Hydrology and Earth System Sciences Discussions

Interactive comment on "Transient analysis of fluctuations of electrical conductivity as tracer in the streambed" *by* C. Schmidt et al.

C. Schmidt et al.

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Reply to Referee#2

We are grateful for the constructive comments and suggestions of Referee #2. We have carefully considered all comments. Detailed responses to all individual comments by Referee #2 are given below.

General comments: This paper presents an improved method for inferring hydrologic travel times from the propagation of electrical conductivity signals from a stream channel to the shallow subsurface. The method is demonstrated with a small dataset from 2 wells installed near a gravel bar in a river subject to dynamic flow from mill use and

storms. The authors interpret the effectiveness of the method by comparing their inferred travel times to patterns in hydraulic head data collected in the wells and the stream adjacent to the wells. My primary criticism is that there are several interpretations of the hydraulic gradient throughout the paper that appear to be inconsistent with data in figure 3.

Figure 3 is not presented as hydraulic gradients, but seems to be interpretable as gradients since the heads in the wells are "normalized" to the stream surface elevation (see specific comments). If the authors are going to make interpretation about hydraulic gradients, they should present the data they are interpreting, and check that those data are consistent with the patterns pointed out in the text. Unfortunately, several interpretations appear to be invalid as currently presented. Despite the apparent usefulness of the method, this substantially detracts from the credibility of the paper.

It appears that the warping technique is useful for automating the process of cross-correlation. However, it does not appear that the ultimate outcome of the analysis is any different from the sliding window cross correlation mentioned near the end of the introduction, and simply removes the "visual" component of that analysis of cited work by Boker et al. (2002). I can see how a more automated and objective method is a contribution, but it needs to be made clear if the results of the method are somehow different from the existing methods of cross correlation, especially if they need to be interpreted in a different way. Based on my understanding of the methods section, it does not appear to me that the results of the method should be any different than the more manual approaches to cross correlation. If it turns out that the results of the method are identical to other cross correlation methods, then the authors are introducing a large amount of analytical jargon with very little return in real understanding. See specific comments regarding suggestions to bring the methods back into the hydrologic context at some point.

The true contribution of the paper appears to be a method that could lead to a more standardized approach to cross correlation of time series signals in hydrologic study. I

like the idea of that, and a short paper like this could be a valuable contribution. It simply needs to be made clearer how results of the new method are comparable to previous methods and it needs to be made specifically clear what value is being added by the new method (e.g. automation, objectivity, etc.). The interpretations and comparisons with patterns in hydraulic gradient also need to be made clearer before publication.

The reviewer raised two main points: 1) The unclear interpretation of hydraulic gradients 2) What is the difference between sliding window cross correlation and windowed dynamic time warping

1) We realized that the presentation of our data was unclear. We transform our water level and head data using meters above mean sea level (m a.s.l) as a reference datum and not as we did the streambed surface. Please also find our response to the specific comments below

2) Taking the correlation coefficient as distance measure in windowed dynamic time warping, the resulting distance matrix is the same as for windowed cross correlation. However, both methods start from different points. Cross correlation is typically applied for the entire data series whereas windowing allows to estimate the cross-correlation function for subsequences of the data. DTW was originally developed for element wise alignment of series. Aligning subsequences of the data series can be viewed as a kind of upscaling from the element perspective, while windowed cross correlation represents a downscaling from the "entire-series" perspective. Windowed DTW has some advantages compared to windowed cross correlation. -The distance measured is not limited to the correlation coefficient and can be selected based on the individual problem. - In cross-correlation window size and the maximum lag depend on each other. Searching for larger lags requires larger windows. In DTW analysis they are independent since the optimal distance is found by searching the entire distance matrix rather than only the subsequences in the window.

Specific comments: Page 6346 Lines 11-12: Would it be clearer to say that "head

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gradients across the gravel bar were lower"? The word "leveled" is awkward here.

According to the reviewers comment we rephrase the sentence to: Our analysis revealed that the travel-times increase at higher stream flows because lateral head gradients across the gravel bar become significantly smaller at those time.

Page 6347 Lines 8-13: Confusing paragraph. Should the characteristics of water that can be used to estimate travel times be mentioned before the fact that these characteristics need to be measured at intervals shorter than the travel times of interest?

