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Interactive comment

Interactive comment on "Declining suspended sediment in United States rivers and streams: Linking sediment trends to changes in land use/cover, hydrology and climate" *by* Jennifer C. Murphy

Anonymous Referee #1

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Major comments Methodology

Pg. 5, ln. 13-14: Here you explain how "daily concentrations are flow normalized (FN) to remove the influence of year-to-year variability from stream…". However, the manuscript focuses on exploring potential drivers grouped into two general categories: (1) land use/management changes, and (2) streamflow regime. I am a bit confused as to how you normalize / remove the effects of flow, but at the same time attribute the changes in stream sediment to "streamflow regime". Can this be clarified? Similarly, on Pg. 6, ln 30 to 31 you give citations on how you "parse water-quality trends into the streamflow trend component (QTC) and management trend component (MTC).” Although, I agree that citations are a convenient way to cite methodologies, I believe that most of the results and conclusions are based on this “parsing” method. Therefore, it would be important to see explicitly how methods from Choquette et al. (2019), Hirsch et al. (2018a) and Murphy and Sprague (2019) were combined, modified, and used to arrive at the QTC and MTC values. I think the current length of the manuscript is good. Therefore, this should be included in the supporting information. I believe this will make the work presented in this manuscript more transparent for future readers, rather than trying to mix and match methods from three different sources. One can then better understand how these methods were applied in this manuscript and judge whether the presented results make sense.

Pg. 5, ln 23-24: “. . .to gauge the uncertainty of the trends, likelihood estimates of the trend direction for each site and parameter were extracted from Murphy et al. (2018)”. I tried to find how this was calculated by looking up this reference. However, this appears to be a USGS dataset which points at yet another set of citations for methods. I believe the citation for the calculation of likelihood is from Oelsner et al. (2017). However, Oelsner et al. (2017) calculates a number of things. Similar to the comment above, I think the supporting information should include a section where the author lists the equations used for the most critical calculations being made (the ones the support the figures, results, and conclusions). The citations can and should still be in the manuscript. However, they are a poor substitute for trying to understand the statistical methods that were used in this manuscript. I think a short appendix where the author provides the reader with the methods used could help the reader identify the necessary methods to recreate the results presented in this manuscript. Pg. 5, ln 10-29. Just curious as to why the author did not use Ryberg et al. (2013)'s SEAWAVE-Q method to separate seasonality and streamflow from the sediment concentration data? This is an available method from the USGS, so I expected it would be popular amongst other USGSers. Is WRTDS superior to SEAWAVE-Q for some reason? Furthermore, it would be important to cite Sullivan et al. (2009)'s contribution to your introduction (pg

2., ln 9-26) who compared various statistical methods for trend detection.

Cluster of increasing sediment in NW US

Pg. 1, ln. 1, title: “Declining suspended sediment in US rivers and streams” – How about the northwest coast TSS cluster that is showing a clear increase in suspended sediment. A more accurate title would be “Changing suspended sediment in United States rivers and streams…”. Pg. 8, ln 13-16. Discussions about the TSS trends in northwestern US seem to need a better explanation. One reason that comes to mind is deforestation, which is arguably one of the largest contributors (in terms of land use change) to suspended sediment concentrations in rivers. A quick search on Global Forest Watch (https://www.globalforestwatch.org/) shows that there has been a decrease in tree cover in the states of Washington and Oregon since 2000. Perhaps there is a correlation between the decrease in tree cover and increase in suspended sediment concentration. Pg. 8, ln. 25 “For TSS, undeveloped sites had the largest proportion of upward trends and some of the largest increases in TSS compared to sites in other land-use categories”: again this seems to beg further explanation. Not sure if the “undeveloped sites” are forested regions mainly used for timber. Pg. 8, ln 30 – 33: “Thus, the stark difference between the largely downward SSC trends and largely upward TSS trends at undeveloped sites in western US could be due to differences in the causes of changes for undeveloped sites…”. OK but this is an unsatisfying explanation. The key would be to dig a bit further to help the reader understand why there are strong spatial correlations, which there appears to be from within the TSS data in Northwest US.

