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Hydrology and Earth System Sciences
Discussions

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.

Richard marinos (Referee)

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Review submitted as PDF for nicer formatting.

Note to editors: It would be nice if this web form could have text formatting, other than LaTeX!

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 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. 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. 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. 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? Line 185: Did you use the DEM to inform identification of riparian vs. upland vegetation? Did you exclude the active channel from your analyses? 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? 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.

Lines 281-299: How well does this imagery analysis mesh with the cropland extent in the NLCD? Figure 3: This was very helpful in understanding your data resolution with respect to riparian zone size. 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. 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 CVof the exact same HUC unit if it was transported to a mean elevation of 1000 feet. 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. Table 5: Why is only March-June snowfall considered? Did I miss something? Methods general comment: You present a LOT of results in your Methods section. I'd prefer to see these moved 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. 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. Figure 8: Nice, love these pics. 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. Line 688: Appreciate this strong caveat.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-137/hess-2019-137-RC1-supplement.pdf

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