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First name: Pete
Last name: Martin
Organization:
Title:
Comments: I am grateful for the opportunity to comment on the proposed Forest Plans Amendment concerning the so-called Eastside Screens. I've studied much of the supporting documentation, including those cited papers that were freely accessible, and I appreciate the scholarship of the authors and the considerable effort represented by the research and compilation of this information.

The foundation principles expressed in paragraph 2 of Section 1.5, Need for Change, are well stated and provide an agreeable starting point.

My concerns are about:

(1) whether the supporting material led to the proposed management change, or whether the supporting material was selected to justify the management change---specifically I wonder if the proposed Forest Plans Amendment comes primarily from a wish to "harvest" big trees for their greater timber value;

(2) whether this amendment may be a change of the rules crafted to preclude further challenges by public-interest groups, in a perceived adversarial contest---specifically whether the proposed Forest Plans Amendment is an attempt to avoid oversight or legal challenge to local timber management decisions; and

(3) possible omission of alternative paradigms for management---specifically that we fail to recognize that there is less wisdom in human schemes for ecosystem management than there is collectively in the ecosystem itself, which by its very nature exemplifies intelligence and adaptability.

Regarding concern (1): One gets the sense that the case for eliminating the "Eastside Screens" was constructed to support a pre-ordained conclusion, in the manner of a lawyer's case, as contrasted to an earnest scientist's case, built in an open-ended manner as circumspect inquiry leads to a tentative model. This sense comes from the inclusion of excessive numbers of citations which actually do not clearly support the questionable claims that precede them, and from the likelihood that there may be ulterior motives to cut trees over 21 inches dbh quite apart from any ecosystem management aims. I respect the academic excellence of all the authors cited, and I recognize that Johnston specifically questions the propriety of the 21-inch (or 53-centimeter) limit, but I think many of the citations are padding added to lend an appearance of undisputable authority to statements made.

For example, in the "Need for Change" section (1.5), there is the assertion "Old trees are at elevated risk of mortality when young trees compete with old trees for light and water," followed by six citations (Bradford and Bell 2017, Millar and Stephenson 2015, Fettig et al. 2007, Kolb et al. 2007, Waring and Law 2001, Kolb et al. 1998).

But in Millar and Stephenson, for example, I find only the statement that "In many cases, drought-hardy species, species with physiological plasticity capable of coping with compound stresses, and species with shorter statures might outcompete current species."

Fettig, et al, 2007, use the word "competition" 15 times, but not specifically with reference to "young" trees over 21" dbh competing to the detriment of desirable "old" trees.

Kolb, et al, 2007, remark (in the abstract) that "...old trees are often more prone to dying after burning than younger trees." That would lead me to think that perhaps there is wisdom in not cutting "young" trees over 21" dbh (as part of an attempt to reduce basal area), in recognition of the fact that they comprise the next generation of "old" trees, even if they are not of the preferred species by some prescriptive human predisposition. The authors of Kolb, et al, 2007, do speak specifically of the potential benefits to old trees (Ponderosa Pine) of removing mid- and under-story younger trees, but I would not presume that is likely to include trees over 21 inches dbh.

That the motive for the elimination of the 21-inch rule may not be just forest ecosystem managent, "to conserve and promote old growth forest", but rather to effect increased timber production, is apparent in the review paper of Spies, et al. 2019 ("Twenty-five years of the Northwest Forest Plan: What have we learned?" Frontiers in Ecology and the Environment. 17(9): 511-520), not cited directly in the Preliminary Environmental Assessment but cited in the "1994 Eastside Screen - Large Tree Harvest Limit: Synthesis of Science Relevant to Forest Planning 25 years Later" white paper: in the Spies, et al. review the first "In a nutshell" bullet point is that the North West Forest Plan did not meet timber production goals.

