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

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HGS-D General Comments:

It would be more appropriate and robust to use HGS as a fully coupled model than to represent groundwater with a steady state analytical solution. There is a large and growing body of literature demonstrating the application of fully integrated numerical models at the basin scale. The authors linking of a transient surface flow model to a steady-state groundwater model makes little sense. Moreover, how is the linking actually performed? Is a fluid balance maintained? Is there any justification for using a simple 2-layer model for the subsurface, especially when there doesn't seem to be any hydrostratigraphic data? In fact, the scarcity of data is a major problem to have any faith in the model.

A recent publication by Liu et al. (2106) demonstrates the application of a transient fully-integrated surface and subsurface flow model (HGS) to investigate wetland connectivity including key components of the transient water balance (precipitation, evapotranspiration, and snowmelt). This simulation domain is very similar in scale to that mentioned by the authors in their response to Reviewer 2's comments where they state that HGS was unable to solve this type of problem. HGS is regularly applied to very complicated surface and subsurface problems at a variety of scales. Considering that the authors only used 22,383 nodes in their 2D mesh for this study, it is likely that with training and support it would have been possible to apply a much higher resolution fully integrated HGS model to this domain as models on the order of 1 million nodes are now routine (e.g., Hwang et al., 2015).

Our response:

We thank the HydroGeoSphere Developer Group (hereafter, HGS-DG) for their comments that were received after the online discussion period closed.

The HGS-DG suggest that we could have used HydroGeoSphere instead of the model we have developed and applied. They suggest Liu et al. (2106) as evidence that HydroGeoSphere can be used to investigate 3D connectivity in wetland dominated landscapes. The Liu et al. (2016) paper explores 3D connectivity. However, the authors' present only 13 2D connectivity lines in one 2D cross section (see Figure 8 in Liu et al. 2016); the authors go on to explain that these 2D connectivity lines may not emulate realistic 3D connectivity lines (e.g., In the caption of Figure 8: "The [2D] flow lines were generated based on the heads shown in the cross sections and may not be exactly as the flow lines in the 3D domain."), which is understandable as topography in wetland-dominated landscapes is complex and most connectivity lines have strong 3D behaviour as we have shown in our paper. Furthermore, the size of our watershed is 5 times larger than the size of the watershed used in Liu et al., (2016). We are certain that HGS-DG knows that watershed size influences the computational challenge of characterizing connectivity lines; increased watershed size increases the length of connectivity lines and thus increases the computational time required to calculate each connectivity line.

We, together with many others (e.g., see review by Golden et al. (2014)), believe that the previous lack of a robust model with the ability to efficiently and effectively characterize all of the 3D connectivity lines in wetland-dominated watersheds has led to poor understanding of the 3D connectivity among wetlands (particularly geographically isolated wetlands) to downstream waters. The HydroGeoSphere model as presented in the Liu et al. (2016) paper provides no evidence that it efficiently and effectively characterizes all of the 3D connectivity lines in large, wetland-dominated watersheds. We maintain that our model can efficiently and effectively generate 3D connectivity lines (e.g., more than 100,000 3D connectivity lines were modelled in our paper) in a large 4,000 ha watershed by respecting the geometric properties of small-scale features.

The HGS-DG criticize our use of a two-layer modelling system. We are surprised by this criticism, given that Liu et al. (2106) use a much simpler one-layer homogenous system in a complex wetland-dominated landscape ("The aquifer was assumed to be homogenous" page 290 of Liu et al. (2106)). Our two-layer model repeated the observed groundwater discharge and recharge areas in an exceptional manner, as the first reviewer acknowledged. Furthermore, the stratigraphy and hydraulic conductivities we used are clearly explained and justified in the Methods and Results sections of our paper, and the calibrated parameters were shown to be correlated well with observed data.

