Response to Reviewer 2

Summary

This paper explores how to adequately represent wetlands in land surface models, with a case study in White Gull Crrek, Saskatchewan. The authors find that existing parameterizations which ignore groundwater and upland influences to be inadequate and make some recommendations for how to improve the representation of wetlands in LSMs. The authors explore four different model representations, from fully uncoupled to fully coupled. The main conclusion of the paper is that explicit groundwater interactions must be accounted for the adequately represent wetlands in LSMs, with chained and fully coupled modeling approaches working well under different circumstances. Overall I think the paper was interesting, and straightforward to understand. However, I think this paper is only able to make a conceptual argument for this case, rather a quantitative one. The only comparison to observations comes from the comparison of the groundwater level in the upland site, which tests the ability of the model to represent the upland, but not the wetland. Of course, I am sympathetic to the fact that observations of discharge from the fen don't exist, making this a large challenge.

However, I think the paper would be stronger if it could make a quantitative argument for the need for coupled models in representing the wetlands component, rather than a conceptual one. I think some additional analysis looking at either some downstream discharge could be useful for making this point, if such a stream gauge exists. I think correlating such a streamflow with the daily outflow into the river from your model setups could yield an interesting result. Similarly, correlating the E_f from your fen model with latent heat from the FEN tower vs the OJP tower could make a good case for including the coupling between the wetland and upland. Because of these recommendations for additional analysis, I am recommending major revisions.

We are grateful to the reviewer for these thoughtful comments. However, we might not fully agree that our analysis is only conceptual/qualitative and not quantitative. We wish to be upfront about the lack of available field observations from the field site for our study – indeed few, if any, sites have adequate data to fully constrain simulations of lateral subsurface flow. Given this, our intention is to produce a semi-hypothetical model that allows us to quantitatively test alternative model configurations with the objective of determining the circumstances in which simpler models are consistent with more complex ones. It is desirable to make models as simple as possible and no simpler, so our approach provides a systematic basis for understanding what level of simplification is justifiable. We will further elaborate on the data limitations and scope of this work in the revised manuscript to address this important review comment.

We have already compared the simulated ET from the FEN and OJP towers with those produced by our model (Figure 1 and 2). The simulated ET at Fen site (Figure 1) is overestimated in some years, compared with the measured fluxes. This is because of limitations with how MESH simulates evapotranspiration from wetlands (which are not explicitly represented in MESH), and the model always calculates evapotranspiration at the potential evapotranspiration rate. The simulated ET rates at OJP (Figure 2) have a systematic overestimation, with larger errors after 2012. This was explored in Nazarbakhsh et al., 2020, who found that the errors are happening during the melt period – a problem that is yet to be resolved in MESH.

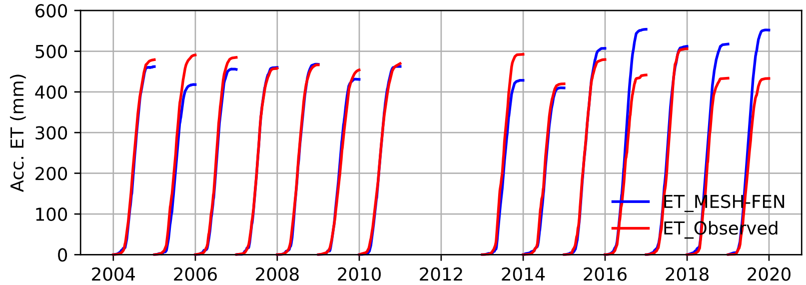

Figure 2: Annual simulated evapotranspiration (ET) rates at wetland-FEN site in blue lines compared with observed ET at FEN flux tower represented in red lines. Years 2011 and 2012 have missing observations.

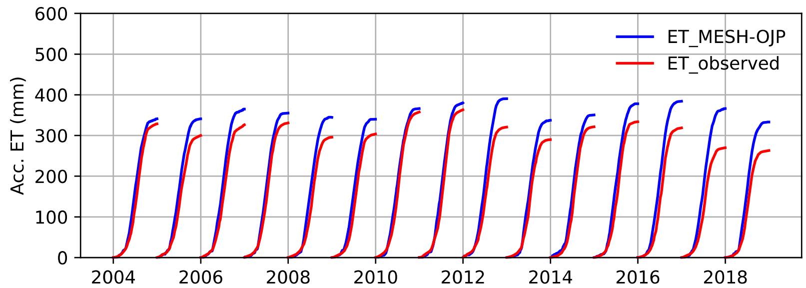

Figure 2: Annual simulated evapotranspiration (ET) rates at upland-OJP site in blue lines compared with observed ET at OJP flux tower represented in red lines.

