels and paper will behave nothing like the carcass analogs that will be used in nutrient augmentation and will, therefore, provide no information on the retention of the analogs.

The statement made in the point-by-point reply to the last ISRP review of this project stated that “*Quantifying absolute levels of allochthonous inputs to a stream is a non-trivial undertaking that would require prohibitive costs, in terms of sampling material resources, human resources, field sampling effort, laboratory equipment and analyses, and biometric computations. For these and other logistical reasons, this has not been done in any system at the time of writing (Lamberti and Gregory 2007; Hart et al. 2013).*” In fact, quantifying allochthonous organic matter delivery to streams is not nearly as labor intensive or costly as some of the other methods proposed for this study. There are numerous studies on this topic that illustrate this point (for example, see citations listed below).

An additional useful measure for characterizing organic matter processing and transport in streams is turnover length (see Newbold, Minshall, Naiman articles). This measure provides an indication of system retentiveness that should be more closely related to overall system food web dynamics than the CPOM retention information provided by dowel and paper experiments. This parameter should be considered for this study.

- Bilby, R.E. and P.A. Bisson. 1992. Allochthonous versus autochthonous organic matter contribution to the trophic support of fish populations in clear-cut and old-growth forested streams. Canadian Journal of Fisheries and Aquatic Sciences 49:540-551.*
- Fisher, S.G. and G.E. Likens. 1973. Energy flow in Bear Brook, New Hampshire: an integrative approach to stream ecosystem metabolism. Ecological Monographs 43:421-439.*
- Fisher, S. G. 1977. Organic matter processing by a stream segment ecosystem: Fort River, Massachusetts, U.S.A. Internationale Revue der gesamten Hydrobiologie 62: 701–727.*
- Minshall, G. W., R. C. Petersen, K. W. Cummins, T. L. Bott, J. R. Sedell, C. E. Cushing, and R. L. Vannote. 1983. Interbiome comparison of stream ecosystem dynamics. Ecological Monographs 53:1-25.*
- Naiman, R.J., J.M. Melillo, M.A. Lock, T.E. Ford, and S.R. Reice. 1987. Longitudinal patterns of ecosystem processes and community structure in a subarctic river continuum. Ecology 68:1139-1156).*
- Newbold, J. D., J. W. Elwood, R. V. O'Neill, and W. Van Winkle. 1981. Nutrient spiraling in streams: the concept and its field measurement. Canadian Journal of Fisheries and Aquatic Sciences 38:860-863.*
- Newbold, J. D., P. J. Mulholland, J. W. Elwood, and R. V. O'Neill. 1982. Organic spiraling in stream ecosystems. Oikos 38:266-272.*

#### Stable Isotope Sampling:

The proposal indicates that stable isotope fractionation will be determined empirically “*Due to large discrepancies in isotopic values associated with trophic fractionation (Pinnegar and Polunin 1999; Zanden et al. 2001), we will use multiple years of baseline trophic isotopic values to understand trophic fractionation prior to nutrient treatment.*” The concern about spatial and temporal variation in fractionation rate is appropriate. However, it will be very difficult to accurately determine actual fractionation rates from samples collected from a natural system (i.e., Hancock Springs). Most determinations of fractionation have typically been made in

controlled environments where the stable isotope ratio of a food source is known. Consumers in a stream may be ingesting food items with varying isotope ratios and the relative proportion of food items may vary over time. As a result, assigning an isotope ratio to the diet of an organism is an approximation, making it impossible to accurately determine fractionation rates.

The terrestrial invertebrates to be analyzed for stable isotopes should be segregated into feeding groups, as is proposed for aquatic invertebrates. At a minimum, the terrestrial invertebrates should be separated into primary consumers and predators.

### Models

The ATP model offers a relatively simple depiction of the food web, but it still requires a large quantity of input data. Unfortunately, the proponents do not appear to be measuring several key variables needed for the model (e.g., groundwater DOM, FPOM). How will these components of the model be parameterized?

The description of how the ATP model will be used on page 47 seems as though it was extracted from an earlier version of the proposal: *“In the context of Yakama Nation led experiments in Hancock Springs and the Twisp River, the model will be used to simulate, a priori, the potential responses to both nutrient analog additions and brook trout removals. In turn, the results of such experiments will provide critical data sets, which will be used to calibrate and validate the ATP model.”* These sentences imply that experiments on the Twisp River are still planned, although the cover letter that accompanied the revised proposal clearly indicated that the Twisp River component of the study is being dropped. Also these sentences imply that a brook trout removal experiment is part of the study. There is no mention elsewhere in this proposal that this treatment is still being considered. But calibration and validation of the ATP model for brook trout removals will not be possible unless this treatment is part of the study. Some clarification on these points is required.

