

Comments on Siskiyou Systematic Review, “Spreadsheet Sisiyou_Lit_Included_1218_v2.xlsx”

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1. Overall comments

Inclusion of literature from the Klamath Basin and elsewhere in northern California is warranted on ecological grounds. However, inclusion of data from elsewhere in western Oregon is equally warranted, but is not adopted here. For example, exclusion of ODF’s Ripstream research and publications is not adequately explained here, nor in my opinion can it be scientifically justified. In fact it is clear to me that the various publications from Ripstream research are far more directly pertinent and robustly informative to this review’s goals and objectives than any of the INCLUDED studies listed in current table.

It seems no specific biophysical basis has been offered by ODF for why Ripstream research is excluded herein, leaving open the likelihood that its exclusion is a political choice, not a scientific one. That choice—or the lack of an explicit and persuasive scientific basis for it—compromises and taints the review irreparably and unnecessarily.

In my view, the studies tagged in the spreadsheet as included in the study do offer useful information to inform the study questions. However, the result is a very small set of studies that are disparate in goals, methods, metrics, and designs, and draw conclusions about widely disparate, mostly non-overlapping subjects. They also comprise only a small fraction of the available scientific literature that offers useful information to inform the study questions. Hence, my comments emphasize that the problems I see lie in the exclusion of some studies that have highly relevant information, and the adoption of a Systematic Review framework that leads to illogical and crippling exclusions. See my comments below, *Appropriateness of the Formal Systematic Review Method*.

2. Study Exclusion by Geographic Factors

Several excluded studies included field data from areas of southwest Oregon directly adjacent to the Siskiyou region, such as the Umpqua Basin (e.g. Hairston-Strang and Adams 1997, 1998). By proximity and regional factors such as climate and snowpack rainfall distribution and annual hydrologic regime, these studies should be just as relevant to conditions within the study region as are studies from the Klamath region. In some cases geology and soils may vary somewhat from the Klamath Mountains terrain that dominates most of the Siskiyou Region, but the relationship is complementary, as some western and northern portions of the Siskiyou region include geology and soils more

similar to adjacent Coast Range sedimentary and Cascade Range volcanics terrain than they are to the Klamath-Siskiyou sheared metamorphic and sedimentary terrain.

As described above, the various published papers by Groom et al. from the RipStream study, derived from detailed field data on shade and stream temperatures in across western Oregon outside of the immediately adjacent Siskiyou Region, are highly relevant. No biophysical argument has been articulated and defended by ODF as to why these results are not applicable to streams in the Siskiyou Region, despite that I and others have pointed out in previous comment that nothing in the available record suggests as a scientific matter that streams in the Siskiyou Region respond differently in shade change v. temperature change relations than streams in any other region. In what data are available data indicate to me that streams in the Siskiyou Region lie in exactly the same domain in this regard as those in the Ripstream studies. Data are available for ODF to test this question directly, but instead, ODF has chosen to a priori discard all consideration of the Ripstream research. Exclusion of these studies is apparently justified by ODF on vague geographical grounds—but also is cited to political directive ODF suggests it received from the Board. Regardless whether that characterization of the Board’s intent is accurate, when relevant scientific information is excluded from such a review on political grounds, that renders it overtly a politically compromised report, not a scientific review. I do not see any way around this fundamental conclusion

3. Study Exclusion by Topical Factors

I agree that many of the studies excluded from further consideration are appropriately excluded. Most of these pertain to specific project outcomes that aren’t either directly or indirectly pertinent to riparian logging rules on private forest lands. However, some of the excluded studies appear to me to be highly pertinent to understanding the consequences and causes of stream response to riparian forest management practices and it’s not clear at all from the offered information why they were excluded.

My greatest concern is that the opportunity for a useful review is fundamentally compromised by excluding studies that are directly pertinent to assessing the outcome of forest management in terms of stream water quality or fish habitat. Indeed some of the excluded research is likely *crucial* to elucidate causal relationships between logging in riparian areas and stream conditions. One example is Benda and Bigelow (2014). This paper includes directly relevant information on large wood in streams from Klamath Mountains, Cascade Range and northern California Coast Range regions that lie directly or virtually adjacent to the Oregon Siskiyou region, and share closely similar climate and vegetation. The study classifies riparian forests as “managed” and “unmanaged,” and although it emphasizes the role of geomorphic settings and processes on wood recruitment to streams, it also documents specific trends associated with forest management in riparian areas. In my opinion it’s irrational and crippling to this review’s

objectives and goals to exclude studies such as this one from the scope simply because (as far as I can glean from the vague entries in the spreadsheet) they do not quantify site-specific aspects of forest stand conditions and treatments. At an absolute minimum this paper conveys crucial contextual and quantitative information about large wood recruitment processes as an “effects modifier” determining the long-term and cumulative instream effects of riparian zone management. That is just one example among many,

