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Hydrology and Earth System Sciences Discussions

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Interactive comment

Interactive comment on "Human amplified changes in precipitation-runoff patterns in large river basins of the Midwestern United States" *by* Sara A. Kelly et al.

Anonymous Referee #3

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The authers use a variety of time series analysis techniques to investigate observed change in different aspects of the streamflow regime of four large river basins in the Midwestern US (in Minnesota, Wisconsin and Illinois), and to attempt to relate the observed changes to changes in precipitation magnitude and agricultural land use. The techniques and breadth of the analysis are strong, and the paper represents an interesting contribution to the on-going discussion of observed streamflow changes in the region.

I do have a couple of overall concerns. The potential impact of dams is mentioned in a few places in the manuscript, but there is no explicit mention of what streamflow gauges were used and which were affected by large dams and why these were included. I do

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not see how this type of analysis (looking at changes in high and low flow dynamics in river basins) can possibly be valid if using stream gauges from downstream of mid- to large size reservoirs. If gauges have been included that are affected by reservoir storage, then this represents a fundamental flaw and I think these gauges should be removed. If the gauges have not been impacted by dams, this should be clarified.

Overall, I would like to see more discussion and interpretation. In my opinion, the over-simplification of comparing annual precipitation and streamflow trends as a method of trend attribution neglects understanding of the non-linear nature of streamflow generation and the complexities of climate impacts on hydrology. We know that in many locations precipitation intensity is increasing, and we know that if the same amount of precipitation falls in a shorter time period, the runoff ratio is likely to be higher, but nowhere in this paper do the authors acknowledge this fundamental relationship.

It is true that the authors have done quite a bit more than a simple comparison of annual streamflow trends, but I maintain that it is theoretically possible to have no change in monthly or annual precipitation magnitude and still have an increase in streamflow, with no land management changes. (Although yes, in some cases the response may be amplified by the presence of drainage.) There was also no discussion of the influence of other climatic changes, such as changes in snow and frost depth that can have strong controls on the strengthening of the semi-annual period streamflow response seen in Minnesota.

I also would like to see more of a discussion of physical mechanisms involved. One of the largest streamflow trends displayed is that of an increase in summer low flows, and it is implied that this is due to an increase in the intensification of agricultural drainage. Field observations of subsurface drainage from around the Corn Belt consistently show that the subsurface drains stop flowing in the late summer, during the summer low flow period. This is true even in Minnesota, where the drains tend to be deeper than in other parts of the Corn Belt. If the drains are not contributing to streamflow in these months, can they be contributing to an increase in summer low flows? I can see that in areas where surface ditches are dug so deeply that they are intercepting a greater proportion of regional groundwater flow there is the potential for sustained baseflow to streams in the late summer, but I think this would only be true in a few isolated cases, with very substantial main stem ditches.

I have included some more specific comments below, tied to specific locations in the text:

1. Section 4.4.1 agricultural land use is not the only thing different between these river basins - climate is also very different. In particular, the seasonal timing of subsurface drainage tends to be very different between Minnesota and Illinois, due to the influence of soil frost.

2. Page 2, line 1. define was is meant by streamflow for these percentage changes - average annual?

3. Section 2, general. I agree with previous comments that the paper is very long. In this section some of the detail regarding physiography and sediment generation could be removed, instead increasing discussion of differences in hydrologic or drainage regime in these basins.

4. Page 10, line 8; page 15, line 23. Some discussion of differences in both the extent and physical impact of surface and subsurface drainage would be useful - surface ditches are generally deeper, and the impact of surface drainage on things like peak flows has been much more clearly established.

5. Page 11, line 15. High flow days and extreme flow days are never defined. How are these calculated?

6. Page 11, line 18. I don't understand how the multiple gauges were combined into one metric time series, or why. Late in the manuscript it seems to indicate that some of these gauges are affected by dams (page 18, line 23; page 30, line 23). I do not see how the inclusion of gauges affected by dams can be justified in this analysis.

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7. Page 13, line 22. The Livneh simulations used static vegetation, so there is no crop transition in this dataset, given that you have attributed part of the transition in Q to changes in ET seasonality is this a problem?

8. Page 17, line 8. I think 10 - 21 years is more than slightly different.

9. Page 18, line 18. Was there a different statistical test done on this period (post-1995)? What date ranges were used?

10. Page 18, line 23. How were cyclicity and synchronicity defined/quantified?

11. Page 21, Figure 5. Could the increase in power associated with sub-annual periods indicate an increase in winter thawing/ winter flows, which may be exacerbated by drainage?

12. Page 23, Figure 7. This is somewhat of a general comment. I agree that the lack of change in the Chippewa River basin relative to the other basins is striking, but it must be acknowledged that this is not a true control, the climate and physiography are different, with the lowest observed precipitation change for this watershed. Figure 7 also illustrates that the kernel density of monthly streamflow for the CRB exhibits a distinct shape from that of the other basins for the "pre-" period. It seems that it might look more like the "post" period for the other watersheds?

13. Section 3.6 and 4.4, hydrologic budgets, general. I think at its best, the hydrologic budget analysis can identify if climate is responsible for trends in mean annual streamflow, not all of the streamflow metrics presented, and I would like to see this clarified. The annual change in soil moisture may be completely different from the short-term changes in watershed storage responsible for peak runoff generation. Overall, I think the uncertainty in the ET, and comparison to Ameriflux has rendered this part of the analysis inconclusive, and this might be a good section to target for removal.

14. Page 26. The hydrologic budget analysis starts with an assumption that ET is stationary. I can't recall that ET trend results were ever reported. I think that the best

use of the Livneh dataset is to evaluate any climatological trends in ET, which you have not emphasized here.

15. Page 26, line 27. I think it would be helpful to present the cumulative change in storage calculated for the pre- and post- periods. I am a little unclear over what periods were used here, but I was trying to see if it is even feasible for such changes to result from drainage – a net reduction in storage in the MRB of -2.7 cm/yr, over a 40 year period, so 108 cm of water lost from storage. With drain depths on the order of 3-4 ft, and a porosity of maybe .47 there is only about 57 cm of water in the soil above the drain to be removed, and with only 45% of the watershed drained subsurface drainage could only explain maybe 26 cm of this decrease?

16. Page 27, line 1. I question whether the 5% bias in the Livneh ET relative to the Ameriflux site would systematically affect the time series. I don't know what vegetation was simulated for the Ameriflux site, but looking at the scale of the entire watershed, the static vegetation of the Livneh dataset would capture the dominant corn/soybeah rotation circa 1990, so going back in time it would like underestimate the ET associated with the greater cover of perennial vegetation.

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