Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-571-RC2, 2016

Hydrology and Earth System Sciences
Discussions

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.

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Received and published: 20 December 2016

This paper presents a relatively thorough analysis of historical trends in precipitation and runoff in four large river basins in the Upper Midwest, United States. The main question addressed in the paper is whether the observed increases in runoff in this region are due to increases in precipitation, or due to increases in the efficiency of drainage caused by expansion of agricultural drainage networks and tile drainage systems. The instrumental records of streamflow and precipitation in this region are quite good, with continuous records of discharge and precipitation dating back to the early 20th century at hundreds of locations. The data sets that allow for historical reconstruction of land cover are also quite good. A comparative analysis of hydrologic trends in

four river basins with different land use/land cover is used to assess the joint roles of precipitation and artificial drainage on increased streamflow.

The results of the analysis suggest that in the three basins that have experienced the greatest change in land cover, increases in annual streamflow are disproportionately higher than changes in annual precipitation; in the fourth basin where changes in land cover have been minimal, there has been no change in discharge over time. Taken together, the results indicate that modifications to land cover and land use in the Upper Midwest have had widespread impacts on the hydrology of the region. This topic has generated much discussion over the last 10 years, and the present paper will certainly help guide future efforts to understand the effects of land cover change.

I have several comments and suggestions for the authors to consider:

1. I suggest adding a brief discussion of the rationale for using PRISM 'data' to evaluate trends in precipitation rather than station data. Presumably, there are dozens of weather stations in each basin, and it wouldn't be too hard to utilize these observations, unless there are significant gaps in the records. Weather station data would also be useful for evaluating changes in temperature, which might have as much influence on ET as changes in land cover. It's not necessary to re-do the analysis, but please consider adding a comment.

2. The 17% adjustment in ET seems unnecessary and it just makes for a more complicated discussion of the results. Livneh (2013) suggested this value based on observations from four Ameriflux sites scattered across the entire US. One of those sites is in the study area. That same site is referenced in Table 2 (Brookings, SD), but here the discrepancy between modeled and observed values is $\sim$30%. At the three other sites listed in Table 2, the discrepancy is opposite in sign (+) and perhaps as little as 5%. Estimates of ET are used in this paper primarily to estimate the storage term, dS/dt, and the same conclusions about storage can be made without a 17% adjustment to a model-derived number.

3. The paper could be shorter. Some of the figures and associated text could be eliminated, e.g. Figure 5, Figure 7, Figure 9. Also, consider whether it is really necessary to consider 7 different flow metrics, e.g. 7-day low flow winter, high flow days versus extreme flow days (what is the difference between high flow days and extreme flow days?).

4. Minor points:

- p. 6, line 20, numbers don't seem to add up. - p. 8, how have reservoirs in the Chippewa River basin affected peak flows? - p. 13, line 3, remind the reader what LCT is; line 27, perhaps say that the storage term is sensitive to estimates of ET, rather than saying "we don't have high confidence. . ." - p. 15, line 2, eliminate 'grossly' - p. 18, line 8, changes in the IRB do not appear dramatic / systematic to me. - p. 19, line 8, where are the results discussed here, and on the next page, shown? - p. 20, Figure 5 is very difficult to interpret; same with Figure 7 - p. 24, Figure 8: This figure is very useful, could appear earlier - pp. 26-27 and Table 4. First, Table 4 is confusing with the different breakpoints and adjustments in ET. Is it really necessary to show results using the LCT breakpoints, since they are essentially the same as results for the 1974/1975 breakpoint? In addition the LCT breakpoints result in uneven record lengths, and in one case (CRB), the record length is only two years. I suggest using only the 1974/1975 breakpoint since that produces roughly equal intervals of time. Finally, p. 27, reverse the order of discussion of IRB and RRB (or reverse the values in the table). - p. 42, Table 1, change references to Livneh from L14 to L13.

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