INDEPENDENT MULTIDISCIPLINARY SCIENCE TEAM (IMST)

State of Oregon

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Ted Lorensen
Oregon Department of Forestry
2600 State Street
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Dear Ted,

The IMST recently reviewed the Hinkle Creek research proposal for the Oregon Watershed Enhancement Board (OWEB) and the Oregon Department of Forestry (ODF). We conducted our review using the general description of the research plan presented to the IMST and considered the questions identified in your June 18, 2002 letter:

1. Does each proposed project clearly identify a research hypothesis or question that will be addressed by the work?

2. Regarding the experimental design of each proposed component:
   - Does the proposed project and methods answer the research question posed?
   - Comment on the adequacy of experimental design with respect to the following: study location, stocks evaluated, sample sizes, sample replicates, years evaluated, and analytical techniques.
   - Does the experimental design appear to be appropriately supported by the expertise of technical and analytical staff?

The IMST reviewed the material provided by ODF and also requested outside reviews by Dr. Jim Hall and Dr. John Van Sickle. Dr. Hall, a Professor Emeritus at Oregon State University, was the leader of the original Alsea Watershed Study, one of the first paired watershed studies to investigate the effects of timber harvest on anadromous salmonids. Dr. Van Sickle is a statistician and landscape modeler with the US Environmental Protection Agency. Their reviews are attached for your additional information.

Does each proposed project clearly identify a research hypothesis or question that will be addressed by the work?

Unfortunately, few of the project proposals stated a hypothesis or clearly stated a research question. All projects identified a series of objectives of the study but did not explicitly identify research questions. Most of the objective statements were general and did not identify expected responses or the mechanisms responsible for the responses. The IMST attempted to interpret the likely questions that the research was designed to answer, but the lack of explicit questions and hypotheses is a clear deficiency in the proposed research.

The extreme brevity of the research descriptions made it even more difficult to evaluate the strengths of the research. We have never seen such brief research proposals. Conversations with some of the project leaders indicated that these project descriptions were not written as research proposals, but rather were handout material to accompany oral presentations about the planned research. Regardless, the lack of description of the proposed research and the lack of clearly stated questions or hypotheses make it difficult to endorse the proposed research in the Hinkle Creek watershed.

Does the proposed project and methods answer the research question posed? Comment on the adequacy of experimental design with respect to the following: study location, stocks evaluated, sample sizes, sample replicates, years evaluated, and analytical techniques.

The project will not be able to draw strong conclusions or inferences because of the lack of replication. This is a case study with no replication, much like the original Alsea Watershed Study. The researchers will not be able to rigorously attribute changes to the forest practices treatments imposed in the study design because any response in a subwatershed could simply be a random change. This has always been a major challenge for paired watershed studies and there are many papers in the scientific literature that discuss these limitations and possible solutions. The IMST was amazed by the exceedingly brief of discussion of past paired watershed studies and the total lack of discussion of the Alsea Watershed Study. Even though the Hinkle Creek proposal is designed to investigate current forest practices, in contrast to the previous studies, the lessons about variability, experimental design, and length of records required to detect changes would be extremely relevant.

One of the most critical omissions in the proposal is any discussion of the experimental design and the statistical power to detect changes. Past studies of sediment dynamics, hydrology, amphibians, and fish provide robust estimates of the variation that the researchers are likely to observe in Hinkle Creek. The researchers can determine the magnitude of a response that could be detected based on the proposed number of years of observation and the variance exhibited for specific factors. A manuscript by Bisson, Gregory, and Nickelson (in press) found that 15 years of record prior to a treatment and 15 years of record after treatment were required to detect a 50% change in populations of anadromous salmon based on variation observed in coho salmon along the Oregon coast. If those levels of variance were observed in Hinkle Creek, only extremely large changes in salmon populations could be detected (without regard to the pseudoreplication issue).

Does the experimental design appear to be appropriately supported by the expertise of technical and analytical staff?

The researchers for the different projects have extensive experience in the Pacific Northwest and are extremely well qualified to conduct the research. One minor concern we identified was the overlap between project (e.g., two studies of coho salmon by ODFW and OSU) with little indication of coordination between researchers. The Principal Investigators need to provide a

description of their overall project coordination and the approach for integrating the results of the individual projects. Currently, the only evidence of coordination is that all projects are located in the Hinkle Creek watershed.

