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Interactive comment

Interactive comment on "Are hydrological pathways and variability in groundwater chemistry linked in the riparian boreal forest?" *by* Stefan W. Ploum et al.

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I have reviewed the draft manuscript: 'Are hydrological pathways and variability in groundwater chemistry linked in the riparian boreal forest?' submitted by Ploum et al for possible publication in HESS. I like the general premise of this study, eg that preferential flowpaths from hillslopes through riparian zones need to be better considered when characterizing the baseflow controls on stream chemistry and dissolved organic carbon availability (DOC). Too many riparian studies are based around 'uniformed' or random piezometer transect designs, and without the hydrological flow context, the groundwater chemistry data are difficult to interpret. I do think this material is appropri-

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Discussion paper

ate for HESS, though the paper could benefit from a change in emphasis and additional heat tracing data that it seems the authors may have already collected.

Answer: Thank you for reviewing the manuscript and providing constructive suggestions to improve the manuscript. We will repeat the comments and provide the answers one by one.

Currently, the primary question addressed by the study is posed as: ‘Are hydrological pathways and variability in groundwater chemistry linked in the riparian boreal forest?’ There have been many studies to document strong variance in groundwater chemistry as controlled by varied advective flowpaths, too numerous to list here. The current study by Ploum and all is unique from many of these as the preferential flowpaths in question source varied dissolved constituents to headwater streams. I suggest the authors refocus the main question to something like: ‘Do DRIP’s represent preferential conduits of DOC-rich groundwater to headwater streams in a boreal forest?’ Or something at least more specific to this study than ‘variability in groundwater chemistry’. It seems that the most compelling data presented in this study show the ‘DRIPs’ in this boreal headwater system are enriched in DOC, which presumably results in higher flux of DOC to the channel via preferential shallow groundwater discharge compared to more diffuse flow through till (though hydrological fluxes are not actually measured or inferred here). Further, the authors document interesting temporal trends in DOC and SpC, the latter being much less meaningful without additional chemical characterization.

Answer: We agree with referee #2 that the most relevant aspect of the DRIP concept is the implications for streams, rather than groundwater chemistry on its own. However, in the line of previous work regarding their riparian function (Kuglerová et al., 2014), the hydrological contributions of DRIPs (Leach et al., 2017; Ploum et al., 2018), and the implications for stream chemistry (Lupon et al., 2019), the aim of this contribution is the characterization of DRIP groundwater chemistry relative to non-DRIP riparian zones. We wanted to represent this specific aim in the title and research question, but as referee #2 understandably remarks the term groundwater chemistry is perhaps too broad. By emphasizing the riparian boreal forest setting, we attempted to steer towards DOC related chemistry however we realize this is not necessarily evident for all readers. We will reconsider the terminology of our title and research question to represent the study outcomes.

In general, I feel the LMM statistical analysis was appropriate for assessing DRIP/non DRIP well DOC, pH, and EC. The results between these binary classifications are interesting, though for all the effort on well installation and sample collection some basic hydrological data seem missing. Where lateral gradients measured between wells and the stream? Were any hydraulic conductivity tests performed? Do we have any idea of groundwater flow rate/flux to the stream from DRIP vs non-DRIP zones? This is a flowpath-based study but the reader is left without any real flow-based hydrogeological information. The addition of some basic quantitative hydrogeological data, and perhaps some additional measured parameters such as dissolved oxygen, could have nicely increased the impact of this study.

Answer: We agree that quantitative data on groundwater hydrology would greatly benefit the study. However currently we have no comprehensive groundwater flux data to quantify flow from DRIPs and non-DRIPs. A few hydraulic conductivity tests were performed under summer low flow conditions in 4 DRIP wells, but we did not deem this representative to use in the study. Instead we based our concept on the assumption that topography and hydraulic gradients are similar in the boreal headwaters. Previous work has shown that continuous saturated conditions occur in the DRIPs and that topographic gradients are low (Kuglerová et al., 2014). Also DEM based flow accumulation model and in-stream measurements indicate that DRIPs provide disproportionally large water contributions, but they remain difficult to quantify and their detectability varies throughout various events (Leach et al., 2017; Lupon et al., 2019; Ploum et al., 2018). We consider the quantification of groundwater fluxes as an interesting future research topic, but it is beyond the scope of this contribution and currently there is no comprehensive dataset available to perform such a study. We will however elaborate on the well installations in the methods and elaborate on the hydrogeological setting.

Without any evaluation of piezometer water age (eg dissolved gas-, isotope-based) the discussion of old vs new water contributions is highly speculative and should be scaled back. I really like the concept you put forth of DRIPs as drivers of younger water fractions in streams where low permeability soils dominate (eg tills), though it is difficult to support this using EC as the primary parameter. Despite these criticisms I do think this work could make an important addition to HESS after some revision considering the major comments below and that of the other reviewers.

Answer: We agree that our young-old water discussion is speculative. In combination with comments of referee #1 we agree to scale back this section of the discussion. Instead we suggest an evaluation of the DRIP/non-DRIP concept with existing concepts of groundwater-surface water interactions (DSL and RIM model (Ledesma et al., 2015; Seibert et al., 2009)).

