Interactive comment on "Diel fluctuations of viscosity-driven riparian inflow affect streamflow DOC concentration" *by* Michael P. Schwab et al.

Anonymous Referee #1

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General comments:

The paper analyses diel concentration dynamics of DOC in a headwater stream. The analysis explicitly addresses dry conditions and thus focuses on the internal controls of the system rather than external hydrological drivers such as snowmelt and rainfall. I think it has an important and general value to better understand stream concentrations dynamics and moreover to disentangle responses of external forcing and internal controls.

In this paper the authors argue that diel DOC concentration variability is driven by temperature-induced changes of hydraulic conductivity (via water viscosity) which leads to changes in the magnitude of groundwater discharge and the associated discharge of DOC and ultimately to variations of instream concentrations. Although the

authors claim that this is the only mechanism that can explain the observed pattern, they do not provide enough evidence. I would like to challenge the interpretation by asking to perform a mixing analysis. Assume that all variations in discharge are driven by viscosity-induced discharge of groundwater (or water from the riparian zone if you like) what would be the DOC concentration in this water that explains the observed DOC concentrations in the stream. Do these concentrations match the observed DOC concentrations in the riparian zone? This would be a simple test to check the plausibility of the proposed mechanism. Moreover one could have a look at the concentration-discharge relationships: From Figure 1 c and d I see a dilution pattern in DOC concentrations at multi-day timescales. Low discharge=High concentration which means that the load (C*Q) remains broadly constant, indicating a constant source. This relationship does not hold for the diurnal discharge peaks, here we see increasing concentrations with increasing discharge - suggesting a temporarily dynamic source, potentially viscosity effects. In g and h this seems reversed. DOC follows discharge at the multi-day time scale- meaning a drastic reduction of DOC load, while daily DOC peaks occur at daily discharge minima. This last fact largely counteracts the "viscosity hypothesis". Thus for ~50% of the data the viscosity hypothesis must be rejected. I encourage the authors to "harvest" their data in multiple ways to support their hypothesis or better to explain the multiple processes rather than focusing on a one-sided interpretation.

Given the fact that the study was conducted in a potentially well studied catchment, I wonder why the data is so sparse. The entire line of argumentation is based on a single temperature observation location which is used to explain catchment scale effects. I think this "extrapolation" is not justified. Moreover, the authors claim that at this location, there is high subsurface flow. However, they do not provide any data to support this. How do I know that this location is representative for the entire catchment? I strongly recommend better explaining the hydrologic aspects of how this catchment functions. I mostly missed some hydraulic gradients between the stream and the groundwater, some numbers on the amount of GW discharge and its spatial distribution and how temperatures, particularly streambed temperatures vary spatially.

I am aware of the Schwab et al. 2016 study but also there, these essential information are not provided.

My main technical concern is the position of the temperature sensor. If I understand correctly temperatures are measured at 10 cm below the land surface in the unsaturated zone. I wonder how this temperature can be representative for the water that is discharging into the stream. In the unsaturated zone water flows vertically, driven by gravity. Thus more or less horizontal flow, discharging into the stream is bound to saturated, Darcian flow. Moreover the relationship between hydraulic conductivity and viscosity is for saturated conditions. Under variably saturated conditions, saturation should have a much larger influence. No data on water saturation is reported. Anyway I doubt that the unsaturated zone is the source for the stream water. Thus to evaluate the effects of viscosity temperatures should be measured at groundwater discharge locations directly in the streambed.

In summary, this manuscript presents only a modest amount of (spatial) data to support the "viscosity hypothesis". My impression is that the interpretation of the data is one-sided towards this hypothesis. I encourage the authors to acknowledge the pattern which are obviously in their data and provide an analysis that is accounting for the different controls of DOC concentration which vary between seasons.

Specific comments:

I find "riparian water" is a misleading term - hydrologically there is no difference between soil and riparian water - both are in the unsaturated zone. So at least it should be defined what exactly is meant here.

P.3. 15-20: This is exactly the point where the authors are on the wrong track. Water (groundwater - saturated zone) cannot flow through the unsaturated zone (I guess that the riparian zone is unsaturated because of 1) p5.l.3 sampling of riparian water was with suction cups, and 2) sampling depth is 10 cm below the ground) and then entering the stream. I encourage the authors to provide a conceptual model on the

water fluxes and heat transport at the site

P.4.l.14-15: Please provide a reference, better data confirming that this location has high GW inflow. Moreover the sampling and measuring locations should be provided, in a way that the reader knows if the riparian water was sampled in 10 cm, 1m or 10m distance from the stream. Also: where are the GW wells? I think the spatial relationships are important. Please provide this in a map or a cross-section.

p.5 l.24.: I don't understand what is meant by anomaly. This seems important for the further analysis but I don't get it. Is it a time shift between the variables? Or is it the difference between the 24h moving average time series and the original time series? I guess the latter. If so, what has been done is a simple form of spectral high pass filtering. You cancel out the low frequencies and only keep high frequencies of 1/24 dˆ-1. This should be better explained, best in terms of common time series analysis terminology.

p.5.l.25-26. If a 24h moving average is applied you filter all fluctuations with shorter timescales.

p.5. l 29.-33. Are periods without DOC fluctuation also periods without temperature variation? if so this would support the viscosity hypothesis. Please report temperature and viscosity fluctuations in these periods as well.

p.10. l.15: What I see in Figure 1 is that for all times SUVA and DOC are highly correlated - also the minima. So far as I can see there is no indication that SUVA is particularly high when DOC is high. This is also supported by the good correlation between SUVA and DOC fluctuation in Fig. 3. Thus SUVA seems a good indicator for DOC concentration and thus not only the maxima, but generally SUVA indicates inputs not changes in DOC quality at this site.

p.11.l5: Here I would disagree, the evidence is not strong.

p.11. l.14-20: I think the reversed relationship between concentration and discharge is

really striking and is not explained. e.g. p.l.17 "different spatial impacts" what is this exactly, how can you assess this by having only measured at a single location. I think if the authors could figure out how the controls of DOC concentration change over the season because the importance of different controlling factors vary, would make this work a strong contribution.

p.12.l4 ff: I think this perceptual model should be extended by discharge effects. The authors should remember that their main line of argumentation is the increase discharge of water driven by viscosity. Comparing Fig.1 g and h with 6 d I would not bet that DOC inputs are high in the afternoon, concentration is high, but discharge is low. So again, also consider loads, not concentrations alone.

Figure 1: Please provide temperature data as well.

Figure 3: This is a tough one for ~8% of male population! Anyway, in 3d the green regression line does not match the data well - visually it should be steeper.