Pg 14, ln. 25-26 and Table 1: Should add forestry/logging to “Land-use and land-cover changes across entire watershed”. This could explain the Northwest increase in TSS.

Pg. 17, ln 28-30: Your statement about many sites exhibiting a decrease in sediment should include a statement about the cluster of increase sediment trend in undeveloped NW US. Surely the remarkable pattern there deserves some recognition and further explanation.

Outlook

Pg. 17, ln. 12: This section lays out the limitations nicely. However, what is missing is a brief outlook. How can we make better sense of these results in the future? What are the priorities for this work moving forward? Is sediment pollution going to be a problem in the future? Or do the trends suggest that this problem is solved? Give the reader your take on where this research needs to go next and what the next few decades will be like based on what your learned from your analysis and the last two decades.

Minor comments Pg. 1, ln. 23 and Pg. 17, ln. 17: You suggest that "conservation efforts" may be successful to reduce sediment runoff as lands are converted to urban and agricultural uses. These "conservation efforts" sound vague, are there any specific efforts you are speaking about. Is there any evidence, from either literature or observations, that these conservations efforts are effective? Also, Pg. 9, ln. 14: ". . . management actions on the landscape likely led to decreases in sediment concentration". Same comment here, this is a vague statement about management actions. Any ideas which management actions are effective in reducing suspended sediment concentrations? Are there many? Pg. 12, ln 5: "suggesting conservation efforts to reduce sediment runoff to streams may be successful". Can you be more specific here? Pg. 17, ln. 17: Again what efforts are you speaking of?

Pg. 3, ln. 18: Can you describe the mitigation measures that are being implement in the Conservation Reserve Program?

Pg. 3, ln 26: ". . . to characterize changes in annual mean concentrations of suspended sediment." Are the annual means a good metric to be looking at for long term trends. Annual mean concentrations can be easily skewed by a large number of low concentration values during low flow periods (e.g., in the winter when runoff is minimal across the northern US. Wouldn't one expect to have a large amount of low suspended sediment concentrations that would skew the average. Would a clearer picture of the annual suspended sediment concentrations come from looking at annual median, 75% percentile or peaks concentrations (e.g., that come during the spring melt and/or high intensity short duration rainfall events)?

Pg. 8, ln 4-7: "Larger percent decreases tended to occur at sites with high concentrations in 1992 whereas the largest percent increases occurred at sites with low starting concentrations (Fig. SM-1)." There are only about 9 samples that fit this description on the SSC plot of Fig. SM-1. The rest which appears to be a cluster of samples (probably more than 9) are closer to a starting concentration of less than or equal to 60 mg/L. The point being that there are a large number of large decreases with low starting concentration as well. Therefore, this statement does not seem to accurately reflect what is presented in Fig. SM-1.

Figure 2: This is a very nice figure that should be enlarged for the Western, Central, and Eastern regions. At the continental scale it is a bit difficult to see spatial trends. Specifically, it is difficult to see the spatial distribution of triangles for TSS (especially Northwest and Eastern regions). For SCC the difficulty of seeing the spatial distribution is mainly in the Eastern regions.

Pg. 12, ln. 1: "...changes in the number of low-medium density dwellings ... had little to no effect on the streamflow regime." I find this hard to believe, would not increase in urbanization change the streamflow regime (e.g., increase rainfall-runoff response from increased paved surfaces).

Pg 12, ln. 30-32: "Previous models have suggested that changes in climate will lead to increases and decreases in sediment in particular rivers or areas of the western US." This is an ambiguous statement. I suggest to delete it or elaborate a bit more with an explanation of where increases and decreases are expected.

Pg. 13, ln. 24: "indicated that large decreases in streamflow relate to large decreases in sediment concentration".

Pg 13, ln. 27-28: "... these improvements are partially offset by human activities in the waetershed." I think this partial offset you speak of is not supported by Figure 7 because the negative correlation MTC for Q slope is very weak.

