Response to Anonymous Referee #2

We appreciate the thorough and thoughtful comments from Reviewer #2; our responses appear below, in italics.

Specific comments:

Page 6519, line 20: the author refers to ". . .grasses whose dense, deep root systems. . ." On the same page, lines 3-5, the author also refers to ". . .deep rooted vegetation such as trees." Seems slightly conflicting.

We apologize for the confusion; the term "deep" is relative here when discussing tallgrass prairie and forest in this region, as both native grasses and deciduous trees can be considered to have "deep" root systems (2 – 3 m for grass and 3 – 4.5 m for trees). In the context of below-ground ecohydrological processes, the actual depth of plant roots in flat, poorly drained regions which seasonally experience long periods of saturated soil conditions may be less important than the ability of grass roots to tolerate extended waterlogging better than tree roots. In light of this we have revised the earlier passage as follows:

"The most recently glaciated regions were dominated by flat or slightly depressed areas overlaying clayey deposits that collected spring precipitation and remained saturated well into the summer, thus preventing establishment of woody vegetation such as trees, which are less tolerant to prolonged waterlogging."

Page 6519, line 26: should read "nitrate-nitrogen."

We have made the suggested change to the final revised manuscript.

Page 6520: should mention the invention of the "clod-busting" plough (ala John Deere) which played a significant part in allowing the Midwestern prairie to be farmed (in addition to tile drainage).

We agree with the reviewer that plowing the prairie was extremely difficult due to a lack of proper technology capable of dealing with the dense prairie root systems, and that that point is worth making in the context of the story we are telling. We have therefore added mention of this to the beginning of the section on the expansion of agriculture in the region:

"With the rich soil now dry enough for crops and aided by the development of special "sod-busting" plows to cut through the dense prairie grass root systems (Bogue, 1994), by 1900 agriculture became the dominant land use in the region, and until about 1945, consisted of annual crops such as corn in rotation with sod-based crops such as oats or hay (Jackson, 2002)."

Page 6521, line 9: write out 2/3 as "two-thirds."

We have made the suggested change to the final revised manuscript.

Page 6523, line 26: should read, “. . .season; thus, in an effort. . .”

We have made the suggested change; thank you for pointing this out.

Page 6525, line 13: should read, “. . .in Kaskaskia, the more recent. . .”

Thank you for catching this – we have made the correction.

Page 6528, line 12: should read, “. . .July 15 through November 15. . .” Also, please discuss why this time period was selected (versus, say July 1 through November 1).

The change to the text will be made as the reviewer suggests.

This time period was selected after visual examination of the average daily flow for 1990 – 2011 for all gauge locations in both watersheds, and appeared to be when flow was lowest in this region. Because this study was done with biofuels land use change in mind, the time period was shifted forward into November to capture the low flow behavior and provide the ability to investigate possible changes in low flow behavior due to the longer growing season of Miscanthus.

Page 6528, line 14: streamflow drought periods?

We agree that this is a more clear choice of wording, and will make the necessary changes.

Page 6529, line 6: please discuss why the threshold (tb) of 3 days was chosen. Did you try different thresholds? Was 3 days recommended in Zelenhasic and Salvai (1987)?

Actually, the recommended interevent time threshold was 6 days, but the river in Zelenhasic and Salvai (1987) was much larger than the two rivers described in this paper. In our region, smaller reaches can respond quickly to large, localized precipitation events, resulting in a brief (1 – 3 days of $Q_{day} > Q_{threshold}$). Threshold interevent times of 1, 2, and 3 days were examined for a few gauge locations. Based on this analysis, if the interevent time was 3 days or more, the two deficit periods were considered separate events; if the interevent time was less than 3 days, the deficit periods were considered one event and the respective volumes were summed.

We have revised the manuscript to mention examining the low flow behavior at a few gauges in order to establish the threshold of 3 days.

