Summary

Wen et al. use a well-studied system (the SSHCZO headwater catchment) to model spatial and temporal variation in DOC production and export. The need for this work is outlined in the introduction where the authors argue that production and export are difficult to predict because they are driven by multiple, often competing factors (temperature and hydrology) that complicate outputs. Their work shows that hydrology is the dominant factor influencing DOC export while temperature drives DOC production. The manuscript is well-written and well-detailed and represents a solid contribution to the literature. The authors pull apart complex interactions to model the stream system and provide important insights into the sensitivity of model outputs to variations in different model parameters. I support publication following the below revisions. My main concern to be addressed is that respiration is an important pathway that is not properly considered here.

General comments

Respiration is a potentially major pathway for C loss that is not accounted for in this SSHCZO carbon mass balance model, a fact which is not discussed until the last paragraph of the discussion. Furthermore, the support for not considering respiration is quite weak. No literature on C budgets for SSHCZO is cited, so the "high DOC accumulation" (p.26, l. 44) is not placed in context of other fluxes. That is, there is no quantitative information given here that supports that vertical carbon fluxes are minimal relative to hydrologic export. More consideration for previous work on C budgets at SSHCZO should be cited, starting with the Brantley et al 2018 VZJ review and refs cited therein. If the vertical flux is comparably large, what impact would including this flux have on the observations made in this model?

The conductivity mass balance hydrograph separation used to calculate groundwater input was referred to multiple times but not shown. Is there a supplementary figure that would be useful for supporting this? Please also incorporate more previous literature on surface-groundwater interactions at SSHCZO and how they influence stream chemistry (e.g., Sullivan et al., 2016, Chem Geology; Herndon et al., 2018, Chem Geology; Thomas et al., 2013, VZJ; Kim et at., 2018, EPSL). Do those observations generally match what is observed here for DOC patterns?

This model uses data only from the South Slope – how comparable are these to pore water DOC concentrations on the North Slope? Is it valid to assume that these sites are representative of the entire catchment?

Specific comments

Line numbers are referred to here by page and line number because the full line number was not visible after 100.

p. 4, l. 13. Since Temperature and Precipitation are a large focus of this manuscript, I suggest including well-defined annual T and P values, i.e., the average and standard error for the past ten years.

p. 7, l. 96. Are there citations for these values?

p. 10, l. 65. Should DOC be in mg m^{-3}, and was this conversion from L to m^3 incorporated?

p.11, l. 7. What percentages do these groundwater inputs correspond to?

p. 12, l. 16. S_T barely changes across the year, so the small S_T during the dry period is not observed. Rather, it looks like it just very slightly decreases relative to wet periods.

p. 12, l. 17. Wouldn't high ET coincide only with shrinking the connected zone, not expanding it?

p. 12, l. 25 (figure caption): Define S_U and S_S in the caption.

p. 13, l. 30. This section states that groundwater contributes substantially to DOC patterns at low discharge. I do not see this in the figure. The stream data and model seem to very closely follow the soil model under all discharge conditions. Is there a way to quantify this contribution and communicate it in the text?

p. 14, l. 64. Does the "legacy of produced DOC…" suggest that DOC is desorbing from the soil in response to flushing?

p. 16, Figure 5. Is DOC mass storage in steady-state over the year?

p. 16, l. 94. The chevron pattern is only observed in the model output, not in the stream data. There are not enough data to support that this pattern occurs at low discharge. I think you can only propose that this pattern would be observed with enough data, but it's not currently supported.

p. 17, l. 00. The explanation for the dilution behavior is clear, but what explains the proposed flushing behavior at low discharge? Swale soil water mixing with groundwater?

p. 19, l. 49. These two lines are contradictory. The first sentence says that sorption "resulted in smaller Re" and the second line says that sorption "increased the magnitude of Re".

p. 20, l. 00. Please clarify…is the increased C storage indefinite? Does high sorption mean that C continues to accumulate in the catchment or is it stored for only a portion of the year and then released (SOC at steady-state)?

p. 22, l. 25. Do these values represent averages over the whole catchment? I would assume there is larger variation in soil moisture between different landscape positions.

p. 23, l. 36. A model like this seems like an interesting way to identify potential hotspots based on temperature and moisture conditions.

<u>Technical comments</u>

l. 101. Suggest "what factors determine"

p. 5, l. 42. Does this mean "Flux-PIHM *separates* the subsurface flow into…"? Awkward as written.

p. 14, l. 58-59. The "Soil water DOC" sentence is not understandable as written.

p. 22, l. 11. Suggest replacing "Rp was identified for…" with "Rp was identical for both groundwater contribution levels…"

p. 24, l. 72. Year for Cincotta ref is missing.