The steady-state assumption used for groundwater-surface water interaction model is a reasonable assumption in the watershed we studied. In the revised version we have further clarified the validity of this assumption in our sites by using more observations. The almost 40-year observed groundwater table showed small variability, with a coefficient of variation of less than 4% in the wells located 15 km east of the watershed and less than 0.002% in the wells located 65 km west of the watershed. Given the relative similarities in climate, geology, topography and soils within the prairie pothole region

(see our original manuscript section 3.4), we believe that these observations collected at close vicinity (from east to west) of our watershed can reasonably be extended to our watershed. The low hydraulic conductivity of our prairie pothole landscape implies that there is a low possibility of fast transient flow typically seen in forested landscapes due the existence of the macropore flow. (We acknowledge that the HydroGeoSphere model would be the best option to simulate such complex fast flow in forested landscapes as was used in (Ameli et al., 2015)). Therefore, steady-state flow for watershed-scale groundwater-surface water interaction is a robust assumption for the prairie pothole landscape that was the focus of our paper. In addition, due to observed low hydraulic conductivity in Canadian PPR, groundwater flow has been typically ignored in watershed-scale modeling and only overland flow has been simulated via the mechanism of fill and spill. We are not aware of any physically based model used to characterize the 3D watershed-scale groundwater connectivity lines and fluxes in the Canadian PPR. Our model presents a significant step forward to characterize 3D watershed-scale groundwater connectivity lines and fluxes in the Canadian PPR. More importantly, it characterizes for the first time the connectivity of geographically isolated wetlands, and the connectivity-related conclusions made in our work can be extended to other parts of PPR (as was shown in the paper) making an important contribution to the implementation of policies to protect surface waters, such as the US Clean Water Rule. Given the validity of the steady-state assumption on our prairie pothole landscape, one questions if the pursuit of a transient model is worthwhile (assuming that a transient model could ever solve the connectivity-related questions raised in our work), given the considerable computational cost that would be involved particularly for model calibration.

The Beaverhill watershed used in our study is one of the most (if not the most) studied watersheds in the Canadian PPR. Therefore, this watershed was one of the best landscapes to explore our important questions on the connectivity of geographically isolated wetlands. We had (1) hydrometric observations in 1,413 artesian groundwater wells installed in the bedrock and screened 30 to 80 m below the land surface, (2) chemistry and isotopic measurements in 208 lakes, wetlands and ponds, (3) groundwater chemistry measurements in 121 shallow (< 10 m deep) groundwater wells and (4) almost 40 years groundwater table observations at two monitoring wells located in the close vicinity of the watershed. We are certain that we have used available data carefully to reasonably calibrate our groundwater-surface water interaction model. Our certainty is validated by comments made by the first reviewer (see their comment on Figure 3 of our paper) as well as leading hydrologists in the prairie pothole region in both Canada (e.g., personal communications with Dr. Jeffrey McDonnell and Dr. Less Henry, University of Saskatchewan) and the US (e.g., personal communications with Dr. Heather Golden, US-EPA).

HGS-DG Specific Comments:

P1 L14-17 – See Liu et al., (2016) for a similar study using a fully integrated surface water and groundwater model P2 L21-24 – Golden et al., (2014) primarily focused on finite-difference models such as MODFLOW which are unable to achieve local mesh refinement without incurring a high node count. Unstructured finite element methods with 3D triangular prism or tetrahedral meshes are able to achieve local mesh refinement to resolve local features with many fewer nodes than would be required for an equivalent finite difference mesh. P2 L27 – See Liu et al., (2016) P4 L18-19 – It is unclear how such a relation is established P4L20 – Can a steady-state water table, in fact steady-state subsurface flow, be supported? Are winter processes such as soil freeze/thaw and snowmelt important in this basin? There is no discussion of this, and would appear to be neglected entirely. P4L22 – One observation location situated 60 km outside of the simulated watershed does not support the use of a steady-state groundwater assumption. P4L25 – While it may be true that there is a connection between groundwater and wetland water levels, using observations from 500 km outside the watershed is extremely weak support for this assumption. Are these systems similar enough to justify this assumption? P4 L28 – 2 layers is not enough capture the details of the hydrostratigraphy P5 L22 – The HGS reference suggests that a rather old