Many of the citations represent a school of thought, literally: Oregon State University and the Pacific Northwest Research Station. That's fine, but I think there is an absence of citations (because of lack of consideration) of alternative schools of thought; for example one might consider the work of Robin Wall Kimmerer of State University of New York College of Environmental Science and Forestry ("Native knowledge for native ecosystems", Journal of Forestry, 2000, 98(8), pp. 4-9); or the work of Suzanne Simard of the University of British Columbia on cooperation (as contrasted to competition) between trees; or after the citation of Waring and Law, 2001, from Tree Physiology, perhaps it would be pertinent to consider Beverly Law's more recent (2018) work as co-author of "Land use strategies to mitigate climate change in carbon dense temperate forests", published in the very prestigious Proceedings of the National Academy of Sciences (115 (14) pp. 3663-3668).

Regarding concern (2): I have some familiarity with the Snow Basin timber sale issue mentioned in the "History of Eastside Screens" section (1.2). I think it was an example of a poorly conceived project, at least as it played out on the ground after timber-markers did their work. Moreover, representatives of the Forest Service apparently lied in court when asked about the ages of trees marked to be cut. This of course relates to the question of trust, apparently well-considered in the Preliminary Environmental Assessment. But it also suggests that it may be best for there to be clearly-defined limits for management decisions, to facilitate the ideal that consensus is preferable to the concentration of Decision Maker power or the delegation of authority.

Regarding concern (3): I believe we might be able to do better by our forests if we could get beyond certain paradigms, or habits of thought. One is that of "competition for resources". Spies, et al. do mention "synergies among species" in the "Conclusions" section of their 2019 review, but from 27 years of my own experience in afforestation and permaculture I'd say that it is under-appreciated.

Another paradigm, or habit of thought, that we might do better without is the relentless presumption that we, as stewards of the forest, must incessantly engineer and control it, even pushing it in whatever direction we think it should go, in light of expected environmental changes. The problem with this habit is that we may not allow nature to reveal to us an unexpected, better course of adaptation, and we may exacerbate the problem that we have precipitated.

I was unable to read the Johnston, et al. paper in review for Ecosphere, but by its title I gather that it presents the results of a model simulation of forest management wherein cutting trees over 21 inches dbh produces desired outcomes. I think models, necessarily incomplete by nature, are appropriate in arguing for curtailment or restraint with respect to destructive practices, but they are not appropriate in arguing for destructive practices, even where the longer-term or larger-scale aim is supposed to be conservation.

Finally I'd like to comment on some assumptions or assertions in the Preliminary Environmental Assessment (or in cited material) that are questionable to me.

In the Preliminary Environmental Assessment supplementary Vegetation Report, Forest Succession Assumptions: All Alternatives (p. 9) there is the statement that "Large fires with uncharacteristically large patch sizes will favor species with light windborne seeds that are capable of reseeding areas much farther away from reproducing survivors instead of the fire adapted species with heavy seeds (e.g. ponderosa pine) which do not travel far from the reproducing individual," followed by six citations. I question this because in the Wallowas, where I have visited several large burned areas, the natural regeneration that I have observed has been predominantly Lodgepole pine and Western larch, with some Engelmann spruce and Douglas-fir seedlings present, but certainly not a preponderance of Grand fir seedlings.

It is further stated in that section that "The increasing representation of these fire intolerant trees in dry and

moist forest landscapes creates a feedback loop that perpetuates conditions more conducive to severe large-scale disturbances with increased risk of a future altered or unique vegetation conditions [sic]." The logic strikes one as odd, as it seems to suggest that, with increasing fires, the bogeyman species Grand fir, which is said not to be fire-adapted, begins to predominate.

The deeper question might be, "What is appropriate or acceptable natural adaptation to anthropogenic global warming and increasing fire events?" An underlying assumption that prevails in the school of thought represented by the Preliminary Environmental Assessment is that "historical conditions" (as surmised, for example, from General Land Office records of the late 1800s) provide a good model of long-term well-adapted forest structure and composition. But it may be that we are departing even from the disturbance regime that prevailed for centuries before now, and it may be that the forest ecosystem is more effective than human imagination in managing adaptation. If so, it is likely that even "young" trees over 21 inches dbh may be part of nature's solution, not part of nature's problem.