Werner et al. examines export patterns and dynamics of dissolved organic carbon from the riparian zone in a temperate, forested catchment. The paper used an array of different approaches to relate DOC source zones within the RZ to their dominant DOC export mechanisms. Stream DOC samples from different hydrological conditions were compared to riparian DOC groundwater and surface water chemistry. They also characterized DOC chemically (via Fourier-transform ion cyclotron resonance mass spectrometry) and used topographic analysis (at a resolution of 1m). Water fluxes were simulated using the code HydroGeoSphere. The paper concluded that surface runoff from zones of high TWIHR values, which occupied about 15% of the total area, exported about 1.5 times the load of DOC from the remaining 85 % of the area, and that "this study highlights that surface DOC export from the riparian zone plays an important role for lateral DOC export from hydromorphic soils with overall low topographic relief."

The work is interesting and collated an array of approaches from chemical analysis to modeling at high spatial resolution. The current manuscript is phrased from the angle of horizontal heterogeneity / landscape topography, which I think is actually already well studied [Herndon et al., 2015; Jencso et al., 2009; Ledesma et al., 2018; McGuire and McDonnell, 2010; Pacific et al., 2010]. But I find the conclusion is not particularly surprising.

We appreciate your evaluation of our Manuscript (MS). Your comments as well as those by the other reviewer make it clear that we need to phrase our conclusions more crisply and clarify the contribution of our work to the existing body of knowledge and its practical implications (see also Referee #1, General Comment on "4 Discussion"). We will discuss the references you mention if we are allowed to revise our paper.

Please note that most of the references you mention (Herndon et al., 2015; McGuire and McDonnell, 2010; Klaus and McDonnell, 2013; Zhi and Li, 2020; Zhi et al., 2019) are concerned with much larger scales than our work, which studies an individual riparian zone (RZ) in detail. The scale of our experimental site is the smallest on which a hydrological landscape unit (in this case, the RZ) can be studied in its entirety and the largest that still allows extensive monitoring in the field during several seasons within realistic constraints on resources and personnel. The paper's contribution lies in the combination of multi-sensor field monitoring backed up by detailed hydrological modelling and high-resolution chemical analyses of DOC quantity and quality. This allowed us to determine the size and contribution of hydrologically different DOC source areas, something that so far has only rarely been quantified (Bernhardt et al., 2017). We intend to better clarify this in the text and improve the explanation of the practical relevance of some of our findings. Perhaps an improved phrasing of our results and the conclusions we draw from them can illuminate where we agree or disagree with earlier work. For instance, Ledesma et al. (2018) contradict the references above by stating that the RZ, not the hillslope, is the dominating source of DOC in streams. Our work supports that claim but shows that surface flow is an important carrier of DOC stemming from localized source areas, in contrast with the concepts of a dominant source layer in the subsurface (Ledesma et al., 2018) or a riparian integration model (Seibert et al., 2009) that both hypothesize predominantly horizontal subsurface flow as the main transport mechanism for DOC. This suggests – as also assumed by Jensco et al., 2009 – a dependency of DOC export on morphologic, climatic and topographic conditions. Our monitoring set-up is well suited for identifying such conditions.

On the other hand, it seems to me that this work presents a rare opportunity to dig deeper to think about the relative influence of vertical versus horizontal heterogeneity. The relative importance of vertical versus horizontal heterogeneity in doc export is poorly understood. In particular, there has been quite some interests in understanding the solute export from different subsurface depths, for example, [Seibert et al., 2009; Zhi and Li, 2020; Zhi et al., 2019]

The relative influence of vertical versus horizontal heterogeneity will be discussed in our MS (see also Referee #1, L487). As mentioned above, we found that surficial DOC export from high TWI zones dominates DOC export from riparian zones to the stream at our study site suggesting that horizontal heterogeneity predominates vertical heterogeneity in low relief catchments with hydromorphic soils. We also point out that the top few decimeters of soil was very organic and contained many rhizomes. Somewhat deeper, the soil contained so many rock fragments that digging with a spade was no longer possible. At roughly 1 m (with considerable variations), fractured bedrock occurred. To study solute export from various depths in the field, tracer experiments are needed. However, taking samples at well-defined locations in this soil proved very difficult, even in the dry season when the groundwater was not near the soil surface. We do not believe it is feasible to excavate the soil in intervals of 5 or 10 cm to study the distribution of a dye tracer. It is likely that colleagues elsewhere have experienced similar difficulties. This would explain why the relative importance of vertical versus horizontal heterogeneity in doc export is poorly understood. We fear that may remain so for the foreseeable future.

