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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.

Anonymous Referee #2

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Review of "Transient analysis of fluctuations of electrical conductivity as tracer in the streambed" by Schmidt et al.

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.

Specific comments:

Page 6346 Lines 11-12: Would it be clearer to say that "head gradients across the gravel bar were lower"? The word "leveled" is awkward here.

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?

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. . ."

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

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).

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.

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?

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.

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.

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.

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.

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.

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

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.

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?

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.

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.

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

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?

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

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

Typographical comments:

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

Page 6346 Line 19: Omit comma after "both".

Page 6346 Line 24: Appears to be word missing, "direction of flow"?

Page 6351 line 23: Omit "by" in "...minus by the window length...".'

Page 6355 line 28: Omit comma after "both".

Page 6356 line 1: There are a lot of unnecessary and confusing commas in this paper. Omit comma after "both" and after "DSS".

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