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Interactive comment

Interactive comment on "Influence of multi-decadal land use, irrigation practices and climate on riparian corridors across the Upper Missouri River Headwaters Basin, Montana" *by* Melanie K. Vanderhoof et al.

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Response to Comments

Manuscript: Influence of multi-decadal land use, irrigation practices and climate on riparian corridors across the Upper Missouri River Headwaters Basin, Montana

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Reviewer #1:

Summary Comment: I think this is a nice study. The authors used some clever methods to infer how changes in irrigation practices might be altering riparian zone wetness in semi-arid regions of the Missouri basin. They do a great job of synthesizing a large number of disparate datasets. The analyses are thoughtful, the results are interesting, and the discussion is comprehensive. The authors are careful to note caveats and do not make statements that outstrip the evidence. The manuscript would have been much stronger if the authors had shown how center-pivot irrigation trends changed over time, rather than just using the two endpoints in the analysis. Then the authors could have used a joint model that included climate and land use, rather than this two step, regression-on-residuals approach. I have some philosophical issues with doing regressions on residuals, especially when the explained variation from the climate model varies widely between basins. Doing this would require rewriting the whole paper, though, and I don't think this is a fatal flaw by any means. I have some questions and minor quibbles that I hope the authors can address in revisions. I recommend minor revisions and look forward to seeing the responses of the authors. -Richard Marinos Response: We appreciate the supportive comments provided by Richard Marinos. We agree that the analysis would be stronger if we had spatially explicit, annual data on irrigation methods and abundance. Because the analysis involved a large number of datasets, generating an additional 30 years of agriculture data was beyond scope. However, we hope that the findings presented in the analysis provide motivation either for our research group or for others to generate more agricultural datasets that include data on irrigation type. We have addressed all questions and quibbles below.

Line Comments: Lines 81, 92, 111: Minor stylistic point; you lead each paragraph with qualifiers (e.g. "Although. . .") which can obscure the main thrust of the paragraph. Response: We have removed the term "although" from the start of paragraphs as recommended.

Line 135: "Our research questions included". . . could you list all the research questions that this paper includes? Else, just say that these were your two questions. Re-

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sponse: We have revised this phrase to clarify that those were our 2 research questions.

Figure 2: Did you derive these P and VPD data yourself using the PRISM model, or are these available data products that you used? If the former, please include this in the results of your paper, not the methods. Response: We did not derive these variables ourselves. The P and VPD data were from the PRISM model dataset as specified at the start of section 2.4.

Line 183: It seems to me that this approach, only looking at the riparian vegetation that persisted during the study period, introduces an issue of survivorship bias. Can you justify this choice further in light of this critique? Response: I think what this comment is getting at, is that if a reach had experienced a severe drying trend then riparian vegetation may have transitioned to non-riparian vegetation (e.g., grassland) which would then be missed by the analysis. We focused on persistent riparian vegetation for two reasons. First, evaluating temporal trends while changing the riparian extent from year to year introduces the possibility of conflating temporal change with spatial change. Second, agriculture tends to be immediately adjacent to, and particularly further from the outlet, is often in the riparian area. Focusing on persistent wetland vegetation allowed us to avoid areas within riparian areas that went in and out of active agricultural activity. To address this comment we added the following sentence to section 2.2. “This approach enabled us to reduce uncertainty in the temporal analysis and increase our confidence in the vegetation type but limited our ability to detect changes in riparian extent induced by climate or changes in human land use.”

Line 185: Did you use the DEM to inform identification of riparian vs. upland vegetation? Did you exclude the active channel from your analyses? Response: A 30 m DEM was found to be inadequate to separate riparian from agricultural and upland vegetation, therefore we did not use it in the delineation. Yes, the active channel was excluded from the area of analyses. We have added a comment to that effect.

Line 190: Could you briefly expand on how you arrived at these specific reaches, either in comments or in the manuscript itself? It seems from the map that contiguous riparian areas cross the boundaries of your reaches. What distinguishes them as units of analysis? Response: We first used the confluences of rivers or the entrance of major tributaries to divide rivers into reaches. As the reaches were still quite long at this point, we then used the distribution of agriculture, which tended to occur in clusters along the major rivers, so that breaks between clusters of agriculture were used as further dividing points. Future work should focus on moving the analysis to a pixel-scale analysis, eliminating the need for deriving distinguishable reaches.

