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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.

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We appreciate the referee's well-considered comments and the reference provided.

Referee: General comments:

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.

Response:

This information was not in the paper but we certainly agree that it should be added to the manuscript. We suggest adding the following:

P4929 L9, after "downstream of this confluence" we suggest adding the following: "On average, the Sycan River contributed about 20% of flow at the Sprague River outlet, while the North and South Forks both contribute approximately10-15%. Between water years 2004-2006, the Sprague River tributaries were estimated to account for 80% of the suspended sediment near the Sprague River outlet, of which about 60% is from the South Fork of the Sprague River, about 30% from the North Fork of the Sprague River, and the remainder from the Sycan (Graham Matthews and Associates, 2007). During the 2000s, the Sycan River at site 4 (Fig. 2) contributed an estimated 4-9% of the annual TN load near the Sprague River outlet (site 2, Fig. 2), and the middle North Fork of the Sprague River (site 5, Fig. 2) contributed 23-35%; for TP, these estimates are 6-18% and 13-23%, respectively. The South Fork of the Sprague River's contributions to loads at the Sprague River outlet could not be estimated for this period because of a lack of nearby daily streamflow data to develop total monthly nutrient loads." [Note that Fig. 2 in this paragraph will refer to Fig. 1 in the original discussion paper]

Referee: 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).

Response:

We appreciate the referee's point about evaluating model performance under different flow conditions.

To clarify, the model was calibrated and validated using monthly mean daily flow and total monthly nutrient and sediment loads, and the statistics we report are on a monthly, not an annual basis (this is noted in the caption of Table 4 and in the text of the results). Peak discharge in the Sprague River and its tributaries occurs over approximately one month in spring, with discharge relatively low in both winter and summer through fall. Therefore, our use of mean monthly flow, while appearing to be a coarse scale, likely captures the seasonal variation in nutrient and sediment load. We acknowledge the value of assessing sub-monthly model performance for sediment and nutrients. However, our observed nutrient and sediment data are monthly loads estimated from bi-weekly water quality samples and daily flow data; therefore it is not possible to assess the sub-monthly model fit for the water quality constituents.

Our understanding of the rank probability score is that it requires a forecast probability for each data point (in this case, mean daily flow) in the simulated time series (e.g., Joliffe and Stephenson, 2012). As our model does not produce these forecast probabilities, we have calculated the percent bias (PBIAS) under the different flow classes.

We suggest adding the information below to clarify model performance:

P4942, L12: After "both the calibration and validation periods." Insert the following: "PBIAS between mean daily observed and simulated flow at the Sprague River outlet (site 1, Fig. 2) for the calibration period, 2001-2006 (validation period, 2007-2010) was 6% (22%) for the high flow class; -9% (-14%) for the moist class; 24% (-13%) for mid-range; 22% (-8%) for dry conditions; and -56% (-73%) for low-flow conditions." [Note that Fig. 2 in this paragraph will refer to Fig. 1 in the original discussion paper]

Referee: 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).

Response:

We thank the referee for pointing out this important aspect of the downscaled hydrologic model forcings.

Net radiation and relative humidity were derived from historical monthly statistics at nearby meteorological stations for both the historic period and future climate simulations, rather than the type of indexing method referred to by Pierce et al. (2013), which uses daily user-provided temperature and precipitation inputs (e.g., from downscaled GCM products for the future period).

To clarify this point in the original manuscript, we suggest revising the paper as described below. Note that wind speed in SWAT is used only when the Penman-Monteith method is selected to calculate potential evapotranspiration (PET). Since the Hargreaves PET method was used for all the SWAT models in this study, the revisions

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below remove references to wind speed.

P4934 L19-20: Revise the following sentence: “The SWAT weather generator was also used to generate solar radiation, relative humidity, and wind speed for the model.” Revise to: “Evapotranspiration was estimated using the Hargreaves method, which is calculated based solely on daily temperature inputs. The SWAT weather generator used to generate the other two climatic variables required by the model from historical monthly statistics at nearby meteorological stations: daily solar radiation and relative humidity.”