The description of the calculation of the fish production values described on pages 45-47 appear to be in error. The equations and text on page 46 are below:

*“The TBP method quantifies 1) the contribution by each prey taxon to fish production and 2) resource consumption rates that would support calculated fish production rates (Benke and Wallace 1980; Cross et al. 2011; Bellmore et al. 2013). Total annual production for fish species j is then parsed out into  $F_i$  components, which quantify the relative fraction of production contributed by each prey type i, such that total production for a given fish species,  $P_j$  is made up of the sum of the fractional contributions from each prey type:*

$$P_j = \sum_{i=1}^n F_i$$

(Eq. 7)

*These fractional contributions for each prey type are then calculated as:*

$$F_i = G_i \times AE_i \times NPE \quad (\text{Eq. 8})$$

where:

$G_i$  = proportion of prey type  $i$  in fish diet,  
 $AE_i$  = assimilation efficiency of prey type  $i$ , and  
 $NPE$  = net production efficiency.

For each fish species  $j$ , the proportion of fish production attributed to each prey type ( $PF_{ij}$ ) is then calculated from the relative fractions ( $F_i$ ) as:

$$PF_{ij} = \frac{F_i}{\sum_{i=1}^n F_i} \quad (\text{Eq. 9})$$

Finally, annual flow of biomass from each prey type  $i$  to fish species  $j$  ( $FC_{ij}$  measured in  $gDM \cdot m^{-2} \cdot y^{-1}$ ) is calculated as:

$$FC_{ij} = \frac{PF_{ij} \times P_j}{AE_i \times NPE} \quad (\text{Eq. 10})$$

where:

$P_j$  = annual secondary production ( $gDM \cdot m^{-2} \cdot y^{-1}$ ) of fish  $j$ ."

Several questions arose related to this set of equations.  $P_j$  is defined at the top of the page as the sum of the fractional contributions from each prey type (Eq. 7) and each fractional contribution is calculated as the product of proportion of that prey item in the diet, the assimilation efficiency and net production efficiency (Eq. 8). This value would be unitless. However,  $P_j$  as used in Eq. 10 is defined as annual secondary production for fish and expressed in  $gDM/m^2/y^1$ . Does  $P_j$  represent two different variables in these equations?

This series of equations also seems to imply that that  $FC_{ij} = G_i$ . To see this:

1. Substitute RHS of equations 9 (for  $PF_{ij}$ ) and equation 7 (for  $P_j$ ) in equation 10.
2. Note that summation terms (sum  $F_i$ ) introduced by substitution cancel leaving  $F_i$  in numerator
3. substitute RHS of equation 8 for  $F_i$
4. Note factors  $AE_i \times NPE$  in numerator and denominator cancel leaving  $FC_{ij} = G_i$

This outcome implies that annual flow of biomass from each prey type  $i$  to fish species  $j$  (in  $gDM/m^2/y^1$ ) = proportion of prey type  $i$  in fish diet. This conclusion makes no sense

conceptually and the units do not match. This issue may be related to the problem with the definition of  $P_j$ , noted above. However, Eqs. 7 and 8 indicate that neither  $P_j$  nor  $PF_{ij}$  can be in  $\text{gDM}/\text{m}^2/\text{y}^1$ , based on the manner in which these variables are calculated. As a result,  $FC_{ij}$  cannot be expressed in  $\text{gDM}/\text{m}^2/\text{y}^1$ . A thorough review of the calculations used for production estimates should be conducted.

### Reporting

The proposal did not contain any information about possible mechanisms for communicating results of the study other than through traditional scientific channels (i.e., publications, presentations at scientific meetings). Communication of the results to managers, policy-makers and restoration practitioners in the Columbia Basin should be a prominent component of the reporting and communications strategy for a project like this one. Websites, briefings, workshops, and training sessions are all possible options for enhancing the dissemination of the results. This aspect of the project should be given additional consideration in the proposal.

### **Facilities and personnel**

Personnel are well qualified for this study and the available facilities appear adequate, although the lack of a budget and detailed timeline make it difficult to thoroughly assess the adequacy of facilities and equipment. Additionally, the team needs someone to conduct outreach to managers and other researchers. WSU is a land grant university with the ability to do effective outreach, and this needs to be an important part of the project.

It is still not clear what would happen should the project leader not be involved with the project for its duration. Although a project timeline was not provided, it appears that the duration will be approximately 10 years. As the ISRP noted in its previous review, *"... this is a long-term project that, in effect, is largely led by a single person. Should the project leader leave, would the program stay the same or be as productive? What are the contingency plans?"* The project remains largely led by a single person. No specific contingency plans are presented for project continuation should the program manager depart. The proponents responded to the original comment that *"...the responsibility and the risk associated with project management as a whole are distributed and diversified among the team members."* However, the distribution of responsibilities is not provided.

### **Editorial comments**

Finally, the proposal would have benefited from further editing. Numerous references are missing, and others are cited incorrectly. Two elements of the previous proposal, enrichment of the Twisp River and removal of brook trout from Hancock Springs, were supposedly omitted in the revised study design, but details on these treatments still appear in a number of locations in this proposal. There also was a general lack of clarity in some sections of the proposal that made it difficult to determine the specific outcomes or products intended to be generated by this project.