Line 228: I wonder how correlated cloud cover and higher NDWI values are, and if this would skew the analysis toward lower NDWI values. Though you did say that most P is as snowpack. Not really much to be done about this anyway, just musing. Response: It is an interesting thought! Yes, in this watershed the snowpack is the major driver of river discharge, therefore I suspect the influence of cloud cover would play a relatively minor role.

Lines 281-299: How well does this imagery analysis mesh with the cropland extent in the NLCD? Response: We did not compare the multiple sources of crop data with the NLCD. The NLCD provides land cover data only every 5 years and provides no specific data on crop type or irrigation method.

Figure 3: This was very helpful in understanding your data resolution with respect to riparian zone size. Response: Thank you.

Line 357: I am trying to work through the statistical implications of letting the input climatic variables for the random forests vary by reach. I would feel more confident if you could explain more why you took this approach, rather than using the same variables across reaches. Response: All reaches considered the same set of climate variables. Our goal with this decision was to find the “best fit” between the independent climate variables considered, and the dependent variable. Past efforts (e.g., Murphy et

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al., 2010) have found variable selection to improve random forest models. Ecologically, it makes sense that the best fit climate variables may change slightly as we move from snow pack mountains down to the Basin outlet. We also note that many of the climate variables were highly correlated with each other, so a statistical selection of one variable over another, may have modified the model very little.

Line 391: This CV approach seems strange to me, unless your datum was the lowest point in the HUC unit. Is this what you did? Otherwise a HUC unit at a mean elevation of 100 feet would have 10x the CV of the exact same HUC unit if it was transported to a mean elevation of 1000 feet. Response: The elevation coefficient of variation was calculated as the elevation standard deviation divided by the mean elevation, not as the mean elevation. As you can see in Table 7, we do not see a directional trend in the elevation coefficient of variation as we move up the watershed.

Line 417: Saying it's an uncertainty is an understatement! Ok but I see you've qualified your uses of this more in the following lines. Response: In addition to the qualifications, we added the word "major" to the phrase "point of uncertainty."

Table 5: Why is only March-June snowfall considered? Did I miss something? Methods general comment: Response: We considered both annual and spring snowfall. Both are listed in Table 3. In our analysis, spring snowfall consistently out-performed annual snowfall and was one of the best single predictors to represent annual climate and water availability for this Basin.

Comment: You present a LOT of results in your Methods section. I'd prefer to see these moved to the Results section. Response: We moved the supplementary agriculture statistics to the Discussion section and moved the 3 tables that contained results data to the Results section.

Figures 5 and 6: These are good figures that answered a lot of questions for me. Could you include as a supplement these plots for all reaches? I'd be interested to know what the "messier" reaches look like. Response: Providing all of the graphs for all plots

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would add a lot of extra pages! The key statistics for each reach are currently provided in Table 3. We have provided (attached) the graphs for our "messiest" reach (defined as the lowest random forest R2 (GR2) Gallatin River below. We hope that this adequate.

Line 518: I know you give this in Table 6, but could you provide absolute areal changes here too? It's hard to interpret these percentages without knowing absolute area as well. Response: We added the absolute areal change values.

Figure 8: Nice, love these pics. Response: Thanks!

Line 651: I am having a hard time understanding this point about cumulative effects. . . unless your ratio of recharge areas (e.g. mountains with snowpack) to withdrawal areas becomes smaller with basin size, in which case I could see how this could be the case. Response: We substantially shortened this paragraph to limit the discussion of cumulative effects. We did retain the sentences explaining the need to look at impact of upstream changes and conditions on the downstream reach of interest.

Line 688: Appreciate this strong caveat. Response: Thank you.

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Graphs for our “messiest” reach (defined as the lowest random forest R^2 (GR2) Gallatin River.

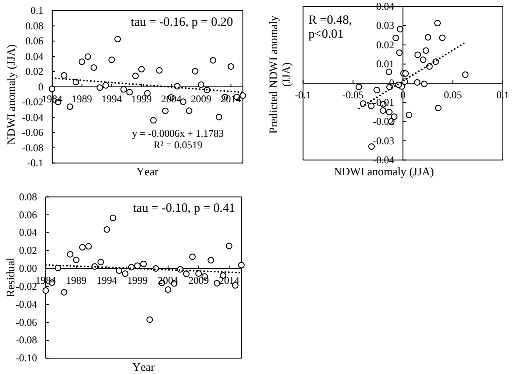

Fig. 1.

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