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Hydrology and Earth System Sciences Discussions

Interactive comment on "Impacts of impervious cover, water withdrawals, and climate change on river flows in the Conterminous US" *by* P. V. Caldwell et al.

P. V. Caldwell et al.

peter_caldwell@ncsu.edu

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We wish to thank the two reviewers and the editor whose comments improved the quality and scientific impact of this manuscript. We have formulated responses to submitted comments and revised the discussion paper accordingly.

Reviewer #1, Comment #1: The paper is well-structured, well-written, and addresses the topic which I believe should be of interest of HESS's readership. The authors are meticulous in describing the model and results. The figures are appropriate and helpful in understanding their results. Overall, this is a nice, solid manuscript with results that

may be used as a reference for future research.

Response: Thank you for the positive feedback on our work, and for providing insightful comments that will improve the quality of this manuscript.

Reviewer #1, Comment #2: Despite my overall positive comments, I have a few reservations. While I think that this is a good paper, it appears to be a good HYDROLOGY paper. Given that this is submitted to a special issue on ECOHYDROLOGY, I would have liked to see more analysis on the ecological aspects, but this is too much to ask at this point. I would therefore suggest that the authors better emphasize the ecological implications/connections of their results. The authors did a little of this in Discussion, but it should be expanded and elaborated more, including citing some more ecological references.

Here are some thoughts in this direction:

The results suggest considerable change in seasonal timing of flows in some watersheds. This can seriously affect the behavior (e.g., phenology) of aquatic organisms. Citing a few papers that show such effects would be nice. While the return flow rates from the thermopower sector are generally high, the returned water is of higher temperature, which could adversely affect aquatic life. Interbasin transfer projects are mentioned several times throughout the manuscript. Below is a recent work on the impacts of such projects on ecosystems (with focus on biodiversity):Grant, E.H.C., H.J. Lynch, R. Muneepeerakul, M. Arunachalam, I. Rodriguez-Iturbe, & W.F. Fagan.2012. Interbasin water transfer, riverine connectivity, and spatial controls on fish biodiversity. PLoS ONE 7(3): e34170. doi:10.1371/journal.pone.0034170.

I think these ecological perspectives would make the manuscript more balanced, and more suitable for this special issue.

Response: The term "ecohydrology" has been used loosely to describe both the relationships between terrestrial ecosystems and the water balance as well as to describe relationships between aquatic ecosystems and water quantity and quality. The work presented in this manuscript focused on the former, as did several of the submissions for this Ecohydrology special issue of HESS.

The objective of this special issue is to "address the dynamic interactions among climate, hydrology, vegetation, soil, and anthropogenic activities at watershed to regional scales" in particular focusing on: 1)New understanding of the consequences of anthropogenic activities (e.g. deforestation, water management) and climate change on water cycles, water quality and biogeochemical dynamics under various geographical and socioeconomical conditions, 2)The advances in new technology applications in ecohydrological research such as integrated simulation models, remote sensing, GIS, isotopes, eddy flux techniques, and 3) Case studies on the applications of ecohydrological principles in mitigating impacts of human disturbances and climate change on water resources.

We contend that this study aligns well with the objectives of the special issue and the science focus areas detailed above, offering a unique perspective evaluating the relative impacts of land cover, climate, and population change on water resources at the continental scale. In this study we investigated the potential impact of future land cover changes on river flows in the conterminous U.S., and we went on to compare the impact of these land cover changes to other global change drivers, namely climate change and population growth. Land conversion from vegetated ecosystems such as forests and grasslands to urban land uses with impervious cover has potential to significantly impact local watershed water balances; increasing surface runoff, decreasing evapotranspiration, and decreasing baseflow and groundwater recharge.

We agree with the reviewer that the global change induced changes in river flows may have significant impacts on aquatic ecosystems, however it was beyond the scope of this study to investigate those impacts and hydro-ecological linkages.

Reviewer #1, Comment #3: My impression is that the model results used for validation

assume no impervious cover and no water withdrawals, but this is not very clear in section 2.2. This should be clearly stated and briefly justified in section 2.2.

Response: We apologize this was not more clearly stated. While impervious cover was included in the model validation, water withdrawals were not. The intention of the model validation was to demonstrate the ability of the model to capture the temporal and spatial variability of the natural water balance, e.g. evapotranspiration, snow accumulation and melt, and runoff processes. We therefore selected watersheds from the USGS HCDN gauge network that spanned a wide range of climate regime and geographical location and that were not subject to significant flow regulation by dams and/or diversions to other watersheds. It would not have been appropriate to validate WaSSI in watersheds with dams or diversions because these are not currently represented in the model.

The model validations accounted for current levels of impervious cover as estimated in the 2006 National Land Cover Dataset (NLCD). This was mentioned in the modeling databases section (Section 2.4.3), but should have also been mentioned in Section 2.2. We have added a sentence to section 2.2 to emphasize this point.

It was not our intention to test the USGS water use estimates in the model validation of streamflow. The USGS water withdrawal data are the best available dataset for the entire conterminous U.S., and we assumed that these estimates were representative of actual conditions in each watershed. We did not feel it was appropriate to include withdrawals in the model validation because the county-level resolution of the water use estimates are rather coarse for site-level evaluation and the water intakes and outfalls are not referenced to specific locations on river reaches. As a result we do not know whether a streamflow gauge is upstream of all water intakes in the upstream watershed (no withdrawal impact on streamflow), downstream of the intake but upstream of the outfall (maximum withdrawal impact on streamflow), or downstream of both the intakes and the outfalls (streamflow impacted by consumptive water use). The latter scenario is what we assumed for the national scale, 8-digit HUC watershed simulations, which we feel is appropriate when working at a larger scale. We have added a summary of this discussion in section 2.2 to justify not including water withdrawals in the model validation.

