I would like to thank the authors for making a thorough effort to address the reviewers' comments and improve the manuscript. However, I still have some suggestions that could improve the manuscript so that it could be acceptable for publication in HESS. These are meant to be constructive comments that could help elevate the paper and make it even more citable.

MAJOR COMMENTS

The authors do not provide a critical evaluation of the model performance. Upon reading the original manuscript, I suggested that data be presented from a dry year. This was to demonstrate that the model could represent a condition that is drier. I want to thank the authors for now including 2009 as a sample of behaviour during a dry year, which is very interesting. However, perhaps I was not clear in my suggestion, because showing simulations from 2009 does not demonstrate how well the model performs because there are no observations against which to compare. There are data for the Beaverhill Creek at the mouth WSC gauge from 1975-1986. I strongly suggest the authors present data for a dry and wet validation year. 1983 was relatively wet, but 1984 and 1985 were dry and would provide improved context. If the data do not exist to do so, please say so in the manuscript.

In Section 3.3.3 the authors provide no sound evidence to support their supposition that there is no fast subsurface flow. If Beaverhill Creek is as representative as their regional data imply, then there should be fast subsurface flow in an effective transmission zone as documented in similar landscapes by:

van der Kamp G, Hayashi M. 2009. Groundwater-wetland ecosystem interaction in the semiarid glaciated plains of North America. Hydrogeology Journal 17: 203–214. DOI:10.1007/s10040-008-0367-1.

van der Kamp G, Hayashi M, Gallen D. 2003. Comparing the hydrology of grassed and cultivated catchments in the semi-arid Canadian prairies. Hydrological Processes 17: 559–575.

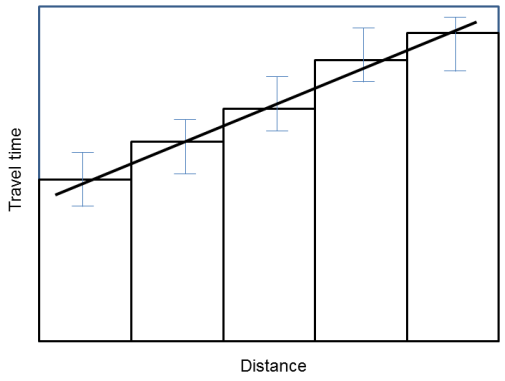
Brannen, R. C. Spence and A. Ireson, 2015. Influence of shallow groundwater-surface water interactions on the hydrological connectivity and water budget of a wetland complex, Hydrological Processes 29: 3862-3877.

Available observations in the Pothole Region do support high frequency of macropores and high shallow hydraulic conductivities. My theory is that this is merely an artifact of model parameterization. This is a problem with result interpretation that must be addressed.

In Section 3.4, the authors discuss the applicability of their results across the Prairie Pothole Region. As above, the authors need to critically describe and interpret the data. The values in the text of Section 3.4 are not the same as those in Table 3 (e.g., p-scores for air temperature median 0.05 in the text and 0.08 in the Table which suggests there is a difference in median temperatures). This is OK; Beaverhill is at the north end of the region. What is more interesting is the departure from the quantile-quantile plots that suggests that wetland distribution in North Dakota is more dense. That is more distant wetlands are relatively closer to one another in North Dakota than in Beaverhill. This is a great opportunity to explain what has been learnt in this study about connectivity length and time could mean

in a PPR landscape with slightly different wetland distribution. This could make for a better, and more citable paper, than what is in the current version.

In the discussion of the influence of distance on connectivity, the authors interpretation is not true to the data. First, the way the second sentence is constructed implies that Brannen et al (2016) showed that wetlands do exchange deep subsurface flow. This was not the case; Brannen et al showed that when the water table was in the shallow effective transmission zone groundwater can maintain the pond levels that sustain surface flow. Second, the scatter in Figure 8 shows that the likelihood that a wetland contributes to a river (or another wetland) increases with proximity, but distance is not an indicator of the volume that can be transmitted. There is clearly an envelope, and the scatter below it could be a function of travel time, and Figure 6 suggests the authors have the data to show there is a distribution. A figure such this below as would do nicely to complement Figure 8 because connectivity is not just about the volume or mass, but also the time timescale at which it is evaluated:



MINOR COMMENTS

Abstract: "Hydrologic connectivity among wetlands …."

Page 1 Line 22: "as these are small features vulnerable …."

Page 1 Line 30: "; this restriction allows GIW's to influence downslope resources (US-EPA, 2015) by enhancing flood ….."

Page 3 Line 15: "average July temperature …."

Page 7 Line 25: "Quantile-Quantile plots as a graphical …."

Page 7 Line 25: "Beaverhill Creek …."

Section 3.3.2. Perhaps it is just me but the content in this paragraph is a bit repetitive so it is hard to tell the difference between when the authors are speaking about wetland-river connections and wetland-wetland connections.

Section 3.3.2. It is a supposition that the model simplification that prevents flows from the groundwater scheme to the surface scheme had negligible effects. This is actually a known unknown, not something to be dismissed, especially in light of evidence from Brannen et al. that groundwater can be an important influence on maintaining surface storage and downstream flows (see above). A more honest way to approach this would be to say "It is not known if this simplification had negligible effects, but would influence the model simulations of the likelihood of surface connections from the moraine to the river (Figure 5)."

Section 3.3.4. Just a comment, but if connectivity is a function of the timescale at which it is investigated, are the wetlands in the moraine really connected to the river over the course of one warm season? Figure 8 implies the connectivity time is 10^7 days.

Page 11 Line 35: "transit time distributions of water flowing from wetlands to the North ….."

Page 12 Line 19: This is perhaps where in the manuscript a new figure like that above would be very helpful in determining the impact of removing a wetland because one could glean how long it should take water to get from that location to a river of interest.

Conclusions: If it is one-way, is it really coupled? No. It is linked. Maybe change this throughout.

Conclusions: I'm not convinced the authors proved that protection of wetlands based on distance can lead to loss of wetlands functions. Could I suggest the authors remove this last paragraph and replace it with the second one from the Guidelines section, and in that reword "Furthermore, …." to "We recommend coupling robust ….. data to (1) improve …."

Tables 1 and 2 might be able to be combined to save space.

Figure 6: These figures are misleading by using a log scale, and the axis labels are not well explained. The text embedded within is too small a font to be legible once it gets into a journal format. Maybe put that information in a table.

Figure 8: Can I suggest the authors standardize the x-axis scale on the three panels. It would really demonstrate the differences in the distances water is travelling.