

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
<https://doi.org/10.5194/hess-2021-358-RC1>, 2021
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Review of hess-2021-358: "A parsimonious model of longitudinal stream DOC patterns based on groundwater inputs and in-stream uptake" by Ploum et al.

Anonymous Referee #1

Referee comment on "Groundwater flow paths drive longitudinal patterns of stream dissolved organic carbon (DOC) concentrations in boreal landscapes" by Anna Lupon et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-358-RC1>, 2021

Content

This manuscript describes a parsimonious model application for instream DOC concentrations along a stream reach, basically consisting of a mixing model and an instream uptake model. While the DOC concentrations were directly taken from groundwater sampling at each of the 25 stream segments, the amount of GW inflow was calculated from the discharge discrepancy between inflow and outflow of the reach. The distribution of water inflow per segment was either equally distributed (diffuse inflow scenario) or according to the area of specific sub-catchments contributing to groundwater inflow at a segment (UCA, upslope contributing area). The model is used to test the hypothesis, that previously defined 'discrete riparian inflow points' (DRIPs, Ploum et al. 2020) have a major influence on DOC stream concentrations and that this is connected with spatially variable groundwater inputs.

General comments

The topic of DOC mobilization and fate is well within the scope of HESS, but the manuscript needs to be improved substantially before publication is possible.

Some explanations of the model structure and setup are unclear. The paragraph explaining the strategy to account for uncertainty in the simulations (L.232-244) is on the one hand hard to follow and some decisions seem arbitrary. On the other hand, simulation

uncertainties are not discussed at all in the manuscript, even though they are shown in the main results figure 3.

The model performance is rated by using four different likelihood measures and pre-defined thresholds for behavioural models. When the model performance is discussed, it seems arbitrary which likelihood measure is highlighted. The discussion would be stronger if the model performance evaluation would be more systematic. Further, the influence of DRIPs on DOC concentrations is only visible (in the sampling data) if the gain in streamflow is $> 40\%$ (Figure S1). Otherwise, the lake outflow dominates, which was also stated in the discussion. Thus, events A, B, F, G and H are the most important for the testing of the hypothesis. These events should be discussed in more detail. For example, 'diff_nobio', which does not consider UCAs, often performs better or similar good compared to the UCA-version. Is the UCA-concept really helpful or are the stream concentrations rather driven by the groundwater concentrations and not the inflow volumes? If the UCA-concept is helpful, in which hydrological situations? After a structured analysis and discussion, more specific conclusions should be drawn.

Specific comments

L.1. Title: In my opinion, the title does not correctly represent the content. In the manuscript, the model itself is rather used as a tool to analyse processes. The title, however, suggests that the main focus is on model structure and testing.

L.16. Change 'concentration' to 'concentrations'

L.29. The word 'Importantly' seems inappropriate here.

L.55. Change 'By contrast' to 'In contrast'

L.58. Remove 'a'

L.79. Remove 'what' and 'is'

L.89. Remove 'than'

L.89. The term 'passive pipe' might be misleading. Pipe flow is a specific type of hydraulic flow, which is not addressed here at all. Probably better refer to 'non-reactive' or 'recalcitrant'.

L.136. Change 'talweg' to 'thalweg'.

L.141. What kind of filters were used for filtering (filter material)? How long was the time between sampling and lab-analysis? Is there a reference explaining DOC sampling and analysis in more detail?

L.165. How were the reaches classified into DRIP and non-DRIP zones?

L.166. What do you mean by '...was weighted based on the mean...'? Did you want to say '...was derived by using the average of all...'?

L.168. Change 'upslope contributing are' to 'UCA'. It was already expanded in the introduction.

L.217. Remove 'yielded'

L.232ff. In this paragraph, the method for uncertainty estimation is explained and the uncertainty bounds are shown in figure 3. However, the uncertainty of the model results is not discussed at all in the results and the discussion sections. Please add it.

L.233-234. '...we compared the range of uncertainty in our simulations to the range of uncertainty in the observations.' Where did you do that? I can't find it in the manuscript.

L.234 The number of model runs (100 for each scenario) appears really low, especially for such a simple and fast model (in terms of computational time). Usually, several thousands of runs are performed for uncertainty estimation. How did you check that 100 runs are

enough for stable uncertainty bounds?

L.236. Why did you choose to use 66% uncertainty bounds? More common are 80% (10th-90th quantile) or 90% (5th-95th quantile).

L.246ff. I understood that 100 model runs were performed for each scenario. Which one was evaluated with likelihood measures? All? Only the best? Which values are shown in table 2?

L.252-254. The description of R^2 is - at least - unusual. I would recommend using a more straight-forward description. In this context, the most important thing might be, that R^2 shows the ability of the model to simulate the dynamics of the measured time series, but not the absolute value.

L.265. The reference to 'Fig. 3F, K-M' should rather be 'Fig. 3E, K-M'

L.277. change 'NSE<0' to 'NSE was <0'

L.295-296. Probably it is worth mentioning that R^2 generally has problems with evaluating uniform values such as D and I.

L.315. Please re-format the table. Floating point numbers and scenario identifiers are in two lines, which makes them hard to read.

L.319-328. This paragraph reads like a conclusion/summary. At this point, however, the results were not yet discussed. The paragraph should be removed.

L.337. change 'influence stream' to 'influence on stream'

L.341. Remove 'of'

L.347 The mentioned UCA_BIO is not the best model under the conditions of 3E (Neff=0.5; $R^2=0.84$). DIFF_BIO performs much better with Neff=0.81 and $R^2=0.86$.

L.364. Change 'that' to 'than'

L.364-365. Change `... spatial variability in groundwater ...' to `... spatial variability in groundwater concentrations...'

L.402. `...demonstrated that UCA can be useful to identify "reactive" reaches...'. Actually, the model results don't show a clear signal towards UCA explaining the variability better. Often, the Diff-version is similar good or even better.

L.419ff. In the conclusions, UCAs are not mentioned at all. Since they play an important role in the whole manuscript, please add your conclusions about this concept.

L.425. `occasionally' is imprecise language. Please, be more specific.

L.429-431. `This study (Mineau et al., 2016)'. This is not a conclusion of this study. Delete or shift the sentence.

Figure S1. I would recommend adding figure S1 to the main text. I think the different hydrological conditions, even though they are provided in the text, are much easier to understand with Figure S1.