Yes, we agree and will rephrase the paragraph accordingly:

Time series of temperature (e.g. Constantz 2008) and electrical conductivity (Cirpka et al. 2007) have been used to study spatial patterns and temporal variations of groundwater surface water interactions. The growing use of heat and EC as natural tracers has also been facilitated by the increasing availability of commercial sensors and data loggers that allow the simple collection of highly resolved temperature and EC time series. Detecting temporal variations of water flow in the field requires measurements at a time interval shorter than that of the temporal variation of interest

Page 6347 Lines 14-18: These statements could easily be construed to be in conflict with the first sentence of the abstract, which reads "Magnitudes and directions of water flux in the streambed are controlled by hydraulic gradients between the groundwater and the stream"

We will reformulate the abstract to resolve the conflicting statements:

Spatial patterns of water flux in the streambed are controlled by the distribution of hydraulic conductivity, bedform-induced head gradients and the connectivity to the adjoining groundwater system. The water fluxes vary over time...

Page 6348 Line 4: Can the statement that "EC is practically a conservative tracer" in hyporheic flow paths be supported by the literature? I can think of several biogeochemical/ weathering reactions that could change dissolved solids and thus EC in a

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nonconservative fashion, at least under certain scenarios. However, this statement appears to be a very general conclusion with no references.

Indeed this statement is very general. We realize from this comment that our statement is misleading since a similar question was raised by Referee#1 . We had in mind that EC is a better natural tracer than heat because variations of EC propagate further into the subsurface then temperature fluctuations.

We will rephrase this paragraph in the revised version of the manuscript and refer to our reply to Referee#1.

Page 6349 Line 16: Conduction with the matrix also acts like "sorption" of heat, such that the lag of temperature signals can be substantially retarded relative to the movement of water (not just damped).

This is indeed an interesting point. The propagation of the temperature signal into the sediment is associated with a damping of the amplitude and a phase shift of the signal. (see Keery et al. 2007 and Hatch et al. 2006). The phase shift is related to the advective and conductive transport of heat and the time lag that occurs between to observation points in space. The phase shift also occurs for equilibrium heat transport.

We think that retardation caused by local thermal non-equilibrium is negligible in our system and streambed sediments in general. The use of the local thermal equilibrium assumption for thermal conditions and flows typically found in saturated porous media has been justified by Levec and Carbonell (1985) and recently also by Rau et al. (2012). Vogt et al. 2010 (a) estimated the characteristic time for conductive heat transfer into spherical grains. For a grain diameter of 2cm it takes approximately 9 s to equilibrate to a temperature change of 1K. This timescale is shorter than the period of diurnal temperature variations which are typically evaluated in heat tracing studies.

We will rephrase the paragraph to include the phase shift. We will also add a sentence to better explain the heat transport. However, we will not go into detail about the theory

of heat as a tracer as this is not within the scope of this paper.

Tracing diurnal temperature variations has become a well-established method for assessing stream-groundwater exchange. The amplitude damping and the phase shift between the diurnal temperature signal in the stream and some depths in the streambed is related to the magnitude and direction (upward or downward) of water flow (e.g. Constantz 2008). However, diurnal temperature signals are strongly attenuated in sediments as conductive heat exchange with the matrix of the porous medium results in significant damping of the signal. Particularly for gaining conditions the diurnal temperature fluctuations do not propagate deeper than 0.2 m (Keery et al. 2007).

Page 6349 Line 17: Somewhere in this discussion, it might be a good idea to indicate whether "EC" is meant to mean temperature-corrected EC or not. Temperature dependence of EC was mentioned in the introduction, but the definition of EC as "representing concentration of solute ions" is only true under constant temperature, unless EC is specifically defined throughout this paper as the temperature-corrected electrical conductivity.

All EC values are temperature corrected as stated in the "Experimental setup and data collection" section. We see that this was not stated early enough and we will change the sentence in the in the theory section.

In contrast EC, a parameter related to the concentration of solutes (ions) in the water, shows much less attenuation.

Page 6351 line 25: I think I know what is meant, but "features" of the time series is never defined. Based on the following sentence, I assume the authors mean "features" like sharp inflections, maxima, or minima in the time series signal?

We agree with the reviewer that "features" should be better explained and will add a sentence as suggested:

The window length should be selected in a way that it contains a sufficient number

of "features" like sharp inflections and local minima and maxima to ensure a clear minimum in the function for the distance measure at perfect alignment.