P4936 L4: After “http://reacchpna.org/thredds/reacch_climate_CMIP5_aggregated_catalog.html in February 2013.” Insert the following: “The SWAT weather generator was also used to generate daily solar radiation and relative humidity for the future period from historical monthly statistics, using the same methods described in Section 3.2.”

P4943 L5: Insert two new paragraphs as follows:

“In this study, solar radiation and relative humidity were derived from historical monthly statistics at nearby meteorological stations for both the historic period and future climate simulations. This could have resulted in some overestimate of relative humidity for the future period, as relative humidity in the western United States is projected to decrease by approximately 0.1-0.6% per decade, with higher rates in the interior U.S. and in spring and summer (Pierce et al., 2013). Future changes in cloud cover could also result in different solar radiation values from the historic values used in the model.

“We believe that the influence of solar radiation and relative humidity inputs in our model setup is likely relatively small. When the Hargreaves method for calculating potential evapotranspiration is used, as in this study, the only aspect of the SWAT model that may be affected by relative humidity is the vapor pressure deficit, which can influence plant growth (Neitsch et al., 2009). Vapor pressure deficit in SWAT is also governed by temperature inputs (drawn from downscaled GCM products for the future period in this study), and the rate of change in relative humidity from historic values is likely to be comparatively modest (Pierce et al., 2013). Similarly, when the Hargreaves method is used to calculate potential evapotranspiration, solar radiation inputs to the SWAT model affect only the total energy available to calculate potential plant and algal growth. Given inherent uncertainty in the model framework, any influence of relative humidity and solar radiation inputs on model results is likely modest."

In the reference section, add the following reference:

Pierce, D. W., Westerling, A. L. and Oyler, J: 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, 2010.

Referee: 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?

Response:

We appreciate the referee's comment, and agree that which underlying processes explain outlet nutrient loads (whether observed or simulated) is an intriguing question. The model does overestimate the Sprague River mainstem TP loads by 26% during the validation period; however, the model underestimates TP loads by 10% during the calibration period and so there is not a consistent direction of bias. As we have noted in our response to the first referee's comments, there is a larger measurement uncertainty in nutrient measurements than in flow measurements, which results in much larger acceptable PBIAS values for TP and TN. Taking this into consideration, we suggest that the apparent differences in PBIAS between the tributaries and mainstem be treated with some caution. It was not within the scope of this paper to perform uncertainty analysis for the hydrologic model, but we agree that it would be valuable to address this topic in future work.

Referee: 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.

Response:

We thank the reviewer for pointing out this wording. We suggest amending the original manuscript as follows:

P4928 L21-L22: Replace the following sentence: "The primary goal of the study was to assess vulnerability of stream water quality to future climate and wetland losses in this watershed." Replace with: "The primary goal of the study was to assess vulnerability of stream water quality to future climate and wetland losses in the Sprague River watershed, southern Oregon, United States. Wetlands in this snow melt-dominated, semi-arid watershed are believed to be an important non-point source pollutant control measure for downstream water quality (Boyd et al., 2002). However, the extent of wetlands and their role in basin water quality under changing climate is uncertain."

P4929, L1-L5: Remove the following sentence to avoid redundancy with the insertion on the previous page: "Our study area is the historically snow melt-dominated, semi-arid Sprague River watershed in southern Oregon, United States, where wetlands are believed to be an important non-point source pollutant control measure for downstream water quality (Boyd et al., 2002)."

References Joliffe, I. T., and D. B. Stephenson (2012), Forecast Verification: A Practitioner's Guide in Atmospheric Science, Second Ed., Wiley-Blackwell, Oxford, UK.

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(5), 1833–1850, doi:10.5194/hess-17-1833-2013.

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