The data from this work have depth profile (top 100 cm) of doc, and flow calcination from different depths. These two can be combined to calculate at what depth most doc was exported, and how the export varied with depth in high flow events. At a minimum, it would be nice to see some discussion along this line of vertical heterogeneity.

We agree, some discussion along this line will be added to the MS (see also comments below and specific response to comment of referee 1, L487). However, given the difficulties associated with tracer experiments to identify flow paths in this soil that we outlined above, this discussion will have to be based entirely on the numerical model and therefore has to rely on a schematized conceptualization of the subsurface.

I also find “Surface export” is a confusing term. Is this really surface runoff, or does the water mostly flow through top soil? Unless in extremely large events, most forests do not see significant amount of surface runoff. In many places, stream water comes from “old” water from the subsurface, not surface runoff “new” water [Klaus and McDonnell, 2013].

There is no ambiguity in our use of the term. The RZ on which this paper focuses was not forested, although the surrounding slopes were. Overland flow in the RZ was quite common during wet periods. With “surface export” we refer to water that has been on the surface at least once on its way to the stream – as indicated by respective exchange fluxes. We will clarify this in the text. Surface flow can reinfiltrate and flow to the stream/ boundaries through the subsurface.

The steep hillslope but low slope and hydromorphic soil in the study site; micro-topography as well as scale and climatic setting of our field site contrasts those reviewed by Klaus and McDonnell (2013). Also, they view water contributions from a watershed/catchment-scale perspective, whereas we focus on the RZ itself. Given the nature of our study site, more overland flow is to be expected (although Klaus and McDonell also reviewed studies that had

up to 100% “new” water contributions). We explicitly mentioned that our findings hold for a low relief riparian zone with hydromorphic soils to account for these differences.
As discussed below (comment on L525 and following) we therefore do not think that the concept presented in Klaus and McDonnell holds for our field site.

Line 52-54, “a strong focus on vertical heterogeneity”. Interesting thoughts but maybe not accurate. My impressions is that existing literature has focused much more on landscape hillslope - riparian heterogeneity. As I mentioned earlier, papers in hydrology and ecology have emphasized a lot on hydrological connectivity from hill to streams. In fact, the management practices related to riparian zones originated from our understanding of differences between hill and riparian and their connectivity.

As Referee #1 and #2 already pointed out, there exist several studies that cover lateral variability. Therefore we will change this sentence as described in Referee #1, L49.

Figure 3: also draw doc in this figure to help viewing when doc coming out most?

We agree, DOC measurements (stream routine and event auto samples) will be added to Figure 3.

Figure 4: this figure is busy. What is ns, hc, oc, … please explain in caption or provide legend. Why not show doc vs depth data. It would be cool to see that data. We rarely have subsurface solute depth profile. Also, these depth data, together with the modeling work for subsurface flow, provide rare opportunity to assess the relative importance of vertical heterogeneity vs horizontal landscape heterogeneity, as I I mentioned earlier

We will add the requested abbreviations to the captions.
Regarding the depth data, we are a bit in limbo. It is clear that this referee has a keen interest in the vertical distribution of DOC transport (see Figures below for DOC depth profile), but because of the nature of the soil and the bedrock, we were unable to explore this in the field in a meaningful way, as explained above. In principle, it is feasible to interrogate the model results to tease out the variations of DOC movement along the vertical dimensions but it has to be understood that the usual limitations to detailed interpretations of modeling results apply: the model is elaborate and fully 3D, but it remains a schematization of the real world, and we are not sure that an analysis of the numerical results at the level of detail desired by the referee is justifiable.