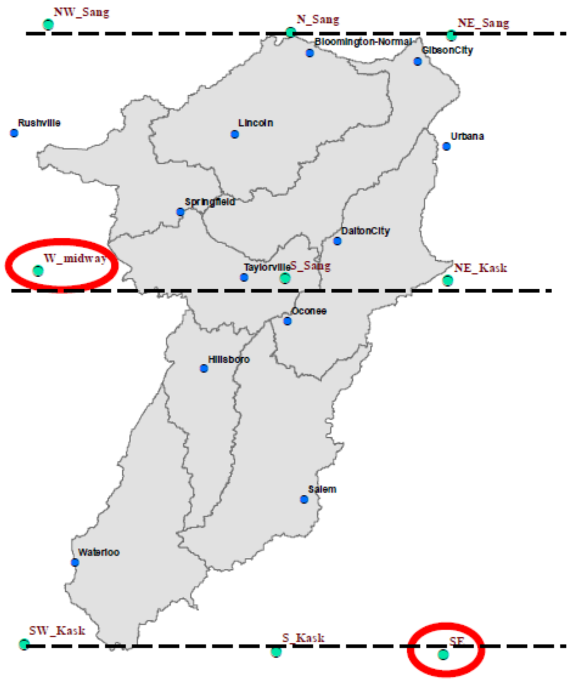
Page 6529, line 13: should read, “Lastly, to gain a quantitative. . .”

Thank you for catching this – we have made the correction.

Page 6529, line 15: the author wrote that 3 east-west transects are obtained from the PRISM dataset, but in Fig. 5, there seem to be 9? Or Fig. 5 refers to each cell in the dataset? Why discuss transects when you could just say you chose 9 cells that cover the two study watersheds?

“Transect” may have been a poor choice of words here, as both reviewers have expressed confusion over this description. On a geographical map, 3 locations were chosen, from western to eastern extents, at three latitudes representing the far northern and southern extents of the

two-watershed region, as well as an imaginary line across the middle, as in the figure below:



These points, representing the spatial extremes of the two watersheds, were then located on the PRISM map, and the data for the corresponding pixel of each point was downloaded and used in the analysis of spatial variability in precipitation. We have revised the description in the manuscript to use the reviewer's suggestion of referring to 9 points:

"Lastly, to gain a quantitative idea of the spatial and temporal variabilty of precipitation in this region, 9 points were selected along a bounding rectangle covering the spatial extents of the two watersheds; from west to east, 3 points were chosen across the northernmost latitude, 3 across the southern, and 3 across the divide between the two watersheds. Annual and monthly precipitation data was obtained from the PRISM dataset (Table 1) at each of these locations…"

Page 6529, line 22: be consistent with the word "inter-annual" or "interannual."

We will replace "interannual" with "inter-annual" in the revised document.

Page 6531, lines 1-2: note which ones are the "Kaskaskia tributary streams and mainstem reaches above Lake Shelbyville" for the ease of the reader.

We will refer to "(Fig. 8, dashed lines)" for the tributaries and "Atwood and Cooks Mills" for the mainstem reaches above Lake Shelbyville. We have also revised the passage in question so that it more clearly presents the results, and thank the reviewer for calling our attention to this.

Page 6531, line 6: should read, "from May to June and sharp decline from June to July" or something of similar.

We have made the suggested change to the final revised manuscript.

Page 6531, line 7 should read, “Thus, three distinct. . .”

Thank you for catching this – we have made the correction.

Page 6531, lines 8-9: should read, “row-cropped” and “tile-drained.”

We have made the suggested change to the final revised manuscript.

Page 6531, lines 11-12: the phrase “with the time element removed” is confusing, as time is still a factor (“At the daily scale”). I understand you mean the “linear” time factor is removed when producing the exceedance plots, but it sounds odd when stated this way. I would recommend just removing “with the time element removed.”

