Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-404-AC1, 2016

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Quantifying hydrologic connectivity of wetlands to surface water systems" *by* Ali A. Ameli and Irena F. Creed

Ali A. Ameli and Irena F. Creed

icreed@uwo.ca

Received and published: 7 November 2016

COMMENT: This paper covers a very timely topic and would be a nice addition to HESS. The concept of hydrological connectivity is still in its infancy, but its relevance to the wetland management is obvious, even as hydrologists are still learning how to apply the concept. The authors are to be commended on their efforts to advance the thinking on this subject. The study summarized in this paper applies a series of process based models to quantify surface and subsurface hydrologic connectivity among wetlands and a major river, in order to address several goals. These include assessing the performance of the models, comparing the relative importance of surface and subsurface connections, determining if proximity can be used as a substitute for connectivity, and if their findings could be extrapolated beyond the study watershed. The authors meet all these goals but only to different degrees, and I have provided

Printer-friendly version

Discussion paper

some suggestions that might elevate the study and manuscript. There are some major comments, and numerous minor ones.

RESPONSE: We appreciate the thorough review of the first reviewer as well as his/her feedback on the novelty and necessity of the current paper. This positive feedback encourages us to continue working on this poorly understood subject in the future.

MAJOR COMMENTS 1) Could the authors perhaps present data from the surface overland flow model for a dry year? I understand why they selected 2013, but it would be good to know that the model could represent a condition that is drier, and what those repercussions are for connectivity. One downside of the research as presented is, it does not necessarily present the spectrum of connectivity that could occur in the Beaverhill watershed.

RESPONSE: Yes, we can present data for years reflecting different hydrologic conditions. In the revised manuscript, we will model surface overland flow for both a wet year (2013) and a dry year (2009) so that a representative range in connectivity is presented.

2) A more critical assessment of the simulated surface flow hydrograph is needed. The high regression coefficient is likely because of the low flow period, and the spring peak, which is relatively well simulated. The true test of a modeled surface stream hydrograph in the Prairie Pothole Region is how well it represents the summer recession, any summer events, and timing of the cessation of streamflow. The model does not do this particularly well. The manuscript would be improved if the authors explain their theories as to why the model simulated an event that did not happen, and missed one that did. Could it be that the model missed some important re-connection? If so, why? This will help inform how the model is behaving and provide some great insight.

RESPONSE: We agree that the surface flow routing model did not perfectly predict the observed surface flow at the measurement station. There are several reasons for this. First, there was a lack of evapotranspiration data before 2000 including the calibration period used in our paper (April 1 - August 1 1983). We used the evapotranspiration data for 2015 for the same period (April 1 to August 1 2015), as the average monthly humidity, average monthly maximum air temperature and average monthly minimum air temperature were similar between April 1 to August 1 1983 and April 1 to August 1 2015. This could have affected the hydrograph shape including the earlier prediction of the second peak. We consider this discrepancy (earlier prediction of the second peak) to have minimal impact on the simulated connectivity map at the end of simulation period (e.g., Figure 6b), mainly because, at the measurement station, the cumulative simulated flow (2.4 107 m3) is only 7% less than the cumulative observed flow (2.6 107 m3) at the end of simulation period. Indeed, in our particle tracking scheme, it does not make a substantial difference if the particle is at its highest velocity on, for example, June 20 or June 24. We will add a paragraph to the text to explain the reason for this inconsistency in the simulated and observed hydrographs, and its effect on our conclusions.

3) I would argue that the authors misinterpret the content of Figure 9. There is good fit for short distances, but not long. Could the authors please provide more information on how the shortest distances were calculated? Are these Euclidian (ie “as the crow flies”) estimates? Or are they along the topographic flow path? Did they come from the digital elevation model? If this is the case, this might explain the departure from the linear function in Figure 9. If I interpret the results correctly, this highlights the problem with the variety of connectivity metrics, measures and indices that are currently used in hydrology. To really address their goal of determining if proximity is a substitute for connectivity, it would be great if the authors could output the contribution of flow from each wetland to the North Saskatchewan River, and plot these flows against distance. This would truly show if distance is (or is not) a proxy for connectivity. The authors do not use a metric that demonstrates the magnitude of connectivity, only its presence or absence. They need one for magnitude to answer their question if proximity can be used as a substitute for connectivity.

RESPONSE: We appreciate this concern and the great suggestion. In the revised version, we will calculate the contribution of flow from each wetland to the North Saskatchewan River, and plot these flows against the wetland distance to the river. We agree with the reviewer that this would show whether or not distance is a proxy for connectivity.

