Response to the interactive comment on the manuscript

"Event and seasonal hydrologic connectivity patterns in an agricultural headwater catchment"

by Lovrenc Pavlin, Borbála Széles, Peter Strauss, Alfred Paul Blaschke, Günter Blöschl

In the following document we reproduce all the comments of the Referees in *italic characters* followed by our answers.

Anonymous referee #1:

General comment

In this manuscript, the authors analysed the hydrologic connectivity of an agricultural headwater catchment in Lower Austria, by comparing the temporal dynamics of groundwater table and soil moisture measured at different sites (i.e., riparian zone, lower, mid and upper hillslope) with the streamflow response. Spearman correlation coefficient, a hysteresis index and peak-to-peak time were used for the analysis of the similarity of the response of the various groundwater and soil moisture sites with the streamflow time series. Results showed a similarity of groundwater to streamflow, with a spatial organisation suggesting a decreasing degree of connectivity to the stream from the riparian zone up to the hillslope. The soil moisture similarity pattern was spatially more homogeneous. Event characteristics were found to be a secondary control for groundwater, but a primary control for the soil moisture similarity to streamflow. The authors also showed that the seasonal and the event similarity patterns can be quite different, indicating that hydrologic connectivity might change depending on the temporal scale used for the observations. The topic of this manuscript is potentially interesting for the readers of this journal.

> We want to thank the Anonymous referee #1 for his/her positive and insightful comments on the manuscript. We have carefully considered and addressed all his/her comments in the following.

Overall, the paper is well structured, but I think that there are too many figures and some of them could be merged (e.g., Figs. 7, 8 and 9 or Figs. 10 and 11).

> We are aware that the number of figures is high. We feel that figures like Fig 7.,8, 9, 12, 14, 15 and 16 are complex on their own and combining them would ge confusing for the reader. In order to answer the issue raised, Figures 10 and 11 will be combined into one.

Furthermore, some methodological details are not clear (e.g., a sketch/scheme for the hysteresis analysis would be helpful to understand how the index was computed) and should be better explained.

> In order to make the methodolical details more clear, the method of calculating the hysteresis index will be added to the appendix.

Specific comments

- Page 5, lines 117-124 and Table 1: The installation depths of the various piezometers vary a lot; do the piezometers reach the bedrock or an impermeable layer? Are there any conductivity data for the various soil layers and for the bedrock?

None of the piezometers reaches the bedrock as it is about 200 m deep based on unpublished seismic measurements. Stations Hxx and BPxx were drilled using a hammering rig to refusal which normally corresponds to the depth of the first consolidated lignite layer, which is less permeable than the soils above it. Unfortunately, no more details are available about the drilling and installation of these stations. Stations Gxx penetrate into but not through all the lignite layers.

Picciafuoco et al. (2019) used double-ring infiltrometer measurements to determine the arithmetic mean saturated hydraulic conductivity of 46.9 mm h^{-1} (st. dev. 33.5 mm h^{-1}) and 20.2 mm h^{-1} (st. dev. 20.3 mm h^{-1}) for arable land and grassland respectively. Unpublished results of pumping tests at the stations G2, G3, G4 and G6 indicate that the unconsolidated lignite layers have a hydraulic conductivity 10-100 times higher than the overlaying silt loams. More detailed hydraulic conductivity information is unfortunately not available.

- Pages 6 and 7: I suggest to provide more details about soil moisture sensors (e.g., brand and model), and how the authors performed an interpolation of soil moisture data at a 15 min interval when the recording interval was 1 hour (line 132). In addition, I am wondering about the sensitivity of the average soil moisture to the number of sensors used for the computation (I would be careful when calculating average values using a different number of sensors every time, particularly if there is a high variability in soil moisture along the profile). Were soil moisture data calibrated based on the soil type?

We will add the details about the brand and model of the soil moisture sensors. We will also add that soil moisture data were interpolated using linear interpolation for the convenience of calculation.

We understand the concern about the sensitivity of average soil moisture on the number of sensors used. Therefore we performed a sensitivity analysis on the average soil moisture by a leave-one-out method. The expected mean absolute errors of the calculated average soil moisture are 0.001 $m^3 m^{-3}$ and 0.007 $m^3 m^{-3}$ for one (35% of data used) or two missing (2.9% of data used) sensors, respectively.

Soil moisture sensors at each station and each depth are calibrated using gravimetric soil samples to convert the sensor outputs to the volumetric soil moisture content.

- Pages 7 and 8, lines 168-173: The fourth condition is not very clear, and I do not understand why the authors considered a specific recession period of 48 hours (why not 24 hours or more than 48?). Furthermore, it is not clear whether the authors also analysed the streamflow response (e.g., larger than a certain threshold) or just focused on the rainfall characteristics and the groundwater/soil moisture response. I also suggest to clarify whether all the mentioned conditions should be met for the identification of the rainfall-runoff events.