As the reviewer correctly pointed out, the confusing phrase was an attempt to emphasize the differences between the RC and the FDC regarding linear time. We have removed the phrase as suggested; however, we do wish to differentiate the daily scale streamflow patterns one might see in a daily hydrograph from the daily streamflow patterns expressed by the FDC, and so have revised the passage as follows:

“For the monthly RC, the streamflow patterns for the Sangamon were much less variable than those for Kaskaskia; for the daily FDC, however, the reverse is true, especially for the low flows (Fig. 9).”

Page 6531, line 16: should read, “. . .5% of the time, while upstream. . .”

We have made the suggested change to the final revised manuscript.

Page 6531, line 23: should read, “Also as seen in the regimen curve, the reservoirs’ influence. . .”

We have made the suggested change; thank you for pointing this out.

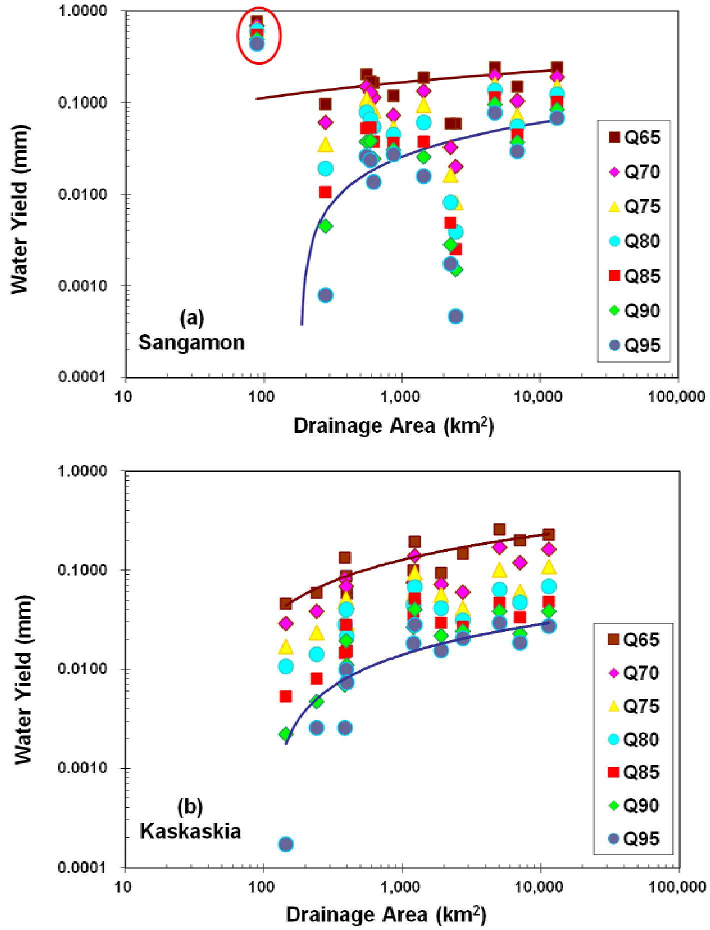
Page 6532, line 1: should read, “It is also interesting. . .”

We have made the suggested change to the final revised manuscript.

Page 6532, lines 14-16: The lower than expected values for Decatur are because of Lake Decatur, correct? Similar to what was seen at Rochester? So why was this pattern not expected?

“Expected” here referred to an increase in water yield with drainage area, based on regression analysis, and illustrated by the two lines representing Q65 and Q95, in the revised version of Fig. 10, below. The Decatur gauge is immediately downstream of the uncontrolled, inline dam, so the lower than “expected” flows, based on drainage area, are a result of Lake Decatur and diversion of water to the City of Decatur as explained on page 6531. At the Rochester gauge, in the S. Fork Sangamon subwatershed, there are two reservoirs further upstream, one on a

tributary and one on the S. Fork; in addition, Lake Springfield may occasionally laterally divert water from another S Fork tributary, just upstream of this gauge.