Furthermore, a discussion of the fine details of the model results will distract from the more practical aspects that referee 1 requested us to address. To us, these appear to be of more interest to the HESS readership.

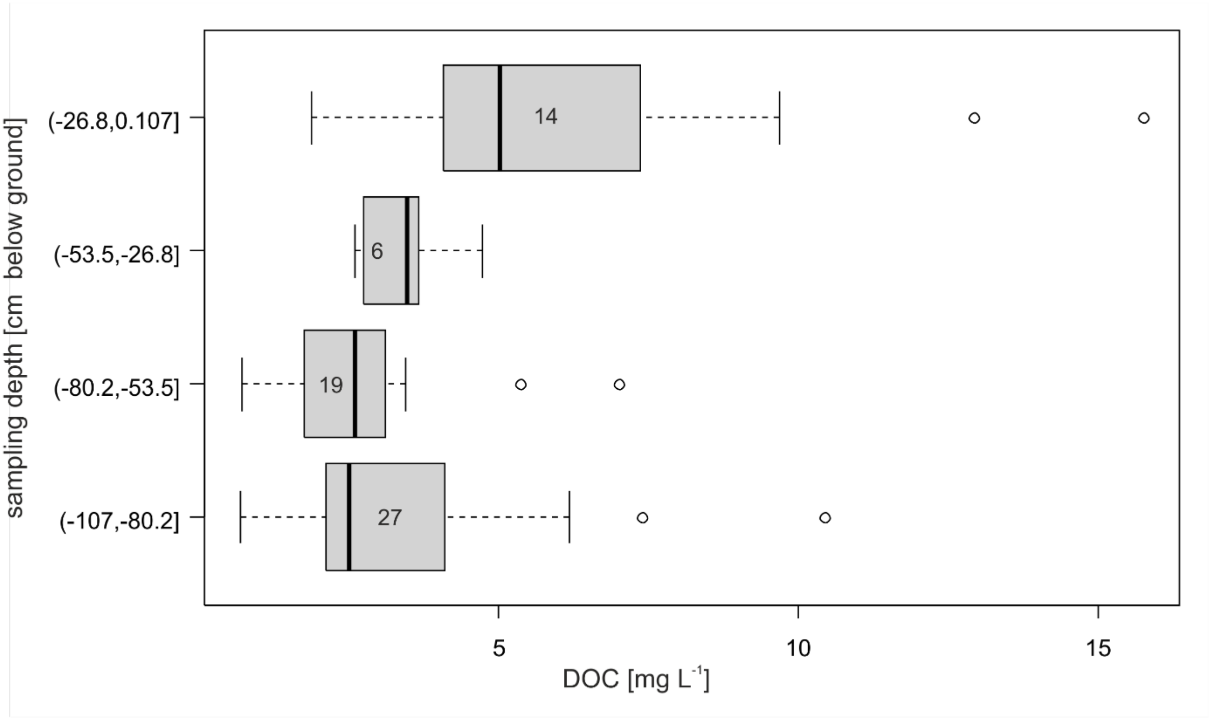

Figure: Boxplots of DOC concentration vs. sampling depth (negative is below ground). Numbers in boxplots indicate sample size.