The definition of rainfall-runoff events is a stepwise process, that starts with the identification of rainfall events – i.e. steps 1-3. The rainfall-runoff event starts with the rainfall event but continues after the rainfall has stopped until streamflow returns to baseflow or a new rainfall event starts. Here we did not choose to determine the recession time on a per-event basis but rather took a fixed recession time of 48 hours. We choose this recession time as it is long enough to always cover the recession streamflow and the longer rise times of groundwater. Longer recession time would include more of the long groundwater responses but also more of the daily fluctuations in streamflow and groundwater that can mask event responses. We will rewrite the rule to make it clearer.

We did not impose any thresholds for streamflow response as other thresholds (e.g. rainfall depth and the existence of groundwater or soil moisture response) already implicitly ensure a sufficient streamflow response.

- Page 10, lines 209-220: It is not clear which variable is x and which one is y; the authors should consider adding a sketch of the event hysteresis in the supplementary material along with the value of HI. Did the authors consider complex loops, such as eight-shaped hysteresis? If so, how did the authors identify the complex loops?

We think this is an excellent suggestion. We will add a sketch and a longer explanation of the calculation of the hysteresis index in the appendix. We did not consider complex hysteresis loops explicitly. With our method we can calculate the hysteresis index of any shape of the hysteresis loop but based on index alone we cannot differentiate between simple and complex loops. E.g. hysteresis index of an eight-shaped loop would be close to zero and we cannot differentiate it from a very narrow loop. This is also the reason why we also consider the Spearman correlation coefficient and peak-to-peak times together beside the hysteresis index.

- Page 12, lines 260-271: I think these lines about the response types belong to the Results; I suggest to move them to the following sections.

We will move these lines to the Results, to section 3.2 Response type occurrence.

- Figures 10 and 11: Details about the computation of the local regression fits (e.g., software used for the fits and statistical significance of the regressions) should be reported in the Methods.

We will add this information to the Methods.

- Page 18, section 3.2.1: An explanation of the term co-occurrence would be useful to understand the findings reported in this section. I also suggest to report all the co-occurrence values as percentages, in order to be consistent with Fig. 12.

We will add an explanation and change the ratios to percentages.

- Tables 3 and 4: It is not clear which Pearson correlation coefficients are statistically significant and which are not. The authors should integrate this information into the tables and the captions.

We will follow the suggestion and add the p-values to the tables.

- Page 24, lines 461-464: I suggest to the authors to consider the recent findings by Klaus and Jackson (2018) and Gabrielli and McDonnell (2020), and, if possible, check whether they can really consider their riparian and hillslope sites connected to the stream. Are there bedrock permeability data for this agricultural catchment?

The findings of the suggested articles are interesting. However, we are not sure if their index could be easily transferred to our catchment. We do not have a clear impermeable layer on which the water would perch but rather a series of thin, dry and possibly cracked consolidate lignite layer and very wet unconsolidated layers. Nevertheless, we will include these two references in the discussion to broaden the argument.

- Figures 7, 8, 9, 12, 14, 15 and 16: These plots are very nice and colourful, but some of them (particularly the ones reporting the hysteresis index values) are quite confusing because the same relations are reported twice with a different colour. To improve the readability of these figures, I suggest to remove the lower or the upper part of the figures.

We were already considering removing part of these figures but decided against it as a sign of the hysteresis index and peak-to-peak lag time changes with the direction of reading (column- or row-wise). We find it easier to be able to read the relationship of a station to another station by following one row or column, therefore, the readers do not have to mix both.

Technical corrections

- Page 1, line 22: "hydrologic" instead of "hydraulic".

We will replace this word with the suggested one.

- Page 2, lines 45-48: I suggest to modify as follows: "Penna et al. (2015) and Detty and McGuire (2010) found that wetter antecedent conditions and higher rainfall depth increased groundwater peaks, the number of activated wells and the spatial extent of the subsurface flow network in a steep catchment in the Italian Alps and in a forested catchment in New Hampshire, respectively."

We will replace the sentence as suggested.

- Page 3, line 69: missing space in "moistureand".

We will add the space.

- Page 4, caption of Figure 1: Please report a brief description of the Topographic Position Index in the caption.

We will add a brief description of the Topographic Position Index in the caption.

- Page 7, line 168: I suggest to change "by following five rules as follows" with "based on the following five conditions".

We will change the sentence as suggested.

- Page 10, line 218: I think that the magnitude of HI indicates more the "fatness" than the "shape" of the loop.

We will change "shape" with "size" of the loop.

- Figure 5: I suggest to increase the resolution and size of the figure.

We will replace the figure with a bigger one with higher resolution.

- Page 15, line 313: It should be “compared” and not “compare”.

We will replace this word with the suggested one.

- Page 16, line 322: the symbol ± is missing between 2.7 and 14 hours.

We will add this symbol.

- Page 27, line 568: Please replace “similarly” or “similarity”.

We will replace “similarly” with “comparably”.

References

Gabrielli C.P., McDonnell J.J., 2020. Modifying the Jackson index to quantify the relationship between geology, landscape structure and water transit time in steep wet headwaters. Hydrological Processes, early view. DOI: 10.1002/hyp.13700

Klaus J., Jackson C.R., 2018. Interflow is not binary: a continuous shallow perched layer does not imply continuous connectivity. Water Resources Research, 54, 59215932. DOI: 10.1029/2018WR022920372, 2020.