The statement is made toward the end of the Introduction: 'However, in order to determine whether DRIPs matter for stream biogeochemistry, chemical characterization of the discharging groundwater is needed.' Yes, but this is only half of the equation, the other being the flux of groundwater to the stream which was not evaluated here in any way. It seems at least some data specific to groundwater discharge was collected in these streams and presented by Ploum et al 2018. Where many of the DRIPs instrumented here with piezometers identified in the stream as preferential discharge points using heat tracing? If so that data could be briefly included here with an additional figure, and go a long way to convincing the reader that the wet topographic low points mapped here as DRIPs are actual preferential flowpaths from the hillslope to the stream. Without any such thermal or hydrological gradient/permeability data it is difficult to accept with confidence that the DRIPs mapped here are actual preferential flow zones, compared to the surrounding soils. To be clear I think that the hydrogeologic interpretation of DRIPs made by the authors is likely generally correct, particularly after reading/watching Ploum et al 2018, but the current paper would benefit greatly from some groundwater flow-based data.

Answer: We agree with referee #2 that anomalies in groundwater chemistry without hydrological fluxes have limited meaning for streams. However for this study we argue that there is already sufficient support to assume that the studied DRIPs have important implications for stream chemistry, which would be undetectable if hydrological fluxes were no different than non-DRIP riparian zones. The earlier mentioned work on our DRIP sites showed that the DRIPs provide the majority of the lateral fluxes to the stream using thermal and isotope stream signatures (Leach et al., 2017). Although this study also demonstrated that the fluxes are difficult to match with contributing areas of the DRIPs, biogeochemically the DRIPs alter streams such that observable differences have been reported in gas fluxes as a result of stream processes (Lupon et al., 2019). This leads us to believe that, although we currently have no reported hydrological fluxes of all the studied DRIPs, they can be considered as the dominant lateral hydrological fluxes to the stream. In addition, the hydrogeological information mentioned above will further support that the DRIPs are major hydrological contributions to streams.

I list several more major comments below (I realize some are a bit redundant with this narrative) followed by some more minor text suggestions:

1. A main conclusion of this work is stated as: 'We concluded that hydrological pathways and spatial variability in groundwater are linked, and that DRIPs are important control points in the boreal landscape.' Can you build on this statement in the abstract to be more specific to your study? Near stream shallow chemical variability has long been linked to flowpaths. Perhaps comment more specifically in the abstract regarding the spatial variability you observed to set your work apart from previous studies. The Results section discusses some temporal patterns, but I do not see this data reflected in any of the figures or tables. You could develop a figure specific to the interesting temporal patterns, this information is shown somewhat in Table 1 'TIME' variable analysis but could be shown more explicitly. Also, I think the fact that your research indicates

DRIPs our important DOC pathways to the boreal stream corridor is quite important.

Answer: We will emphasize in the abstract the context and the setting to which our findings apply. Given the specific study site and the generic claim we make, we understand that this can be interpreted in various ways. For the second point we want to clarify that the temporal patterns we refer to are the seasonal differences that are reported as TIME in the analysis (spring, summer, autumn). We will clarify this.

2. Under your definition, are DRIPs only driven by surface topography and wetness? There are numerous instances of preferential discharge of groundwater and interflow through the riparian zone through highly permeable sediments that are not correlated with surface topography, and in glacial sediments often occur at local topographic high points (sand and gravel deposits transecting the riparian zone). I agree that local topographic depressions often lead to saturated conditions in the riparian zone, but that is not the same thing as strong hydrologic connectivity to the stream channel, which depends on the combination of lateral gradient and sediment permeability. Previous work by this group (Ploum et al 2018) used heat tracing methodology to locate/confirm preferential discharge of riparian water to the stream channel, which makes sense. However the current work does not seem to tie the definition of DRIP to actual observed high-discharge points, which I think is unfortunate. Not all saturated depressions will be points of preferential discharge to the channel, which strongly depends on soil permeability. Further, according to your statement: 'water in DRIPs travels a longer distance horizontally; in presumably wet, highly permeable, organic rich soil." It seems your definition of DRIP is relatively narrow, and based around forested headwaters similar to where your study has been conducted. I think it would be quite helpful for you to more specifically define 'DRIP' early in the manuscript (in the Introduction), and acknowledge that this definition applies to only a subset of preferential riparian groundwater discharge zones in headwater systems. Your broad definition of DRIP in section 2.2 (eg 'groundwater discharge zone, groundwater hotspots, cryptic wetlands, swales, focused seepage, discrete seepage, springs, upwelling zones, preferential discharge,

ephemeral streams and zero-order streams') does not seem to apply to the functional definition you apply here for shallow lateral flow above the mineral soil horizon, so please be clear on what definition you are using for this study.