version of HGS was used. Many feature and numerical performance enhancements, including parallelization, have been made to the code since 2008. The author should contact the developers to upgrade to a current version of the code (Aquanty, 2016). P5 L25 – The coarse mesh discretization (22,383 nodes) is highly inappropriate for the stated objective of representing 130,157 wetlands. P5 L34 – How is the connection between the HGS model and the 3D analytical model achieved? This is crucial. How can transient surface flow and steady-state saturated zone models be linked. This seems incompatible. No details are provided. Is the linking mass conservative? How is the unsaturated zone dealt with for infiltration (or exfiltration)? It seems to be neglected. Section 2 – Parameterization of the groundwater model needs to be described in more detail. P7 L3 – Do the calibrated saturated hydraulic conductivity values make sense compared to the type of geologic material or available data? P7 L29 - What units is the Manning coefficient being reported in? P7 L30 – Rill storage seems very small when considering the element sizes in the model. Is a value of 1 mm physically realistic? P9 L18 - How is it possible to mix a steady-state model with a transient model? This is incompatible and it is unlikely that mass balance will be preserved. P11 L8 – The authors should provide a definition of “semi-coupled”. Table 4 – Units Figure 3 – What is the rationale for blank portions in Figure 3a) Figure 5 – What is the purpose of showing the simulated hydrograph if not to compare it to observed data. Overall, the paper is technically weak and rejection is recommended.

Our response:

In their specific comments, HGS-DG asks questions to which answers to a majority of them are providing in the original manuscript and in the preceding section. Our model is based on a rich dataset that justifies our characterization of the stratigraphy, hydraulic conductivity and ultimately the steady-state assumption, and it provides an advantage over more complex models, such as HydroGeoSphere, for efficiently and effectively characterizing all required 3D subsurface connectivity lines.

In response to some of the more specific comments:

We have used “one way coupled” instead of “semi-coupled” in the revised manuscript, and justified the use of one way coupled method in our work using the original reviewers very useful suggestions. We have shown that this treatment has a minimum impact on the water balance and our simulation of 3D connectivity of geographically isolated wetlands as the majority of the watershed is a recharge zone with minimum exfiltration potential. Connection between wetland and groundwater is a reasonable hypothesis as HGS-DG state. Such connections have been observed in a wide range of studies in the Canadian PPR (van der Kamp and Hayashi, 2009) and we also confirmed this with available observations. The “rill coefficient” and “mesh discretization” are highly interrelated. Therefore, if we had used a different mesh discretization, we would have obtained a different rill coefficient as an outcome of the calibration processes (we have clearly explained this in the section 2.3.2). Nonetheless, our calibrated overland flow model corresponds with observed data with a high accuracy. We also think the calibrated value of rill is reasonable; a uniform rill coefficient was considered for the whole 4,000 ha domain, thus we expected to obtain such small rill coefficient. Above all the developed overland flow model accurately repeated the available observations.

We found HGS-DG’s editorial comments on Table 4 and the units of the Manning coefficient useful and considered them in our revised manuscript. Blank portions in Figure 3a are also explained in the caption of Figure 3a. Groundwater model parameterization is also explained in Appendix A. Based on the original reviewers’ suggestions we have further explained it in the revised manuscript.

There is no doubt that HydroGeoSphere is the most powerful available physically based hydrological model with the ability to solve very complex problems (as shown in Ameli et al. (2015)). However, we are certain that our analytical groundwater-surface water interaction model is an important advance to meeting the challenge of characterization of 3D connectivity lines of geographically isolated wetlands that we addressed in our study. The semi-analytical model used in our work was developed with the connectivity challenge in mind and the model’s assumptions were reasonably justified by available

observations. This model has also recently been used successfully by different research groups to answer questions on biogeochemical connectivity (e.g., Ameli et al., 2017; Ameli et al., 2016).

Finally, in neither the original or revised manuscript do we discuss whether other models (such as HydroGeoSphere) solve the wetland-connectivity problem raised in our paper. We developed both surface and subsurface models using available observations with an exceptional accuracy, and answered some important questions on the connectivity of geographically isolated wetlands using carefully developed models. A model comparison was beyond the scope of our work. However, recent papers (Golden et al., 2014; Golden et al., 2017) define a series of requirements that physically-based models must have to be able to directly characterize the watershed-scale connectivity of wetlands in wetland-dominated landscapes. For example, Golden et al. (2014) analysed a wide range of finite element methods in their assessment of connectivity (Sec 2.3 pages 199-201). To see these requirements and an assessment of existing models that meet these requirements, we refer the readers to these papers.

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