Fig. 8: Including Watershed Land-Use Change as an additional column would make this figure more insightful. I would imagine that MTC should have a stronger correlation and p-values associated with Watershed Land-Use Change.

Pg. 14, ln. 25-32: This explanation with how to interpret Fig. 5 should be when Fig. 5 is 1st introduces (i.e., Pg. 9, ln. 26, rather than 5 pages later.

Pg. 14, ln. 33: The explanation of high relative humidity leading to more vegetation and less erosion is very helpful in interpreting and understanding the results presented in Fig. 5. More explanations like these would be helpful.

Pg 15, ln. 17-20: Here you indicate that QTC and MTC usually exhibit opposite trends. Can you speculate as to what this means and why this happens?

Pg. 15, ln 25-27: Here you mention that 1 SSC and 10TSS trends had a large change in sediment and MTC near zero. Is there a reason for this? Is there a spatial pattern for these sites? What is special about these sites?

Pg. 17, ln. 19: You state here that land management was the primary contributor of changes in sediment. Can you give the average percentage? You also state that streamflow regime had a mild-to-moderate influence on sediment. Can you give the average percentage here? The purpose of this comment is to move away from a qualitative statement to a quantitative one.

Pg. 17, ln 23-25: The proximal zone results is not discussed in very much detail in the manuscript and adds little insight. I would suggest removing it from the main text and figures. Move it to the supporting information.

Fig. 5: Fracking wells is negatively correlated for SSC in undeveloped lands. Can you explain why this may be the case? I can imagine that fracking activity would require

large quantities of groundwater extraction and that this could decrease local stream baseflows, leading to higher TSS and SSC values. Instead the opposite is true, can you explain?

Fig. 7: In the 'Low-med density dwellings' column, there appears to be a number of undeveloped and mixed land-use points located along a vertical line on the zero change in low-med density dwellings. Is there an explanation for this pattern? Also, have you considered non-linear regression? Did any of the relationships exhibit non-linear dependencies?

