Hydrology and
Earth System
Sciences
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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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Received and published: 31 May 2014

GENERAL COMMENTS

The goal of this manuscript is to examine the combined effects of climate change and wetland loss on stream water quality in a watershed located in the U.S. Pacific Northwest. The authors advance that such an assessment has not be done for the snowmelt-dominated regions of the Western U.S. and the combined (or cumulative) effects of climate change and wetland loss on the sediment and nutrient export dynamics prevailing in those regions have not been examined. I found the manuscript to be well articulated with clear research objectives and it should be of interest to the readership of HESS. It is also well written, of appropriate length, and figures and tables are of

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great quality. I have two general comments about the manuscript in its current form:

* As currently written, the manuscript reads a bit as a case study because of the sole or over-emphasis on Pacific Northwest hydrology. While this is OK, I think that most of the methods presented are generalizable and the authors should explicitly state (in their discussion or conclusion section, for instance) how their methodological framework can be applied elsewhere to resolve similar research questions.

* In addition, the authors should clarify that they are not referring to wetland loss due to man-made drainage but rather to climate-change-induced wetland loss (i.e., increased evapotranspiration leading to reduced water tables and riparian wetland inundation, wetland type conversion or wetland loss). So in fact, the novel aspect of this work is that it does not only consider the impacts of climate change through broad, regional metrics (such as delta T or % change precipitation) but it also considers local factors such as wetland area/extent.

Additional (specific) comments can be found below.

SPECIFIC COMMENTS

P4929 L2: What do the authors mean by "historically snowmelt-dominated"? While they provided ranges of annual precipitation, they did not estimate the percentage of snow versus rainfall. Also, they did not quantify total annual runoff and the portion of it that is attributed to snowmelt (rather than rainfall events).

P4930: The authors should provide more information about wetland coverage (absolute total wetland area or total wetland area as % of watershed area). Also, the third research objective of the manuscript refers to the position of wetlands in the landscape and at that stage of the introduction it was unclear (to me) whether "wetland position" had exactly the same meaning as "wetland hydrogeomorphic class". I was expecting the Study Area section to expand on this but it does not do so. The definitions of riparian and depressional wetlands are very briefly touched on in section 3.5.1 but I think

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that the wetland landscape position aspect warrants more explanation in the Study Area section. Also, Table 5 (much later in the manuscript) gives estimates of buffer area by Strahler stream order (defined for the SWAT modelling) but those are different from actual wetland areas.

P4933 L4-5: I would move the schematic of the hydrologic modeling framework and scenarios from the Supplement to the main manuscript as it gives a good overview of the work that has been done. Also, further to my previous comment, this diagram actually refers (in part) to wetland landscape position or wetland HGM class with the mention of riparian and depressional wetlands.

P4933 L15-9: I would add two references to that list, i.e., Wang et al., 2008 (Transactions of the ASABE) and Melles et al., 2010 (Proceedings of the International Environmental Modelling and Software Society). Also, the authors should mention that although all the papers they listed did use SWAT to model the impact of wetlands on flow and water quality dynamics, those papers did not represent wetlands in the same way as the authors. In the Wang papers (2008, 2010), notably, the treatment of depressional wetlands (potholes) is very different from the one used by the authors as the concept of HEW (hydrologic equivalent wetland) was introduced and used within SWAT. The authors might want to compare their representation of riparian versus depressional wetlands to the HEW concept later in their manuscript (discussion section).

P4939 L5-6: About the surface-area to volume equations available from the literature, there are many of those and the authors should mention the ones they used and cite relevant source papers.

Section 3.5.2: The authors should further explain their rationale for selecting wetland loss scenarios. As per one of my comments above, the authors are not interested in wetlands lost due to anthropogenic activity (or are they?) and rather want to target those wetlands that would be lost because of (climate-change-induced) changes in their water balance. Under a warmer-drier climate, it is probably reasonable to hypoth-

esize that depressional and/or groundwater-fed wetlands will be lost first, followed by riparian wetlands adjacent to headwater streams, then those adjacent to higher-order streams in extreme cases. However I am not sure that the temporal "loss sequence" would be exactly the same under a warmer-wetter climate... (?). Besides, the authors did not address wetland type conversion under climate change and one could argue that the 30 m buffer criterion used to define riparian wetlands might be too large under a warmer-drier climate. Regardless, the authors should just clarify the motivation behind their choice of wetland loss scenarios.

P4940 L6-7: Some of those thresholds are really high; is a simulation really acceptable when we get a % bias of +65% or -65% between observed and predicted nutrient concentrations? Especially at the monthly time-scale where all the event-driven short-term variability is smoothed out?

P4940 L13-9: While I agree that model performance criteria are generally less strict for validation periods, a threshold of 0.2 for NS is very low... That equates to a model performance that is barely better than using the mean of the observations.

P4944 L17-24: While the authors found that total wetland loss increased average annual TP by 58% under the "warmer-drier" scenario and by 97 % under the "warmer-wetter" scenario, these results should probably be interpreted with caution. Indeed, the authors wrote on P4929 L27-28 that the soils of the region are highly permeable and naturally P-rich: hence any increase in P loading under climate change could be due to either 1) wetland loss, leading to the nutrient sink function that cannot be performed to the same extent as before the loss, or 2) newly dominant subsurface flow processes (climate-induced shift in dominant flow paths?) that mobilize the naturally present soil P from areas proximal to the stream during non-flood periods. I am concerned that the authors were not able to differentiate those mechanisms in their modelling framework because riparian areas do not affect the model's hydrology (as written on P4944 L4). It would be worth expanding on/clarifying this.

P4946 L9-10: I would have the same cautionary note as above until the authors can confirm that their model is also taking into account the influence (or lack thereof) of natural soil P.

FIGURES AND TABLES

Figure 1: From the figure and the caption alone, it is not straightforward to figure out what the legend item "Irrigated" refers to. From the text, I am assuming this is irrigated cattle pasture?

Table 1: Text explanations are lacking to support the choice of the 0.58 value for the fraction of irrigation applied to HRU that leaves as surface runoff. Also, I am not sure I understand the "efficiency fraction parameter accounting for losses between irrigation source and applied location" correctly: if it is set to a value of 1, does that mean that all water is lost?

GRAMMAR AND SPELLING

P4932 L9: hydrologic response unit –> hydrologic response units