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Hydrology and Earth System Sciences Discussions

Interactive comment on "Mesoscale connectivity through a natural levee" *by* A. E. Newman and R. F. Keim

A. E. Newman and R. F. Keim

anewma9@tigers.lsu.edu

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We sincerely appreciate the feedback from both reviewers and anticipate substantial improvements in the manuscript as a result of their careful evaluation.

Response to Reviewer #1:

*On the definition of hydrologic connectivity, our intention was to provide a broad context for the study and then pare down to the specific objectives. In order to alleviate confusion, we have included a specific definition of connectivity in the first paragraph.

*p. 7763 line 12 – The typo has been corrected so that 'hydrologic' is now 'hydraulic'.

*p. 7764, line 10 – We made this distinction to differentiate our situation from those in which connectivity is thought to occur along discrete flowpaths of saturation in the vadose zone. (i.e. Tromp-van Meerveld and McDonnell, 2006) We have edited this statement for better clarity.

*7767, line 26- The approximate depth of a solid clay layer was at 2 m, so this made a convenient boundary (the soils are fluvial deposits, and bedrock is hundreds if not thousands of feet deep). Also, this was the greatest practical depth for manual augering. We have clarified this in the manuscript.

*On well screens – We had a limited number of pre-made well screens, so we used nylon stockings to screen additional wells. Nylon stockings are commonly used as screens in fine grained systems, and we did not observe differences between the two types of screens during experiments or in the amount of sedimentation that occurred in wells. We have now noted the functional similarity of the two types of screens in the manuscript.

*p. 7773, lines 20-23 – The opposite responses of EC and chloride in well 2 are still an enigma to us. We address potential explanations in the discussion (7776, lines 13-21).

*p. 7774, lines 7-10 – We were trying to avoid confusing readers by identifying all of the cases that did not behave in a way that agreed with our model, but we have now added some specific examples of the nonbehavioral cases. We have also expanded on potential reasons for these differences in behavior in the discussion section.

*p. 7776, line 26-27 – Because of the scale of our experiments, measurements are more likely to intercept preferential flowpaths. Whereas traditional K estimates are calculated at the borehole scale, our measurements are made within a large volume of natural levee, and we hypothesize that variability of flowpaths is relevant at this scale. We have described this logic in the second paragraph of the introduction and in the last paragraph of the discussion.

*p. 7777, lines 5-6 – In our low-gradient system, even calculating likely surface flowpaths would require very high-resolution topographic data that was not and is not available to us. Estimating subsurface flowpaths is still more challenging, because the direction of flow depends on very small hydraulic gradients as well as both texture and structure of the soils (including macropores created by root channels, animal burrows, etc.). Therefore, we chose a simplified approximation of pathlengths and acknowledge the potential error associated with this method.

*p. 7777, lines 22-24 – We agree that this information may be more helpful up front and have moved the paragraph to the introduction.

*On the effect of the size of the natural levee (berm) on results: we have addressed this more clearly in our edited description of the importance of scale.

*On quantification of antecedent moisture: A simple antecedent precipitation index is not necessarily applicable in our system, because soil moisture is influenced not only by rainfall but also by wind and tide conditions as well as water level in the adjacent stream channel, which in turn is influenced primarily by water level in the Mississippi River rather than local precipitation. Therefore, probably the most reliable way to quantify antecedent moisture would have been to measure it; however as you pointed out previously, shallow water table measurements provide a satisfactory surrogate in most cases.

*Linking water table and tracer dynamics with individual rainfall events: As discussed above, rainfall is not necessarily the primary driver of antecedent moisture conditions in this environment, so we direct our comments toward linking our observations with antecedent moisture. Clearly, differences in soil moisture and water table between the two experiments affected both the propagation of the pressure wave and individual flowpaths of tracers, which we address in paragraphs 2 and 3 of the discussion section. It is also clear that the number of experiments provided by this work is insufficient to draw correlations between antecedent conditions and connectivity mechanisms; how-

ever, the two experiments we conducted do provide evidence of preferential flow under both comparatively wet and dry conditions, so the effect of variable environmental conditions on the magnitude of preferential flow through natural levees is a potential direction for future work. We have made this note in the aforementioned portion of the manuscript.

*Spatial correlations of topography and stratigraphy with hydrochemistry. Although we did measure these things for a more general understanding of our system, we did not have enough data to investigate correlations of specific features with specific behaviors. Indeed this is an intriguing topic for future research, but it was not within the scope of this study.

*Thank you for pointing out discrepancies in the references cited. We have revised them.

Response to reviewer #2:

*On the comparison of pressure and tracer transport: As discussed in section 4.2, it is well known that pressure transport differs from tracer transport, but understanding their variation is key to understanding connectivity, and there is a distinct lack of data. Further, we acknowledge that the derivation of hydraulic conductivities (K) from mean residence times of pressure deviates from standard groundwater hydrology practices. A more standard way to present these results would be in terms of aquifer diffusivity, which requires some prior knowledge of aquifer properties that are unknown, so estimating properties of the natural levee system in this way would require making assumptions with regard to effective porosity and aquifer thickness that would be difficult to defend. In order to avoid any misinterpretation of K values derived from pressure transport velocities, we have removed all references to pressure-derived K from the manuscript. The main analysis method, which is calculation of residence times, provides a better metric for evaluating variation of transport and thus supports our conclusions.

*Effect of unsaturated flow: As discussed in paragraphs 2 and 3 of section 4.1, antecedent moisture conditions certainly affected transport mechanisms. However, we modeled residence time distributions during a period when the reservoir contained surface water, and this occurred after the soil beneath the reservoir became satiated (if not saturated). In the absence of soil moisture measurements, saturation of the soils is the best assumption we can make, and if there were deviations from this condition, these were likely insignificant given the scale of measurement. We have included this discussion in paragraph 3 of section 4.1.

*The sensitivity of calculated K [derived from tracer velocities] to estimated pathlengths is discussed in the last paragraph of section 4.1.

*In field experiments, there really are no "replications," and this is certainly the case for our situation. We acknowledge differences in hydraulic conditions between the two tests on p. 7769, lines 3-12. Both active and passive tracers were conservative, and we expect them to measure the same process in the same way and thus to behave similarly under similar conditions. We have added this language to the manuscript to make our intentions more clear.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 7761, 2012.