Page 6352 line 15: In the end, this appears to be fundamentally the same thing as a sliding-window cross correlation analysis, where the ultimate outcome is a time series of the minimum lag times that produce the highest cross-correlations between windows of the signal.

The only difference appears to be the addition of a recursive algorithm to automate finding those lag times for a given pair of signals (hence generating what the authors call a "minimum cost path" through the matrix of cross correlations defined by those lag times). While maybe useful to understanding the details of the method, the substantial jargon like "minimum cost" and "warping" used throughout this section are not particularly useful in the hydrologic context.

At the end of the methods, I suggest the authors briefly revisit how all these "minimum costs" and "warps" result in data that are meaningful to interpretation of hydrologic transport times, especially if the reader should somehow be interpreting the results differently from a sliding-window cross correlation mentioned earlier in the paper.

This specific comment is related to one of the main issues raised by the Referee. We agree that for searching the minimum cost path through a sliding window cross correlation matrix basically the same recursive algorithm can be used. This can indeed be an extension to the visual interpretation of windowed cross correlation matrices. Dynamic time warping offers more flexibility since the lag time and the length of the window are independent. For a more elaborate explanation we refer to our earlier reply to the general comments of Referee#2.

We will add a paragraph at the beginning of the discussion section to better explain the difference between DTW and windowed cross correlation and to discuss the application of the minimum cost path to cross correlation matrices.

By using the correlation coefficient as distance measure in the proposed sliding window DTW, the resulting distance matrix is basically the same as for windowed cross correlation providing the windows are only shifted by one single time step. However, both methods start from different points. Cross correlation is typically applied for the entire data series whereas windowing allows to estimate the cross-correlation function for subsequences of the data. DTW was originally developed for element wise alignment of series. Aligning subsequences of the data series can be viewed as a kind of upscaling from the element perspective while windowed cross correlation represents a downscaling from the "entire-series" perspective. Despite this similarity DTW has particular advantages compared to cross-correlation with sliding window. First the distance measure is not limited to the correlation coefficient and can be selected based upon the individual problem. The conventional measure is Euclidean distance for instance. Second in cross-correlation with sliding window the window size and the maximum lag depend on each other. Searching for larger lags requires larger windows. In DTW analysis they are independent since the optimal distance is found by searching the entire distance matrix rather than only the subsequences in the window. In DTW the time series are optimally aligned by finding the minimum cost path through the distance matrix. Thus, the transient time shift that is required to optimally align to time series is also inherent in the minimum cost path. As such this provides an automated method to detect the time lags between two signals. A similar algorithm could be used to automatically extract the optimal alignment from a windowed cross-correlation matrix.

We want to make clear that the method-related terminology like "warping" and "minimum cost path" are established terms although from a different field than hydrology. They are required to describe the method. We agree that to better assist the reader how to apply the method and how the results can be interpreted some more explanations are required. This will be added at the end of the methods sections as suggested by the referee.

The implementation of the algorithm leads to a distance matrix where the correlation

coefficient can be visualized as a function of time and the time lag at this time. The local time difference from the minimum cost path to the unity (to zero time lag) can be interpreted as the local time lag between the two EC signals.

Page 6354 line 1: For the purposes of this paper, this should be part of the definition of EC earlier in the introduction, such that it is clear that discussion is always about temperature-corrected measurements throughout. Several earlier statements about the interpretation of EC in terms of solute concentrations are dependent on the fact that the authors are referring to temperature-corrected measurements.

We agree, please see the reply on an earlier comment. We also rephrase the sentence in the data collection section.

Measured EC values are internally compensated for temperature to derive specific electrical conductivities normalized to 25 °C. Throughout the paper we will refer to the temperature-compensated EC.

We will also add a sentence on the temperature corrected EC in the Introduction section at the beginning of the paragraph p 6347 l. 19: Natural fluctuations of water temperature and EC can be used as a tracer for flow in the streambed and thus provide a direct estimate of travel times. It is important to note that EC in this paper always refers to the temperature compensated electrical conductivity.

Page 6354 line 6: "Normalized to the streambed surface" is a confusing way to say we are looking at the head difference between the stream and the well at each site. The red and green lines appear to be "differences in hydraulic heads" rather than "hydraulic heads". Might also be worth a brief mention of the sign convention used. I presume, as is traditional, that positive numbers imply gaining and negative numbers imply losing.