We have revised the passage in question to make our meaning more clear:

"Furthermore, based on regression analysis for Q_{65} (Fig 10, brown lines) and Q_{95} (Fig. 10, blue lines), *we expect the magnitude of water yield to increase with an increase in drainage area; however, two mid-size subwatersheds (Decatur and Rochester) have water yield magnitudes similar to a subwatershed an order of magnitude smaller in size (Spring Creek, in Lower Sangamon). From the FDC, we see that all three of these subwatersheds occasionally experience zero daily flow (Fig. 9). For Spring Creek, this is likely due to the small drainage area (< 100 km^2), but for Decatur and Rochester, this is more likely due to municipal and industrial water supply diversions."*

Page 6533, lines 6-8: should read, "Overall, average deficit volumes in the Kaskaskia watershed are fairly small (<0.2 mm), with an average time between deficits in most places of about a week, but varied average deficit durations. Some of the threshold flows. . ."

We have made the suggested change; thank you for pointing this out.

Page 6533: line 9: the phrase "as a consequence" is confusing. Please discuss how the very small threshold flows were a consequence of the previous sentence (and which part of the previous sentence?).

The ave. deficit volumes (not shown in Fig. 11, but calculated as part of the analysis) for Kaskaskia were small overall, but the $Q_{thresholds}$ *at some gauges in Kaskaskia were very small. Thus the largest relative deficits (ave. deficit volume/* $Q_{threshold}$*) were seen in Kaskaskia, because of the division by a very small number; the "as a consequence" refers to these very small* $Q_{thresholds}$*. We have changed the phrase to "as a consequence of the very small denominator…" to avoid possible confusion.*

Page 6534, line 25: should read, ". . .this region, which can be seen. . ."

We have made the suggested correction – thank you for bringing this to our attention.

Page 6534, line 26: should read, "At smaller time scales, this spatio-temporal variability can increase. . ."

We have made the suggested correction – thank you for bringing this to our attention.

Page 6535, lines 4-5: remove "removing nutrients from."

Thank you for catching this – we have made the correction.

Page 6535, lines 3-6: large reservoirs reduce flow variability on whichever stem they are located on, not just the main-stem (perhaps this is so in the Kaskaskia). Please clarify the language.

Yes, while in Kaskaskia the two large reservoirs are on the mainstem, we agree that the effect is not dependent on whether they are on the mainstem or a tributary. Since all three reservoirs mentioned in this paper are inline and on the mainstem, we have changed the sentence to read:

"Second, the storage effects of the large, inline, mainstem reservoirs play a role in the catchment response, especially in Kaskaskia, where they are instrumental in reducing nutrient output from the watershed as well as reducing flow variability."

Page 6525, line 11: should read, ". . .it may be the combined. . ."

Thank you for catching this – we have made the correction.

Page 6535, line 20: should read, "Thus, in the summer. . ."

We have made the suggested correction; thank you for bringing it to our attention.

Page 6536, line 23: should read, ". . .larger and/or longer. . ."

We have made the suggested change; thank you for pointing this out.

Page 6537, lines 8-9: the phrase, "some regions already had depleted water resources" is confusing. Please elaborate and describe how that influences the spatial variability of the results.

On re-reading the passage in question, it appears the influence of already depleted water resources referred to the spatial variability of the impacts of these results, not the results themselves, which only showed that ET increased and drainage decreased. The conclusion was that because of this decrease in soil moisture, in locations where water resources were already stressed, large-scale planting of Miscanthus could worsen the problem. We will revise this passage accordingly.

Page 6538, lines 12-15: please elaborate on the point that an increase in the fraction of Miscanthus will cause an increase in streamflow deficits and deficit durations and decrease in time between deficit periods. This is a very important point and should be well discussed (i.e., consequences of these increases/decreases).

Since field and modeling studies have shown that Miscanthus decreases soil moisture, and our analysis of these watersheds has shown that they are dependent on shallow groundwater for low flows, large scale planting of Miscanthus will make low flows lower. Thus, decreased soil moisture would indicate that it will take more precipitation (and therefore usually more time) to recharge the soils and increase stream flows. We will revise the manuscript to include this discussion.