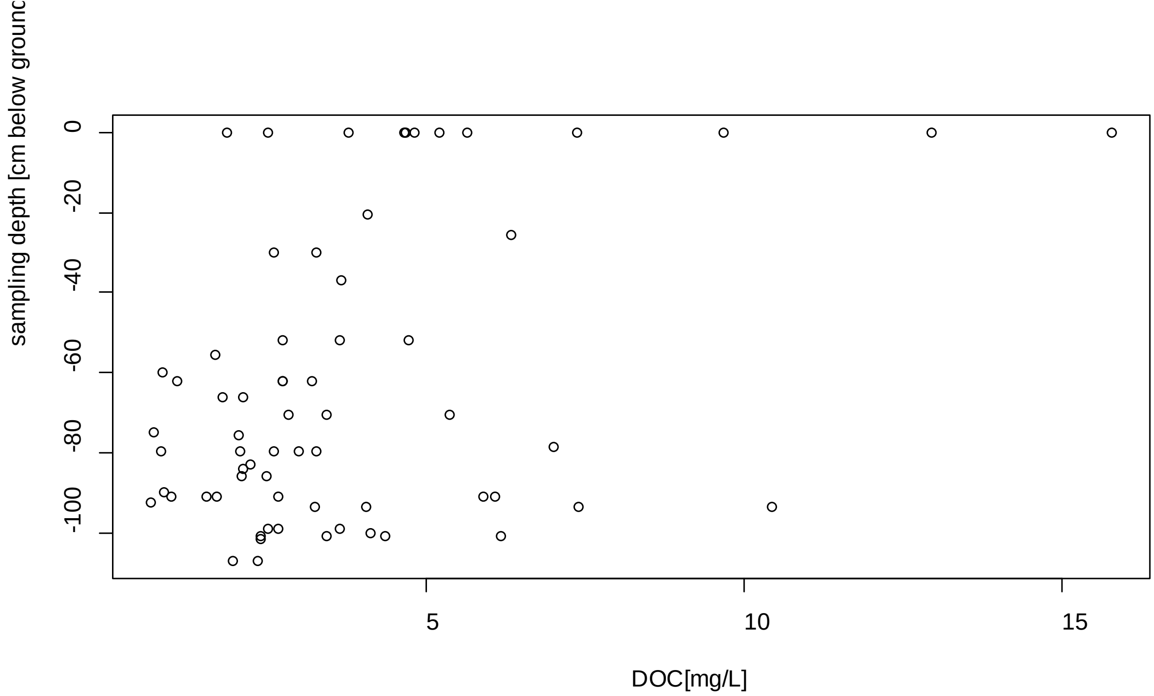

Figure: Plot of DOC concentration vs. sampling depth. Surface pond/soil solute sampling depth was set to 0 cm.

Figure 7: can the discharge data be added here? Would it be easier to understand the time series of doc export?

We agree, discharge data will be added to Figure 7.

Line 525-530: it seems that there is some mis-understanding about “lateral export”. Lateral export means doc export via surface water (streams and rivers). Stream water can come from the surface runoff and subsurface (soil + gw).

We apologize for the misunderstanding, we will change the sentence to

“In this regard, we found that surficial DOC export dominated overall DOC export to the stream in our study site. “

In fact, in many places, stream water comes from “old” water from the subsurface, not surface runoff “new” water (Klaus+McDonnell 2013). While I agree that surface runoff can be important during events, it may be misleading to present these numbers without mentioning the temporal scale (event scale). At the annual scale, these numbers might be quite different.

At the annual scale, the median contribution of surficial runoff to total runoff generation was 61 % (± 12 % standard deviation), but at the event scale surface contributions increased up to 99 % during event situations (L424, Figures 6 and 7 in the MS). Note that the stream runoff is erratic (Q ranges between < 0.01 in dry summer and > 1.1 $m^3 s^{-1}$ in wet winter). Most of the runoff generation in our study site occurs during events under wet, saturated conditions. Furthermore the riparian study site has an overall low topographic relief and consists of hydromorphic soils of typically low hydraulic conductivity. The high groundwater level and the soil properties both increase the probability of surface runoff generation under the given conditions.
The generated surface runoff might be mixing of “old” water that exfiltrates from the subsurface (as indicated in our study and suggested by Frei et al. (2012)) with “new” water, but there is no isotope data or StoreAge Selection function (SAS) at hand to investigate the age distribution or preferred selection of the generated runoff. Klaus and McDonnell (2013) arrived at a different conclusion because their spatial perspective was very different from ours (see above), and possibly they were interested in much larger spatial scales. We further want to note again that the range of surface contributions (“new” water) in our catchment is in line with studies mentioned in Klaus and McDonnell (2013).

We will contrast and discuss the different conclusions from Klaus and McDonnel in the MS accordingly and emphasize the different temporal scales that we consider.

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