A dozen or so similar examples I identified on the Considered but Excluded list in ODF’s spreadsheet include obvious geographically relevant literature on exotic weed encroachment, Port-Orford-cedar root rot, oak diseases in relation to logging disturbances--these are obvious factors that can directly and that hugely affect forest succession in riparian areas after disturbance by logging and other factors, including potentially strong effects on shade recovery, large wood recruitment, erosion resistance, fire severity and recurrence intervals, sediment retention, and nutrient leaching to surface waters with loss of key riparian species and their replacement by vegetation species with dramatically different phenology. Thus they pose potentially extreme consequences for future forest states and water quality and fish habitat in the face of riparian forest logging rules and other aspects of forest management. They are directly salient to the study questions and it would be illogical and crippling to exclude the information in these studies from explicit consideration in a science review.

If the factors listed above are to somehow be addressed as “effects modifiers” in the systematic review, it remains utterly unclear how that will be accomplished, when the primary literature relevant to them is excluded from review.

As you know, several Oregon DEQ TMDL studies are directly and immediately responsive to the study questions at hand, and they are available for numerous streams within the Siskiyou Region. Does ODF consider Oregon’s TMDL studies—based on simulation models calibrated against select field data—to be science, or not science? In my opinion, they are science, just as much so as many other studies identified for inclusion, therefore ODF should offer scientific reasons for their exclusion from this review. Suggesting that testimony from ODEQ to the Board somehow substitutes for inclusion of these studies in a systematic review offers only a political rationalization for their exclusion, not a scientific one. Again, if relevant scientific information is excluded from such a science review on political grounds, that renders it overtly a politically compromised report, not a scientific review. I see no way around this fundamental premise.

Similarly. My and others’ previous comments on the future importance of climate change in altering forest-stream state relations seems absent from the list of included literature.

4. Appropriateness of the Formal Systematic Review Method

In formulating the criteria for any effective literature review, one must balance the exclusion of science that sheds substantial light on the questions of interest against criteria that emphasize quantitative, geographical, or methodological comparability. The topical sweep of the "Accepted" list of studies in this spreadsheet is very broad, and it's hard to find even two on that relatively short list that address the same study question or have data that are remotely comparable, or even that draw conclusions about the same practices and forest conditions. The upshot here, in my opinion, is that the available scientific literature pertinent to the question of riparian forest management effects on water quality and fish habitat in the Siskiyou Region is not appropriate for a formal systematic review of the kind ODF is attempting to implement here. The formal construct for Systematic Review was designed as a means to cut across multiple controlled, quantitative studies that may use somewhat different methods and designs, but bear on the same fairly narrowly-defined question, and usually with common or comparable metrics (as is commonly found in medicine or pharmacology). The approach is useful to evaluate science when multiple studies approach the same or closely similar research question, but appear to offer conflicting results or interpretations.

The inherent unsuitability of this approach for best available information at hand re. the Siskiyou region questions is illustrated by ODF's exclusion here of numerous publications with directly or indirectly relevant, even crucial information, in favor of a handful of studies that appear to meet narrowly defined criteria for acceptance, but which themselves are in fact so varied there is very little overlap among their objectives, designs, methods, data, and findings. A formal systematic review with a narrow scope is the wrong construct for review in this case.

ODF should instead support or conduct a traditional general literature review and synthesis, in which every relevant study is evaluated on its own merits, and assumptions are identified, critical uncertainties are evaluated, and general conclusions are drawn based as best as can be on robust logical resolution of apparent data contradictions and weight of evidence. That would be the appropriate framework to produce a robust, complete, and coherent review and synthesis of relevant science to inform the policy questions at stake. In such a review, for topics considered of high importance for which studies in the specific geographic area of interest are lacking or deficient, the review makes a concerted effort to synthesize the best available information from comparable areas of not dissimilar climate, geographical, and land use. Such a broad search is coupled with a careful and explicit consideration of whether and why any specific biophysical factors that vary between the regions may limit the certainty by which information from these other regions can be extrapolated to the study area. In lieu of such a transparent and rational scientific analysis, the approach exemplified here by ODF relies on seemingly arbitrary and apparently political, not scientific reasons for excluding much highly relevant science (e.g., the Groom et al. Ripstream literature).