Page 6538, line 16: should read, ". . .watersheds such as Sangamon. . ."

Thank you for catching this – we have made the correction.

Page 6538, lines 26-29: the statement beginning with, "Furthermore, the empirical analysis. . ." seems contradictory to one of the main points of this paper, which is (as in the title), "legacies of the past to inform the future." I think this statement is too strong, perhaps you can soften it a bit so as not to seem contradictory.

The next sentence does soften this somewhat, in that "understanding gained from such analysis [of the legacies of the past] can be used to inform the modeling process." It is not so much that we wish to use the past to directly predict the future, but that by understanding how the past changes are manifest the current response combined with the effects of watershed-scale variability in precipitation, we may better inform the modeling experiments that will be used to try to predict possible futures.

Page 6539, line 1: should read, ". . .modeling process; thus, both empirical and physically-based modeling analyses. . ."

We have made the suggested change; thank you for pointing this out.

Page 6539, lines 6-7: by “integrated systems model framework” do you mean a system dynamics model? What sort of human actions will you include in the model? If you could elaborate slightly (without tipping your hand too much), that would be great.

The framework would include a systems model to optimize the human system based on economic valuations and would be run interactively with a watershed hydrologic model. Human actions would be possible responses they might have to feedbacks from the hydrologic system, and may include selectively imposing minimum flow requirements, removing Miscanthus from sensitive areas, etc.

Figure 2: I can barely see the difference between the Sangamon and Kaskaskia watershed boundaries (left plot) and river (right plot). Please organize the HUCs by watershed (subwatersheds to Sangamon in one group and Kaskaskia in the other).

Figure 2 has been modified so that the HUCs are grouped by watershed on the right (detail) side of the figure. The left (locator) side of the figure shows only relative position within the US and the state of IL and provides a sense of how large these two watersheds are relative to the state and surrounding country. For this purpose each watershed is presented in a single color, with a corresponding scheme on the right. We hope this addresses the reviewer’s concerns about this figure.