Major comments

As of section 2, there is no real mention for the digression into coupling LSMs with GWMs as it relates to wetland modeling. Perhaps a few sentences connecting these topics and prior work on this connection would be useful.

Thanks for pointing out this. We will include more literature regarding LSMs and GWMs coupling in the revised manuscript.

I was unclear where the information from the FEN tower actually comes in. At first it seemed like it was the data used to force the wetland model, but later it seemed like OJP was the site where the meteorologic data was taken. Could you clarify which datasets were used to drive which models?

We use the meteorological data from both two towers OJP and the FEN to force our model. The data from OJP flux tower are used to force the upland component (MESH-OJP), while the data from FEN flux tower are used to force the fen component (MESH-FEN). The outputs (such as evaporation and runoff) from the two MESH setups are used to drive our upland-groundwater-fen model. This will be highlighted in the revised manuscript.

Equation 3 shows that there are several parameters, c_{spill}, h_{spill}, and n, whose values don't seem to be given anywhere. I am not sure how to interpret the results in section 5.2 without knowing these values. To me, it looks like the uncoupled model (V1) simply shows no outflow because h_f is always less than h_{spill}, but I am not sure if this is correct. If so, doesn't that correspond to starting the simulation so that h_{spill} is simply set to whatever the initial fen water level is? Wouldn't modifying the parameter values change the results? Even if you do modify these values, of course you will find that you just don't have enough water to maintain the fen's water level because precipitation is not enough to maintain the water level, but showing how these parameters affect this result would be useful and probably highlight your point that the coupling is necessary to maintain predict reailistic discharge from the fen.

Please note – a similar comment was made by reviewer 1 – we provide the same response here. We agree we should provide the parameter values used, and explain how these values were selected, and will do this in the revised manuscript. These three parameters control the timing and magnitude of discharge from the fen into the river channel. We did not have streamflow data to calibrate these parameters, and therefore our approach was to use sensible fixed parameter values that enable us to perform controlled numerical experiments. Our objective is not to simulate streamflow precisely, but rather to explore the sensitive in simulations to changing coupling between the wetland and the upland.

h_spill represents the spilling threshold of the fen, so that when the fen water level is below h_spill, there is not discharge from the fen into the river. We set this value equal to the elevation of the fixed head boundary from the uncoupled groundwater model (Figure 2b/c).

The values of c_{spill} and n (c_{spill} = 0.1 and n = 1.5) were arbitrarily chosen within the reasonable ranges. The ranges were defined based on the recommended ranges in (Razavi and Gupta, 2019), which they used in modelling the fast reservoir with non-linear response in the HBV-SASK model.

We will make this clear in the revised manuscript.

While you say the data is available, the code to reproduce the experiments does not seem to be. This should be linked in the "Data Availability" section.
We will make that available with the revised manuscript.

Minor comments

The term L is never defined in equation 2.
L value is the length of the upland hillslope. That will be added.

Line 186-7: "chosen rather arbitrary" -> "chosen arbitrarily"
We will modify this in the revised manuscript.

Line 238: You compare V1 and V3 and don't include V2 because it is functionally the same as V1 for the uplands, but having this spelled out here would here would be useful for anyone who skimmed over 5.1. Maybe just say (V1/V2)?
This is a good suggestion – we will adopt this.

Line 280: "Considering the GW dynamics underneath the upland is essential" Could you elaborate what it is essential for?

This is essential for more accurate simulation of streamflow into river, instead of high overestimation of the streamflow, which happen when ignoring groundwater representation. We will make it more clear in the revised manuscript.

Figure 6: Need to fix "(Error! Reference source not found.)" in the caption.
That should refer to Figure 2 in the manuscript. We will fix this.

References

Nazarbakhsh, M., Ireson, A. M., and Barr, A. G.: Controls on evapotranspiration from jack pine forests in the Boreal Plains Ecozone, Hydrological Processes, 34, 927–940, https://doi.org/10.1002/hyp.13674, 2020.

Razavi, S. and Gupta, H. V.: A multi-method Generalized Global Sensitivity Matrix approach to accounting for the dynamical nature of earth and environmental systems models, Environmental Modelling & Software, 114, 1–11, https://doi.org/10.1016/j.envsoft.2018.12.002, 2019.