MINOR COMMENTS Some relevant work the authors should consider working into the manuscript are listed below. Shook, K., J.W. Pomeroy, C. Spence and L. Boychuk, 2013. Storage dynamics simulations in prairie wetlands hydrology models: evaluation and parameterization, Hydrological Processes 27: 1875 – 1889. 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. Hayashi, M., G. van der Kamp and D. Rosenberry, 2016. Hydrology of prairie wetlands: understanding the integrated surface-water and groundwater processes, Wetlands doi: 10.1007/s13157-016-0797-9

RESPONSE: We thank the reviewer for suggesting these references. These are relevant studies and we will refer to them in the revised manuscript.

Page 1 Line 22: Could read: " . . ... protection, as these are small features typically vulnerable to drainage or manipulation . . .. . ." As for the rest of the sentence, please provide information on why being numerous equates to a need for protection.

RESPONSE: We will revise the sentence as suggested.

Page 1 Line 25: Maybe reference Brannen et al. here too.

RESPONSE: It is a relevant reference, and we will refer to it in the revised manuscript.

Page 1 Line 26: I know that fill-and-spill has become common vernacular, but perhaps the authors could say ". . .. . . via mechanisms analogous to fill-and-spill runoff generation (Rains et al., 2006)."

RESPONSE: We concur with this suggestion. We will revise it.

Page 1 Line 29: Be very careful when using the term "function" because it has very specific meanings depending on the context. For instance, the hydrological function of a specific wetland using the hydrogeomorphic assessment method, which can be required for development works, follows methodologies necessary for the specific purpose of discerning a loss or gain in wetland function relative to a reference standard. This approach was designed to detect and measure variation in function due to human impacts, not natural variation. In contrast, Black (1997) proposed that landscape units have hydrologic functions such as collecting, storing and discharging. Could I suggest the authors explicitly define what they mean by "function"? Or, use the word to "role".

RESPONSE: We have a rich literature to support the use of the word "function" – which refers to the hydrologic functions such as "collecting, storing, and discharging" water, and will both define it and refer to key references that describe what we mean in the revised manuscript.

Page 2 Line 6: Perhaps instead of committing to a statement that an inability to quantify connectivity would lead to preferential protection to certain types of wetlands, maybe say " . . . .. may lead to incorrect or inappropriate management decisions regarding wetland removal, protection or reclamation."

RESPONSE: We concur with this suggestion. We will revise it.

Page 3 Line 11: remove italics here and throughout this section.

RESPONSE: We concur with this suggestion. We will revise it.

Page 3 Line 12: Maybe provide a URL for the climate data.

RESPONSE: We concur with this suggestion. We will revise it. Based on 40-year (1974-2014) climatic data collected at the Edmonton International Airport, the average January temperature is -13.5 °C and the average July temperatures is 15.9 °C (http://climate.weather.gc.ca/).

Page 3 Line 15: Maybe rephrase to: . . . ..although snowmelt can be an important to

Printer-friendly version

Discussion paper

runoff in the spring."

RESPONSE: We concur with this suggestion. We will revise it.

Page 3 Line 34: Do the authors mean the probability of depression existence or presence?

RESPONSE: We do indeed mean "probability of depression" in concurrence with our published technique on how to map the probability of wetlands.

For more details, see reference citations below: Lindsay JB, Creed IF, Beall FD. 2004. Drainage basin morphometrics for depressional landscapes. Water Resources Research 40: W09307. Lindsay JB, Creed IF. 2005. Removal of artefact depressions from digital elevation models: towards a minimum impact approach. Hydrological Processes 19: 3113-3126. Lindsay JB, Creed IF. 2006. Distinguishing actual and artefact depressions in digital elevation data: Approaches and Issues. Computational Geosciences 32: 1192-1204.

Page 3 Line 37: What are "integrated wetland features"?

RESPONSE: The wetland mapping technique sometimes detects wetlands fragments that then need to be integrated into a wetland object. We will revise the wording in the revised manuscript to improve clarity.

Page 4 Line 1: In recent years in the Prairie Pothole Region what would normally be considered GIWs had ponds that have been above their surface outlet elevations. Perhaps a sentence or two would be a good idea on how often a GIW needs to be not spilling in order to be considered a GIW.

RESPONSE: We concur with this suggestion. We will revise it.

Page 5 Line 33: Please explain why there is such a short calibration period. The gauge was open until 1986.

RESPONSE: We did not have access to the evapotranspiration data before 2000. April to August 1983 was selected as we were able to link its evapotranspiration to the one calculated during the same period in 2015. Please refer to the response to major comment 2 above for more detail. In addition, HydroGeoSphere is a very computationally expensive model and a longer simulation period would have required substantially more computational resources without adding too much information to our paper.