Answer: We agree that preferential discharges not necessarily follow surface topography, and that saturation in topographic depressions does not unconditionally infers hydrological connectivity. DRIPs differ from generic saturated near-stream areas by the large contributing area that they drain. The location of DRIPs in this study were predicted using surface topography and wetness, but the condition is that their contributing area is disproportionally larger than the remaining riparian zone. To ensure that not any saturated area is assigned as DRIP, the predicted DRIPs are also field validated by thermal detection using the DTS system and/or visual inspection (Kuglerová et al., 2014; Leach et al., 2017). Altogether, surface topography is therefore an important aspect of our definition of DRIPs, and we realize that preferential flow to a stream is not always represented by that property. We will put our DRIP definition into context of the studied landscape and also account for systems outside the boreal forest. In addition we will clarify and synchronize the definition of DRIPs in the abstract, introduction and method section.

3. It is not clear to me how DRIPs are defined for 'upland' areas. . . do you use the same definition based on topographic depression and wetness index that you use for the riparian zone DRIP areas?

Answer: Yes, the upland wells in the DRIP transects were predicted based on the same criteria as riparian DRIP wells. The exact location of upland wells of DRIPs were then determined in the field starting from the riparian well following the surface topography in order to approximate the most likely hydrological flow paths. The additional hydrogeological information in the methods should clarify this.

4. Abstract L10 and elsewhere: it is somewhat of a misnomer that all riparian flowpaths lead to biogeochemical alteration of discharging water chemistry. Low-carbon mineral

soils and highly preferential flowpaths such as peat pipes and other macropores can yield little alteration in hillslope and deeper groundwater discharge. In fact your findings indicate that DRIPs lead to less net reaction then more diffuse groundwater discharge through the riparian zone: 'Moreover, DRIPs were chemically more stable from the upland area to the stream'. You might check out this commentary for some relevant discussion : (https://onlinelibrary.wiley.com/doi/10.1002/hyp.11153)

Answer: We agree that the generalization might be too broad. We will reformulate this and we thank the referee for providing the reference to develop our discussion.

5. It would seem fully screened wells down to apprx 1.5 m depth would integrate shallow 'DRIP' water and deeper mineral soil water, how did you account for this?

Answer: The wells were drilled until resistance or until a first aquitard was encountered. Given the exponentially decaying hydraulic conductivity profile, we can assume that the majority of the water is DRIP (or non-DRIP) water. We will explain this in more detail in the material and method section.

I think your statements in the Discussion section regarding 'old' and 'young' water are a bit to speculative, as this is essentially only based on EC data, a parameter influenced by a number of flowpath process. It does not seem like any age dating/isotope analysis was performed, so how confident can you be regarding relative water ages/residence times?

Answer: We agree that our young/old water discussion is speculative, and considering other comments we suggest to downscale this section.

Also, you mention the piezometers were installed until they reached a hard layer. Did this depth vary systematically from DRIP to non-DRIP locations? If so you could include that data, as depth to rock/confining layer can also be a strong control on shallow groundwater flowpath chemistry.

Answer: We will elaborate on this in the material and method section. Typically the drilling was limited by large pebbles and not bedrock. This was around 1.2-1.5 meter depth.

Fig 1: Although we might expect local shallow percolation in non-DRIP near stream zones, groundwater flow is likely dominated by the lateral component toward the stream (in gaining stream reaches), though the discharge magnitude may be reduced compared to preferential discharge points. I suggest you alter the 'vertical groundwater flow' language in the 3rd panel of this Figure, the vertical flow you refer to may instead by non saturated percolation toward the water table, where groundwater flow is predominantly horizontal.

Answer: We agree that non-saturated percolation is a more appropriate term for this process and will change the figure accordingly

Have you measured any vertical head gradients at the wells, and lateral gradients between wells, to support these conceptual diagrams?

Answer: We will provide typical groundwater level timeseries in a broader hydrogeological section in the materials and methods. We don't have current head gradients to present complimentary to the conceptual figure.

Fig 3- The caption could be simplified, you do not need to define DRIPs in the caption as this is done in the text

Answer: We will change the caption

Minor points:

Pg 2: L2 repeat of the word 'landscape', please look for replacement

Answer: We will rephrase the sentence

L3 I am not sure what you mean by 'newly introduced water', can you be more specific?

Answer: We mean event water, we will clarify the sentence

L19 do you have a reference example to associate with: 'Traditionally, streamflow generation has often been assumed to be driven by spatially diffuse groundwater exchange often released at a constant rate.'?

Answer: We will provide references for this statement

Pg 4 L20: could cite here the hydrographs shown in Ploum et al 2018

Answer: We will provide the reference and also elaborate more on the hydrogeological setting

L24: you would not consider the fall period to be 'baseflow' dominated as well or is this just a winter condition in your watershed system?

Answer:We considered baseflow as the flow conditions in winter period given the snow dominated region we performed our study. In autumn low flow conditions can occur but not as long and consistent as during winter due to regular rain events. In winter snow and soil frost inhibit flow in the shallow subsurface and 'true' baseflow conditions occur.

Pg 7 L13: replace 'double as high'

Answer: We will rephrase the sentence

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