

October 21, 2015

Russ Henly
Assistant Secretary of Forest Resources Management
California Natural Resources Agency
1416 Ninth Street, Suite 1311
Sacramento, CA 95814

Re: Timber Regulation and Forest Restoration Program Pilot Projects Concept Paper

Dear Russ,

I wanted to take a few moments to outline some comments on the Forest Planning Watershed Pilot Projects Concept Paper dated August 24, 2015. I appreciate the public meeting you held last week, and I hope that you find my comments constructive.

I've tried to organize my comments in the format offered in the concept paper, but of course there are some comments that may overlap with more than one topic area.

Admittedly, there is a lot here to consider. I'm quite happy to engage with you and/or others more extensively where needed.

General Comments

While I am very excited by the fact that California is re-engaging the cumulative effects issues and understanding larger-scale forestry impacts, I would hope that the TRFR Program will learn from many of the expensive mistakes that were made by other jurisdictions in past decades. One way to leverage this knowledge is to include those with experiences in those other jurisdictions.

I suggest the team review the notes from the JAG Science and Research workshop for some insights into the challenges and opportunities of watershed assessments. I believe the panel discussions at that event highlighted several key issues this process is likely to experience. I've attached a copy of our synthesis notes from that

workshop.

I also strongly recommend reaching out the Dr. Kate Sullivan at US EPA about the approach considered by the TRFR. Dr. Sullivan is currently the Branch Chief for Ecosystems Assessment at US EPA, has extensive expertise in the design and implementation of watershed-scale assessment methodology, as well as extensive knowledge of both California's operating environment and ecological conditions. I believe she can offer some outstanding direction based on some pretty expensive lessons learned with other projects. Her lab is also developing some really interesting science-based approaches to ecosystem assessment that may benefit this project.

Critical Questions

The Critical Questions are (in my opinion) way too open ended. While these may be a good starting place, we will need a lot more specific structure to the inquiry or risk a process that is way too broad and general to be much use. I strongly suggest that the Program consider developing specific working hypotheses driven by known understanding of the dynamics between forest ecology and forest management practices.

The current questions appear focused on using the THPs as the primary information resource for the assessment. In general, the cumulative effects procedures in THPs seems like it might take the Program down a deep rabbit hole that will complicate and confuse. I suggest starting with first principles based on watershed process and function FIRST. Then look for various sources of information needed to explore these functions (which may or may not be found in the THPs).

In the absence of a set of clear working hypotheses, the potential "data gaps" are likely to be very large. The availability of data alone is a poor substitute for effective hypothesis testing or conceptual understanding.

The Program may want to think carefully about the objectives of the pilot (what is the purpose of the pilot)? Some careful thinking about the approach here might yield a more effective approach in building out an effective environmental management system.

I recommend the Program consider the following (in no particular order):

- What functions and processes are dominant in the watershed and what do we currently understand about these processes?
 - Where is our understanding weak, and what are the best methods for resolving the gap in understanding?
- What ecosystem process models do we have that apply to the study watershed (e.g., conceptual, quantitative, etc.)?
 - How much confidence do we have in these process models?

- What is the current recovery trajectory for [insert key ecosystem process] in this watershed and how well can.
 - Are existing management practices advancing or retarding the recovery trajectory?
 - How much can be attributed to past management practices?

Furthermore, I think there is value to focusing the lines of inquiry into a series of proxy ecosystem functions; for example,

- fuel loading and fire patterns,
- aquatic wood loading,
- hydrologic and sedimentary roading conditions and recovery,
- geomorphic sediment balance (are channel incising, stable or aggrading),
- timber growth dynamics (although this may be sufficiently addressed elsewhere).

Starting with these basic processes might narrow the lines of inquiry more specifically into ways that define core management impacts in ways that can be measured and evaluated.

We should appreciate that we are not starting from a place of ignorance. We have many decades of research and understanding to guide us. Let's start with an inventory of KNOWLEDGE instead of an inventory of DATA.

Data Collection & Characterization

It appears that this process will begin by compiling information. This is an approach that may distract from a more direct approach to watershed assessment. Sifting through vast quantities of data in search of meaning can be a daunting task. It is likely to generate all sorts of spurious correlations, errors driven by data generalizations, and relationships that cannot be discovered from the available data due to issues with resolution and original collection methodology.