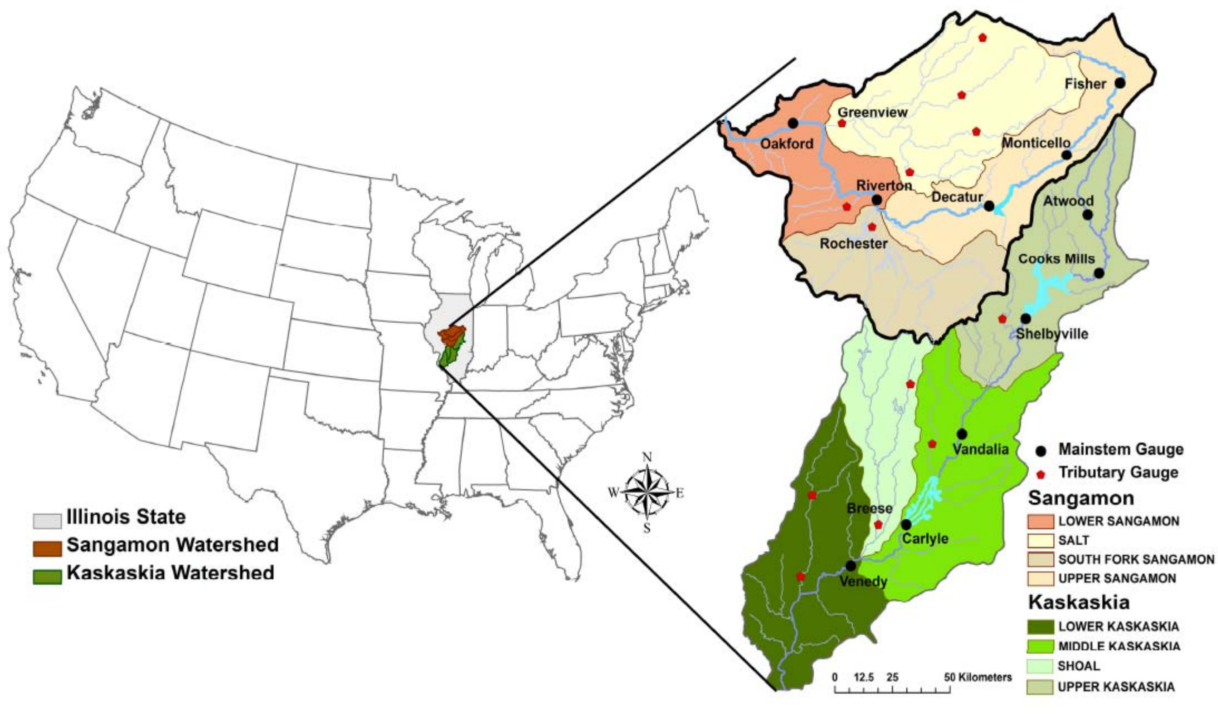


Figure 4: the labels for tb should be differentiated, as they represent two separate time periods.

Figure 4 has been modified accordingly, and the revised version appears below:

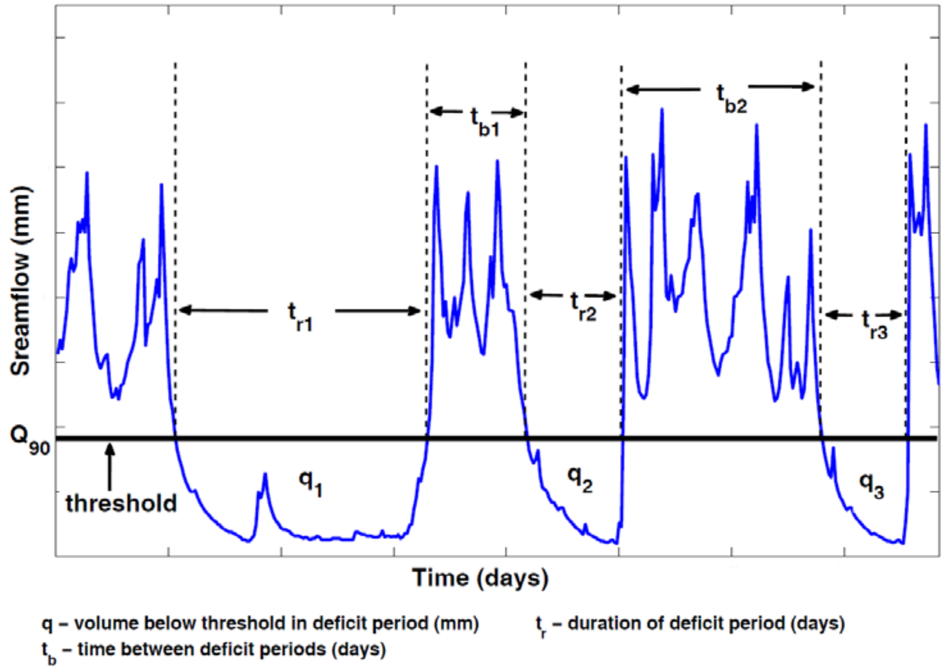


Figure 5: the x-axis is cut-off at the right side.

Figure 5 has been revised according to comments by Reviewer 1, and we have ensured the axes are clearly legible per the suggestion of Reviewer 2. The final figure appears below:

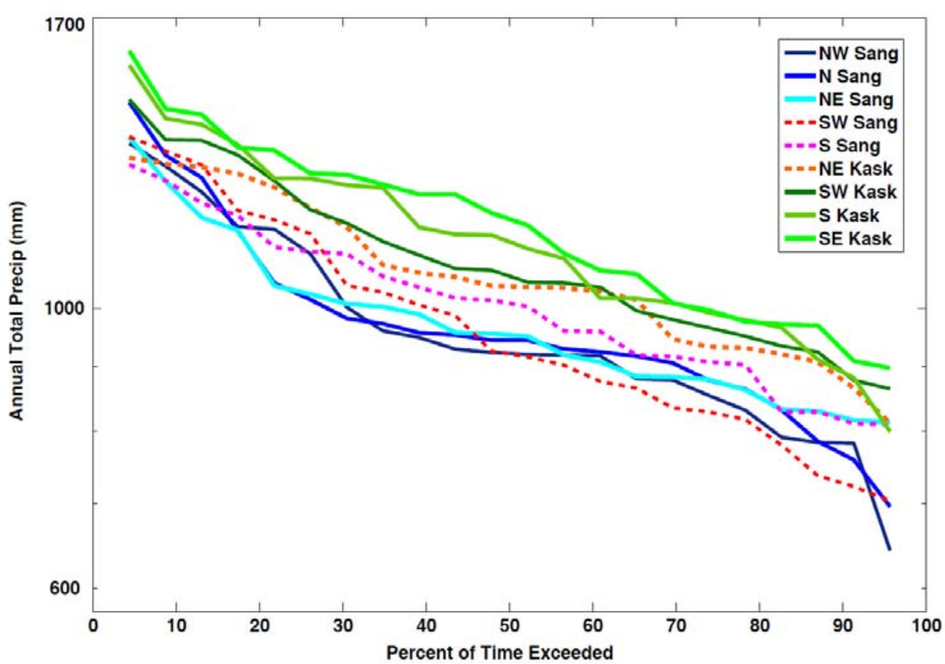


Figure 8: please use months (like Figure 7) instead of numbers. Then you can remove the x-axis title.

Figure 8 has been modified as suggested and appears below as it will in the final revision of the manuscript:

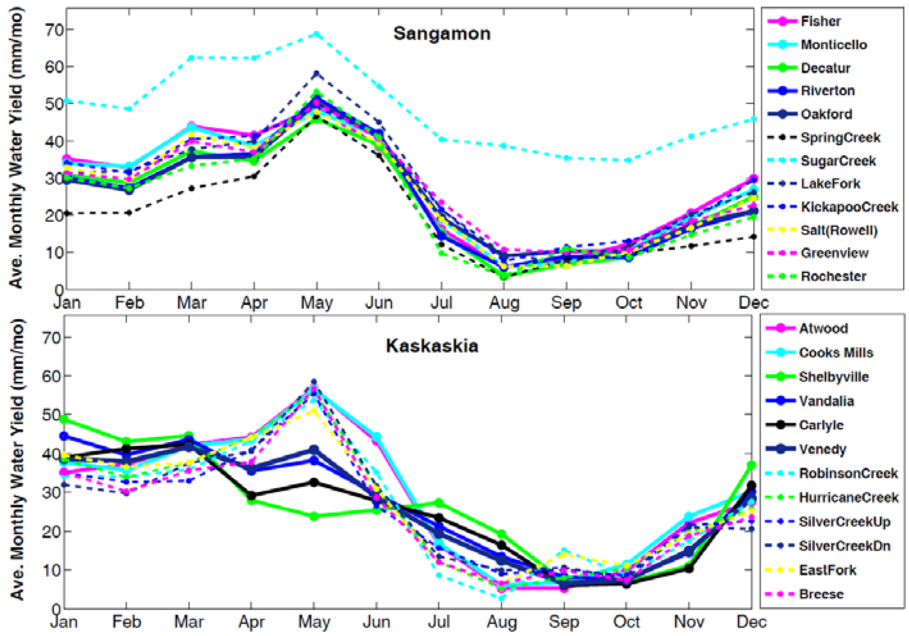
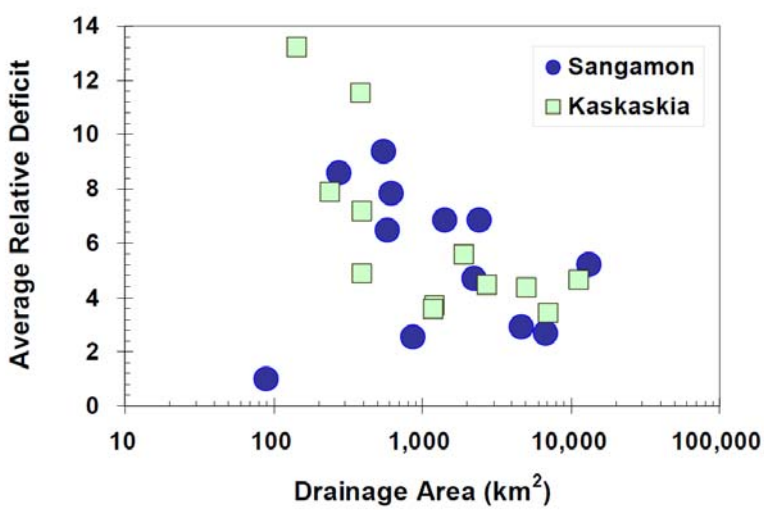


