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Discussions

Interactive comment on "Climate change and wetland loss impacts on a Western river's water quality" *by* R. M. Records et al.

Anonymous Referee #2

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This study examines the effects of both future climate change and possible future wetland loss on flow volume, sediment load, and nutrient loads in the Sprague River basin. To this end, the authors use the SWAT model, calibrated at 4 gauges within the basin, and driven by the downscaled outputs of 3 CMIP5 GCMs (spanning the range of CMIP5 forecasts for the basin) for the RCP4.5 and RCP8.5 pathways, to compare flows and loads between historic (1950-2005) and future (2030-2059) periods. The authors find that climate change impacts alone may cause anywhere from an 11% drop (coolest, driest climate) to a 38% increase (warmest, wettest climate) in annual total phosphorus (TP) and similar changes to total nitrogen (TN) flowing into Klamath Lake. In contrast, wetland losses could result in increases in TP of 58% (coolest, driest climate) to 97% (warmest, wettest climate) over historical levels, and smaller increases (23-

31%) for TN. Losses of riparian wetlands along first-order and third-and-higher-order streams cause the bulk of the nutrient load increases, which the authors attribute to the large area of third-and-higher-order riparian wetlands and the higher TP loads flowing through the first-order riparian wetlands. The authors also find that wetland losses have the highest impact on high-magnitude, low-probability flows, which typically carry the largest nutrient loads, and which increase in frequency under the warmest, wettest future climate.

I think the study addresses an important question – should we worry more about land cover change or climate change, and how will the two interact? – and its experimental design is sound. My criticisms are fairly minor, involving the metrics used for model evaluation and the discussion of the results. I recommend publication after minor revisions.

General comments:

1. When evaluating the model performance at the four gauges (section 4.1. and table 4), the authors present metrics such as percent bias, R2, and NSE. While these normalized metrics allow for comparison between streams of different flow volumes, for example, the normalization makes it difficult for the reader to interpret their meaning. For example, the bias of 97% in TN in the South Fork of the Sprague River seems huge, but perhaps it is a bias of 97% of a very small observed load, in which case perhaps we can live with it. Similarly, without showing us the mean annual flow volumes at each gauge, we cannot tell how important a given tributary is to the overall water, sediment, or nutrient budget of the system. E.g., perhaps the 97% bias in TN in the South Fork is a minor error given its small contribution to the system – we can't tell. Forgive me if this information was posted elsewhere in the paper or the supplemental materials; but if so, the fact that I did not readily find it means other readers will be confused too.

2. Again, regarding table 4: if flow distributions (specifically, extreme flows) are so important to nutrient transport and have been overlooked by previous studies (as the

authors state in the introduction), then why have they evaluated their model in terms of annual flows? Wouldn't it be important to demonstrate that the model can, in fact, reproduce the high-magnitude, low-probability flows that are revealed, in the results section, to deliver the bulk of the nutrients? I would like to see some metric of the fit (rank probability score? Or something similar) of the flow distributions, and of the nutrient loads under those flows (to the extent that observations are available for this). This is necessary not only to validate the model itself, but also the meteorological forcings (including the weather generator).

3. Regarding the downscaled GCM forcings, could you clarify how shortwave, longwave, and humidity were downscaled from the GCMs? Were they taken from the GCM outputs and downscaled, or were they derived via the SWAT weather generator from downscaled GCM air temperatures? The reason I ask is that using indexing methods to derive humidity from downscaled air temperature, instead of using downscaled humidity, has been shown in at least one case to cause humidity trends that were opposite to those of the GCM (Pierce et al., 2013).

4. Again referring to table 4: The model underestimates nutrient loads in 2 of the 3 tributaries to the mainstem, and overestimates TP (by 26%) at the Sprague River Main Stem gauge. The authors speculate that the underestimation at the upstream gauges is due to various upstream sources not accounted for in the model. However, this does not explain the overestimation at the main stem gauge. To me, the overestimation of TP at the main stem, despite an underestimation at the upstream gauges, implies one of the following things: a) the assumed rate of nutrient input from agricultural activities along the main stem (which is where the vast majority of them appear to be) is too high, b) the rate of nutrient removal by riparian wetlands along the main stem is too low, or c) maybe nutrients are exiting the stream via groundwater (not sure how likely this is). Are there any tests you could perform to isolate which model component is to blame (for example, comparing simulated and observed relationships between nutrient load and flow volume; vary agricultural input rate and other wetland parameters and

see if fit improves, etc)? And if the model either overestimates agricultural inputs or underestimates nutrient removal rates, how would these model limitations affect your predicted future nutrient loads and the effects of wetland losses?

Specific comments:

p. 4928, line 22: You didn't mention the Sprague River before referring to "this" watershed, except for in the abstract. You need to specifically mention it in the introduction before referring to "this". A few sentences describing the Sprague River watershed and why you selected it (is it a good example of a basin whose wetlands are under threat?) would suffice to introduce the watershed.

References

Pierce, D. W., A. L. Westerling, and J. Oyler, 2013: Future humidity trends over the western United States in the CMIP5 global climate models and variable infiltration capacity hydrological modeling system, Hydrol. Earth Syst. Sci., 17, 1833-1850, doi:10.5194/hess-17-1833-2013.

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