The concept paper correctly recognizes MacDonald's (2000) point that accurate assessment is limited by understanding the key cause-effect relationships, and relevant data characterizing resources and watershed processes. However, this lack of understanding is not a function of "gaps in data topics" so much as a function of specific gaps in data resolution, quality *and relevance to key working hypotheses*. **Simply trying to fill these gaps by exploring "available data" misses the point that certain key interpretations can only be informed by careful study design and analysis.**

For example, we know from past experiences that inventories of data rarely produce reliable and objective interpretations reflecting management practices. Often conditions reflect a combination of legacy effects, historical natural disturbances, and a variety of integrated factors (e.g., fuel loading, fire suppression practices,

other land-use impacts, transportation network effects, introduction of invasive species and removal of predatory species, etc.). Discovering correlations among these desperate processes often requires a quality of data resolution that simply may not exist. Therefore, proxy data must often be uncovered and tested against working hypotheses or functional watershed models.

We also know that it is impossible to fill all the data gaps, and that there will always be a desire for more data. However, data does not resolve issues or define understanding. We need working models and working hypotheses. And in many cases, I suggest LESS data may be more effective than more data (it will help focus inquiry in ways that can resolve specific issues).

Mapping Standards

While mapping can be an effective way of visualizing spatial information, *the analysis of spatial data typically depends on more quantitative measures*. Comparing such features depends on data quality, scale and careful attention to the criteria for measurement. I suggest developing the data standards and analysis procedures *before* proceeding with mapping standards (trust me, this is based on experience). Otherwise, the maps will tend to drive the lines of inquiry instead of well-defined scientific procedures.

Site Selection

Nothing beats historic stereo aerial photo interpretation, which unfortunately does not necessarily conform to modern analytical tools. Existing GIS technologies rely mostly on orthorectified imagery. But considerable interpretive power is lost when the stereo aspect is removed. Again, this reinforces the need to define the study objectives before defining the assessment approach.

Because of the intensity of data required to evaluate true cumulative effects and recovery trajectories for key processes and functions, the Program might consider engaging the research forests in one or more pilot studies. This is where the JDSF research and monitoring strategy that we laid out could really be useful.

Cumulative Impacts Assessment Approach

Existing assessment methodologies are reasonably good at characterizing watersheds, but generally poor at evaluating cumulative effects, determining important cause-effect relationships, evaluating existing recovery trajectories or evaluating risks across watersheds. Also, most existing methodologies are too generalized to provide much useful guidance relevant to the goals of this project.

Having participated in a large number of watershed assessments similar to what this project is looking to do, I can share that devils are in the details. Evaluating

cumulative watershed effects is very complex and requires a great deal of experience and scientific acumen.

May need to consider some issues at larger scales – 3-10K is probably too small for fire effects, some watershed effects, etc. Channel gradient for example is an important factor for detecting sedimentation, and the ideal gradients may not occur in some watersheds of those scales.

Again, better to define the specific issues (and associated working hypotheses) and then seek to define the right scale of inquiry to resolve that question.

Restoration Opportunities

It seems to me that there already exist several major studies that outline research opportunities throughout the State. I suggest we start there instead of re-inventing the wheel. In my experience, restoration opportunities are a natural outgrowth of good watershed assessments. I'm not sure it requires a specific focus in the assessment methodology.

Process & Collaboration

The program's organizational framework does not appear to include a working group for the assessment methodology. The assessment methodology must be standardized enough to provide repeatable measures across a wide variety of geographic domains, but must also be flexible enough to evaluate specific issues of concern relevant to specific watersheds. I recommend that the program consider an assessment team that can focus on the key questions, working hypotheses, available models and assessment approaches.

Success as a system will depend on having a strong, independent pool of technical experts who can roam across the state to cross-pollinate across regions to provide some consistency between project areas. In many other State jurisdictions, there have been a strong community of expert consultants as part of the assessment teams. These experts bring more capacity to float around from project to project bringing a lot of continuity that can't occur when all contributions depend on large landowner contributions or regional agency staff.

The Program may need a funding source to support broader participation. Some collaborators are typically well-funded (e.g., agencies, academia, LO's), but the quality of work will also depend on independents that may not be as well funded (e.g., enviros, technical consultants, public, local govts, etc.).

Conclusions

I hope you find these comments in the constructive manner in which they are intended.

Personally, I would LOVE to participate in one (or more) of these types of projects. I believe my expertise and experience throughout the western United States offers a lot of potential benefit to the thinking of the TRFR Program. However, I certainly cannot afford to provide much quality into my contributions if my time is expected to be voluntary.

As always, please feel free to contact me with any questions. I can be reached at (510) 927-2099 or mike@soundwatershed.com.

Sincerely,

Mike Liquori
Principal, Sound Watershed