Figure 10: add the following to the caption. Note: the circled area in (a) is the Sugar Creek catchment.

Per a nearly identical comment from Reviewer 1, we will be modifying the caption, and will use the language suggested by Reviewer 2.

Figure 13: please remove the decimal places from the y-axis.

The figure has been modified as suggested and is shown below as it will appear in the revised manuscript:



General comment:

The scientific concept is valid and the analysis and is timely. However, I feel that for the amount of effort put into the work (which seems like quite a lot), the conclusions are too short and somewhat lacking. That is, more critical information can be extracted from the authors' work. Currently, a majority of the conclusions are drawn from previous peoples' work. Elaborating your conclusions would be a great way to solidify this paper. Otherwise, great job!

We agree with the reviewer's point about the content of the conclusions as they originally appeared, and we appreciate the suggestions for improvement, which we will implement. Per the comments of the first reviewer, who was very similarly critical of the original conclusions, we have already proposed extensive revisions to this section. Mainly, we will move the bulk of the Conclusions out of that section and place them in a revised Discussions section, where they will contribute to the discussion of possible catchment responses to new biofuel crops. The first paragraph of the Discussions section will then become the main portion of the Conclusions. For convenience, we have excerpted the proposed revision to the Conclusions from the Author Response to Reviewer #1 and presented it below; we hope this addresses the concerns of Reviewer #2 as well:

"Analyses of hydrologic signatures have revealed three main controls on the hydrologic response of these two watersheds: first, the precipitation inputs are spatially and temporally variable in this region and this area effect can be seen in both the annual and average monthly precipitation. At smaller time scales, this increases the heterogeneity of the catchment response due to the intensity of small convective storms common to this region. Second, the storage effects of the large, inline, mainstem reservoirs play a role in the catchment response, especially in Kaskaskia; there they are instrumental in reducing nitrate-N output from the watershed as well as reducing flow variability. Third, at the scale of this study, tile drainage plays an extensive role in the catchment response in both watersheds, especially in the Sangamon, where a higher proportion of the land is tile drained. These effects manifest as a homogenous regime curve, increased heterogeneity in the low flows, and a higher BI relative to less-tiled areas. However, because tile drainage in this region is also generally associated with intensive row-crop agriculture, it may the combined effect of these land modifications that is being observed.

Analysis of the histories of the case study catchments shows that impacts and modifications have been layered on top of each other through time: fire, prairie conversion, surface drainage, subsurface (tile) drainage, reservoirs, intensification of row cropping, erosion, fertilizer, etc. Spatially, this layering does not always follow watershed boundaries, but often human ones; resulting in the formation of a different mosaic of layers in each watershed. These mosaics of change, combined with the controls identified by analysis of the hydrologic signatures, is manifested in the summer low flow behavior, where greater variability in duration, frequency, and relative magnitude is associated with the deficit periods in Sangamon compared to those in Kaskaskia. These differences would affect the suitability of certain sub-watersheds for growing Miscanthus, and thus must be taken into account in future plans for biofuels expansion to avoid worsening or creating water stress conditions."