Page 6 Line 7: Just my preference, but more detail in the paper on the methods would be helpful for the reader, particularly the water particle tracking approach and how surface water velocities were approximated.

RESPONSE: We thank the reviewer for bringing this to our attention; we will add a few sentences to address the reviewer concerns.

Page 6 Line 22: Could I suggest the Hayashi paper I note above be worked into the context here? Hayashi and his co-authors present a new conceptual model of subsurface flow in the Prairie Pothole Region that is a major departure from the model of Toth that is the basis for the assumption that geographic proximity is an indicator of connectivity.

RESPONSE: The Hayashi paper is an extremely relevant reference. But we will refer to this work in the introduction of the revised manuscript, as our main focus in this sentence was to explain how we compared surface and subsurface connectivity.

Page 6 Line 29: Maybe rephrase to: ". . ..will be linear but not following y=x."

RESPONSE: Thank you for noting this. We will revise it.

Page 6 Line 31: Please rearrange this sentence.

RESPONSE: We will remove this sentence, based on the reviewer's earlier suggestion to implement an alternative approach to assess the effect of distance.

Results: The description of the results reads a bit terse. Sometimes the content seems little more than a figure caption. Could I suggest the authors provide more description on the results, particularly where the model does not work well.

RESPONSE: We thank the reviewer for this useful comment. We will add appropriate sentences to explain the results in more detail.

Figure 9: It is unclear where the North Dakota data are from. Could the authors provide this detail in the Methods section.

RESPONSE: We obtained wetland polygons in North Dakota from the National Wetlands Inventory (https://www.fws.gov/wetlands/) and stream polylines from the National Hydrography Dataset (http://nhd.usgs.gov/). We will add these references to the revised manuscript.

Page 9 Line 15: Maybe discuss within the context of the results of Shook et al.

RESPONSE: Good suggestion. We will include the conclusions of Shook et al. work here.

Page 9 Line 36: Figure 6 does not illustrate what is discussed here.

RESPONSE: We thank the reviewer for this concern. We feel that the comparison of Figures 6a and 6b shows that the number of subsurface connectivity lines is significantly larger than the number of surface connectivity lines.

Figure 10: The authors need a more explicit explanation of how they decided which services were associated with each portion of this curve.

RESPONSE: We agree with the reviewer that the implications of associating cumulative probability of travel time with functions requires explanation. The associations of functions with portions of the curve reflects the collective expert judgment of an international team of researchers as recently published in the Proceedings of the National Academy of Sciences of the United States of America (Cohen et al., 2015). We will revise the manuscript to make these associations clearer. Reference Cited: Cohen, M.J., Creed, I.F., Alexander, L., Basu, N.B., Calhoun, A.J., Craft, C., D'Amico, E.,

DeKeyser, E., Fowler, L., Golden, H.E. and Jawitz, J.W. (2016) Do geographically isolated wetlands influence landscape functions?. Proceedings of the National Academy of Sciences, 113(8), pp.1978-1986.

Conclusions: Just a comment, but even though most of the hydrology community knows that wetlands are not hydrologically isolated, I completely agree that it is good to make this point. RESPONSE: Thank you for noting this.

Table 1: Is the p value for magnesium correct? It seems small, especially in light of the content of Table 2.

RESPONSE: We confirm that the p-value is correct. Note that the p-value of the Wilcoxon rank sum test explores if the data in x and y are samples from continuous distributions with equal medians, against the alternative that they are not. So the difference between the absolute mean values cannot predict the p-values of the statistical tests.

Figure 2: The last word in the caption “time”, could be “period”.

RESPONSE: We concur with this suggestion. We will revise it.

Figure 3: Great figure.

RESPONSE: Thank you for your encouraging comment. This figure clearly shows that the new grid-free groundwater-surface water interaction method that is presented in this paper can appropriately and efficiently address multi-scale naturally complex systems. Solving this problem was very challenging with numerical models, such as HydroGeoSphere.

Figure 4: It is hard to see the wetlands in this. If this figure was created by clipping Figure 3 by a wetland layer, my suggestion is that you delete Figure 4 because it does not add too much information

RESPONSE: We concur with this suggestion. We will remove this figure.

Figure 10: Why is there a gap?

RESPONSE: The connection time to North Saskatchewan River cannot be continuous as there is a considerable difference between the time-scale of subsurface and surface connections. The surface connection time-scale is on the order of 102 days but the subsurface connection is on the order of 105 days. This gap also appears in Figure 7a (left panel).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-404, 2016.