Conclusions

In summary, we find the brevity of the research proposal and lack of replication of the paired watershed study to be major flaws in the Hinkle Creek research proposal. The IMST believes that the cooperation of the landowner is a valuable aspect of the proposed research. During our June 18, 2002 Team meeting, you had also indicated that ODF was planning to develop coordinated paired watershed studies on state forest lands. This would greatly strengthen the proposed research. The IMST strongly encourages the State to continue to develop this research direction and build on the research planned for Hinkle Creek. Development of explicit questions, statistical power analysis prior to initiating the research, development of additional paired watershed studies using similar research methods and designs, and development of research coordination plans would greatly improve the scientific rigor of the Hinkle Creek research project. The additional reviews by Drs. Hall and Van Sickle (Attachment A and B) provide valuable insights that should assist the researchers and ODF in designing the research to provide important answers about our current forest practices for the Oregon Plan.

Sincerely,

Stanley V. Gregory and William G. Pearcy
IMST Co-Chairs

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cc: Geoff Huntington, OWEB
Jim Brown, ODF
Ken Bierly, OWEB
Kelly Moore, OWEB
Neal Coenen, Oregon Plan Manager
IMST

Attachment B

To: Stan Gregory, OSU Dept. Fisheries & Wildlife July 8, 2002

From: John Van Sickle, US EPA, National Health and Environmental Effects Laboratory, Western Ecology Division, Corvallis, OR

Re: Review of Hinkle Creek Research and Demonstration Project

Stan -- I have reviewed the Hinkle Creek proposals and given special attention to the overall experimental design, research hypotheses, and statistical methods. In general, I think the proposal is a sound example of a paired watershed study. The N. and S. Fork Hinkle watersheds seem well-paired and well-suited to the study objectives. The project appears valuable and I hope that it will be able to proceed.

However, I have some concerns with the project's design and analysis. It may not be possible to address these concerns directly in the proposal due to space limitations, but I hope that the investigators will give them some thought in any case.

1) Timeline -- It would have been nice to see some more detail here. The project is slated to last for 10 years, with harvest treatment occurring in 2003-2004. What is the sampling schedule for all 4 components, both pre- and post-harvest? Annual? Same schedule on the "N" basins and the "F" reaches?

2) Treatment vs. Control -- It appears that the N. Fork and S. Fork have already been designated as "Control" and "Treatment", respectively. The study results would be most convincing if this choice had been random, but I doubt that it was. Apparently, the S. Fork has already had harvest activities within it, so it would be the logical choice for future harvest treatment. The danger with this choice is that the experiment's outcome is foreordained – because the S. Fork has already been impacted by harvest, it is much more likely to be seen as "impacted" in any future assessment, regardless of the actual effects of the 2003 – 2004 harvest.

Put another way, the investigators will have to be very careful and lucky to be able to convince themselves and their sponsors that any post-treatment effects in the S. Fork are due strictly to the 2003-2004 treatments, rather than to earlier harvest activities. A "settling-down" calibration period of 2-3 years (2001 – 2003) seems an awfully short time for existing harvest effects to fade away, given the time scales of channel-changing processes.

3) Sample Size and Statistical Inference --

As with any paired watershed study, this study has virtually no replication and so there can be no formal statistical testing of Treatment vs. Control differences. This is not a bad thing, and it is pretty much unavoidable in whole-watershed experiments. But it is important that investigators and sponsors understand that they will not be getting any P-values for the significance of harvesting effects.

The use of four “N” subbasins in the treated watershed allows for an “average” effect of harvesting to be calculated, although the sample size is pretty small. But there is only one “N” watershed in the Control watershed (N. Fork) against which to compare this average. (Why is the second no-harvest “N” subbasin being chosen from the treated S. Fork, rather than the Control N. Fork?)

It may be possible to carry out statistical testing if the design is set up as a Before-After-Control-Impact-Pairs design (BACIP design; Stewart-Oaten, et al. Ecology 73, 1396-1404 (1992)). However, it looks to me like the time sequence of sampling, especially in the “Before” (calibration) period will be to short for such an analysis.

Given the sample size restrictions, the investigators might instead consider trying to pair up their “N” subbasins (Treatment vs Control) with respect to slopes, aspects, geologies, etc. This would allow paired comparisons (but with no statistics) of both the “N” subbasins and the fish-bearing watersheds.

I noticed that the amphibian component talks about sampling in 4 reference streams, rather than the 2 streams mentioned in the general proposal. This appears inconsistent.

4) PIT Tagging

The proposal does not describe overall research hypotheses, but rather states a set of project objectives. This is fine for the sediment and temperature components, because the hypothetical effects of harvesting – increased temperature and sediment – are well known.

But the rationale and expected effects for fish movement, as studied by PIT tagging, are quite unclear and are not stated in the proposal. Both cutthroat and coho are to be PIT tagged and tracked, and this activity seems to be a substantial piece of project resources. (Again, what is the timeline? Are new fish tagged every year, or is the tagging done just once?).

The investigators should make it clear what specifically they plan to learn about harvest effects from the movement studies, that can not be learned from abundance sampling.