Reviewer #1, Comment #4: I'm guessing that "mm" on page 4280, line 23, is a typo.

Response: Yes, that is correct. This error has been corrected in the revised manuscript.

Reviewer #2, Comment #1: This is an interesting topic, particularly in the context of water resources assessment, planning and management. The manuscript was well written compared to most manuscripts I have reviewed.

Response: Thank you for your support of our work and its potential use for water resource management. Your thoughtful comments will improve the quality of the manuscript and make it more understandable for international readers.

Reviewer #2, Comment #2: Page 4266, line 27, change "a diverse domain" to "diverse domains".

Response: Corrected in the revised manuscript.

Reviewer #2, Comment #3: Page 4271, line 24: a comma is needed between "et al." and "2009".

Response: Corrected in the revised manuscript.

Reviewer #2, Comment #4: Page 4272, line 6: "have" should be "has".

Response: Corrected in the revised manuscript.

Reviewer #2, Comment #5: Page 4298, Fig. 2: consider adding sentence indicating numbers from 1 to 18 correspond to 18 water resource regions in the Conterminous US.

Response: Added sentence to figure caption "Numbers 01 through 18 identify locations of Water Resource Regions."

Reviewer #2, Comment #6: Page 4279, line 5: should "2010 water withdrawals" be "2005 water withdrawals"?

Response: We agree that this is confusing in the manuscript. Because 2010 water withdrawal data are not yet available, we assumed that 2010 withdrawals for all sectors remained at 2005 levels. To clarify this, we revised section 2.4.4 and replaced all references to "2010 withdrawals" with "2005 withdrawals."

Reviewer #2, Comment #7: Using 10 validation sites, the WaSSI model was validated. The bias greater than 20% is 40%(i.e., 4 of the 10 sites), which is kind of high. Explanations should be provided what would be the major reasons for which the model calibration was not conducted?

Response: The reviewer is correct, the bias in mean annual runoff at some of the validation sites could likely be significantly reduced by adjusting model parameters to minimize model error in an intensive calibration process. However, it was not our intention to precisely match observations and predictions at these sites, rather to demonstrate the ability of the model to capture broad spatial and temporal variability in runoff across broad regions such as the conterminous U.S. We built our model to include the key ecohydrological processes that affect the water balance with off-the-shelf input datasets while having an acceptable level of predictive performance across the conterminous U.S. without calibration. In doing so, the model is more robust when expanding the model domain to other basins not included in the model calibration process and using the model to assess the impact of climate or land cover scenarios outside of the conditions for which the model is calibrated. We have added this discussion to section 2.2 of the revised manuscript.

Reviewer #2, Comment #8: Explanations should be provided on why two periods 1981-2000 and 2041-2060 were selected to be compared.

Response: The objective of this study was to quantify the long-term mean impacts of changes in climate, land use, and water use on stream flow around the middle of the 21st century as compared to a baseline historical time period, under the High and Low emission and growth scenarios. Twenty-year time periods for both was considered to be sufficiently long to isolate the long-term impacts with minimal influence from extreme events that may significantly influence results when evaluating a shorter time period. The 1981-2000 time period was selected as the baseline because both the A2 and B1 realizations of the CM2.0 general circulation model were run with the same 20C3M atmospheric greenhouse gas concentrations prior to 2000 and thus had identical monthly time-series of PPT and TEMP estimates with which to provide a common baseline. While a more recent time period such as 1991-2010 may be more representative of current conditions and align more closely to the water withdrawal and land cover inputs, the A1 and B2 realizations of the CM2.0 PPT and TEMP estimates already diverge by 2010 so there is not a common baseline. The 2041-2060 future time period was selected because it brackets the year 2050, mid-21st century.

We added the above rationale to section 2.3 in the revised manuscript where the future scenarios are discussed.

Reviewer #2, Comment #9: Consider introducing the concepts of water resource region and the 8-digit hydrologic unit and the association as part of the background information. It is probably common knowledge for hydrologists in the United States, but not so for international readers. If space allows, suggest a summary table providing the 18 WRRs and their associated information (name, drainage area, PPT, ET, etc.), which would support the discussions in section 3.2.1.

Response: This is an excellent suggestion, and we agree, will help international readers become acquainted with the Hydrologic Unit Code system. We added a description of the Hydrologic Units of the United States in section 2.1, and added a new table to the revised manuscript, Table 1, that summarizes the water resource regions by name, size, number of HUC watersheds, and mean annual PPT, PET, and actual ET.

Reviewer #2, Comment #10: It is confusing about surface water withdrawals. In sec-

tion 2.4.4, it was discussed the water use was remained at the 2005 levels from 2010 to 2060. However, in section 3.3, “2010 water withdrawals” was used. Some explanations may be considered to address the difference between the 2005 and 2010 water withdrawals?

Response: We agree that this is confusing in the manuscript. We provided clarification as described in our response to Comment #6 above.

Editor, Comment #1: Thanks for submitting your work to our HESS special issue. I received review reports from two experts. Overall, both of them are positive and offer constructive comments. Based on these and my own reading, I will be pleased to accept this manuscript for publication if you could consider the review comments and revise your manuscript accordingly.

Response: Thank you for your support of our work.

Editor, Comment #2: I think reviewer#1’s suggestion about adding ecological perspectives is important and relevant to the topic of this special issue.

Response: Please refer to our response to Reviewer #1, Comment #2.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 4263, 2012.