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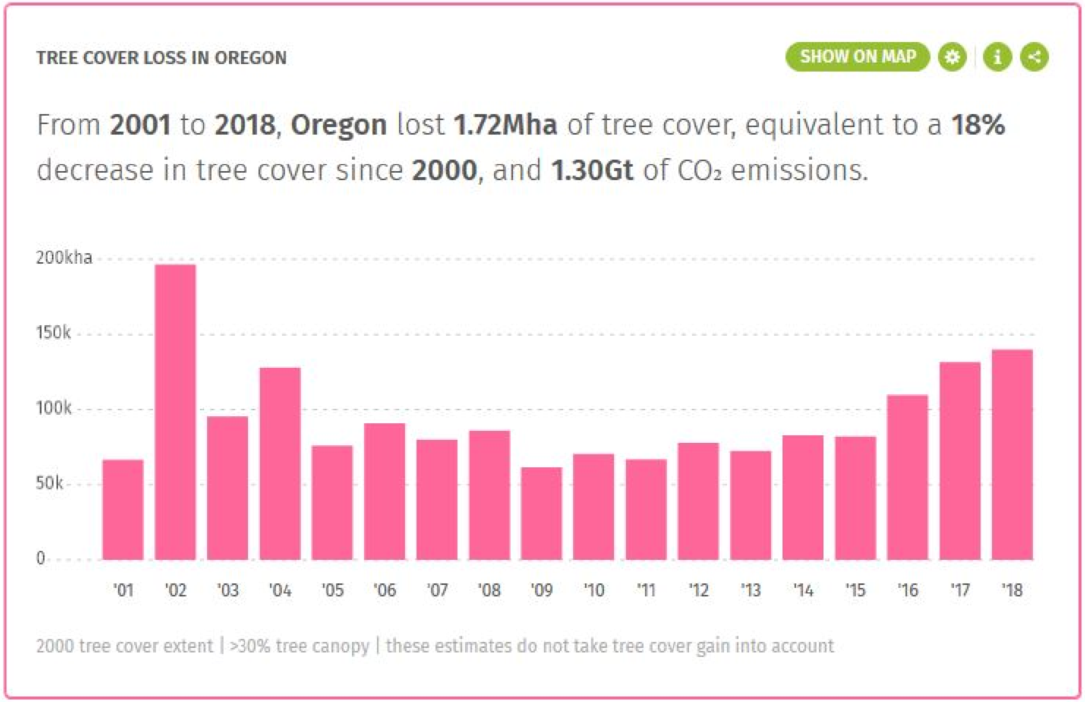
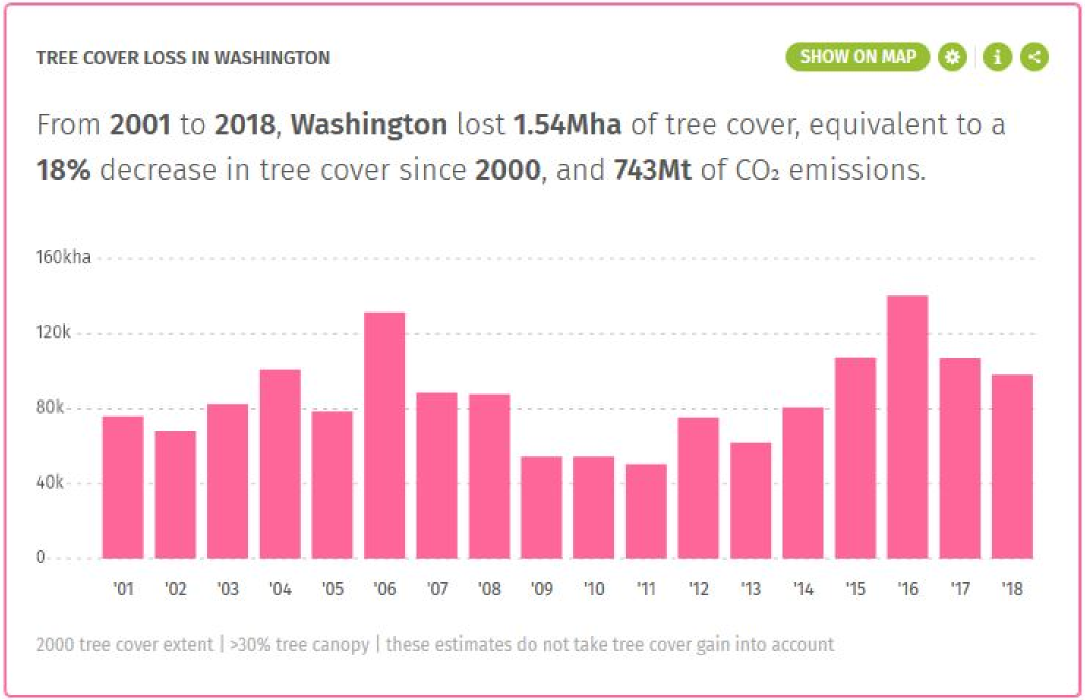

TREE COVER LOSS IN OREGON
SHOW ON MAP
From 2001 to 2018, Oregon lost 1.72Mha of tree cover, equivalent to a 18% decrease in tree cover since 2000, and 1.30Gt of CO₂ emissions.
200kha
150k
100k
50k
0
'01
'02
'03
'04
'05
'06
'07
'08
'09
'10
'11
'12
'13
'14
'15
'16
'17
'18
2000 tree cover extent | >30% tree canopy | these estimates do not take tree cover gain into account

Fig. 1.



TREE COVER LOSS IN WASHINGTON
SHOW ON MAP
From 2001 to 2018, Washington lost 1.54Mha of tree cover, equivalent to a 18% decrease in tree cover since 2000, and 743Mt of CO₂ emissions.
160kha
120k
80k
40k
0
'01 '02 '03 '04 '05 '06 '07 '08 '09 '10 '11 '12 '13 '14 '15 '16 '17 '18
2000 tree cover extent | >30% tree canopy | these estimates do not take tree cover gain into account

Fig. 2.