From the comment we realize that our way of presenting the hydraulic data is misleading. Figure 3 shows no head differences only heads. The reference elevation is the streambed surface which is set to be 0. Hence, the head difference is the difference

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between the blue and the red or the green line respectively. We will change the figure and also the text to [m a. s. l.]. Please also see our earlier comment. Head differences are positive upward. This will be better indicated in the revised version

Page 6354 line 7: Losing conditions do not appear to be "prevalent" in the upstream site in this dataset. It appears to be near neutral to gaining for about half this time period.

This comment arises from our unclear presentation of the data. We refer to or reply above

Page 6354 line 7: Was this normalized to some sort of sliding window mean? The signal does not look stationary enough for the global mean to be very useful for this purpose.

We suppose the Referee refers to line 15-20 on the same page. The mean value of EC described here is the mean of the entire series. The purpose of presenting the global mean is to show that the streambed is essentially influenced by the stream water. This becomes evident when comparing stream and streambed EC to groundwater EC which is approximately twice as high. We realize that the information on the mean EC was confusing and will move the paragraph to a later position in the results section

Page 6355 line 10: Not clear what relative scale is being used when referring to the lag times as "short".

We agree and simply removed the sentence. It is not needed

Page 6355 line 24: I am not seeing these patterns in Figure 3. Am I looking in the wrong location? To me, it appears that the hydraulic gradient is > 0 at both locations for nearly all analyzed times after the 25 August spate.

This comment arises from our unclear presentation of the data. We change figure 3. In the revised version all head data will be presented in [m a.s. l.]

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Page 6355 line 28: Again, in figure 3, the head response at the upstream and downstream site appear almost identical. Where are these interpretations of the hydraulic-gradient coming from?

This comment arises from our unclear presentation of the data. We will change figure 3. In the revised version all head data will be presented in [m a.s. l.]

Page 6356 line 2: Does "length" here refer to distance or time? Similarity in transport times would only suggest similarity in flow path distances if the hydraulic conductivities were also similar.

The flow path lengths refers to distance. The hydraulic conductivities are within a narrow range around 10-4 ms-1. We change the sentence to include this fact:

The similar time lags between the stream and both, the USS and DSS, suggest similar flow path lengths from the stream to the sensors, providing that the hydraulic conductivities are similar.

Page 6357 line 7: Where do these assumptions come from? I am not aware of anisotropy of 3 being a common assumption, and I'm much more used to seeing a number more like 10. The authors need to support these numbers somehow if they intend to make interpretations from the data derived from them.

This paragraph is obviously not clear since Referee#1 raised similar questions.

The paragraph compares the vertical velocities estimated from the time lag of the EC signal and the depth of the EC sensors with independent Darcy based flow velocities. It shows that the results are comparable with Darcy based estimates and EC might be even the better choice for short term variations of water levels. However, will remove the information on Darcy based velocity estimation since they are partly based on assumptions. Further this information is need essentially needed for the message of our study.

Page 6357 line 10: How does similarity in these numbers necessarily indicate a vertical

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component and how is conclusion about a strong vertical component not simply an artifact of the assumption of 3 being the ration of anisotropy in conductivity?

The section will be removed in the revised manuscript please see our earlier comment

Page 6357 line 15: A bit strange to not mention this until after all the assumptions earlier in the paragraph are given and interpretations made from the resulting data. What is the point of this paragraph?

The section will be removed in the revised manuscript please see our earlier comment

Figure 1: How is the minimum cost path indicated? The white line?

Yes, the white line. We will indicate this by adding the information to the caption of 1b)

Figure 4c: What "double peak" is this referring to? Can this be indicated with an arrow in the figure?

We will add an arrow to indicate the double peak in the revised figure

Typographical comments: Page 6346 Line 4: Appears to be a typo in wording of ": : :driven by for instance by::"?

Will be changed to: ...driven for instance by short term...

Page 6346 Line 19: Omit comma after "both". This will be changed

Page 6346 Line 24: Appears to be word missing, "direction of flow"? We will change the sentence to: Changes of flow velocity and flow direction in the streambed can be induced by flood events... We also refer to our reply to Referee#1

Page 6351 line 23: Omit "by" in ": : :minus by the window length: : :".' This will be changed

Page 6355 line 28: Omit comma after "both". This will be changed

Page 6356 line 1: There are a lot of unnecessary and confusing commas in this paper.

Omit comma after "both" and after "DSS".

We will check all commas in the revised manuscript and improve the use of commas.