

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

Anonymous Referee #2

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The overall aim of this research is to characterise in situ subsurface flow exchange rates between channels and floodplains through levees, which are common geomorphologic features that can be important for biogeochemical cycling in river systems. This study case seeks to: (1) quantify the spatial variability of mean residence time (MRT) and velocity of water through a natural channel levee, as well as the inferred hydraulic conductivity (K); and (2) compare methods to achieve such a goal, namely continuous head time series, environmental tracers (here, electrical conductivity and chloride), and artificial tracers (here, potassium chloride). The hypothesis is that if the channel and floodplain are disconnected in terms of surface flow, they remain connected in the subsurface. The study is based on measured hydraulic heads and tracer data at a 20 m experimental reach equipped with a dense network of wells. During

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the experiment, a constant hydraulic head was imposed artificially on the external side of the levee, by submerging a 300 m² surface area (the “reservoir”), while subsurface data were collected in wells located on the crest of the levee, between the reservoir and the channel.

The manuscript is well written and relies on an interesting field experiment. However, I am afraid that I cannot recommend this work for publication in HESS, as it contains substantial weaknesses, and as findings cannot really be considered “new”.

Most importantly, a problem lies in the comparison between “pressure transport” and solute transport, the corner stone of the paper. Such a comparison has little meaning, as pressure wave propagation is not advection. It is like comparing the high velocity of a wave propagating at in a canal at the surface of the water with the low velocity of the water flow, and concluding that there are orders-of-magnitude differences. The difference is to be expected (as somewhat acknowledged in the introduction). Actually, what the authors call pressure transport is the result of “aquifer diffusivity”, an important concept in subsurface hydrology, but that is mentioned nowhere in the paper, and the misconception of which leads to hydrological aberrations. For example, the hydraulic conductivity value of 1000 km/d (or 12 m/s !) comes from the inadequate use of pressure wave propagation to compute transit times using Darcy’s law. Transit times are better reflected by the tracer tests that were carried out. It is possible to use pressure waves to estimate K, but then porosity or specific storage (which again are not mentioned) must be known, and the appropriate theory used. There are several points that suggest that the authors are not proficient in subsurface hydrology, and so I really suggest that they first discuss with a groundwater hydrologist about their conceptual model before submitting another manuscript.

If the ‘pressure transport’ aspect is removed from the paper, the problem is that conclusions will appear quite thin, the basic message being ‘levees can show heterogeneous K, values of which vary over four orders of magnitude’. In alluvial settings, this is not so surprising, and although, as stated in the abstract, it’s higher than expected from their

core samples, texture-based K values derived from literature (as done here) are highly inaccurate. I see three additional weaknesses: first, the approach does not include unsaturated flow processes, which are likely to play an important role when an unsaturated soil is suddenly submerged, as reported by the authors; the effect on results may be insignificant, but this should be controlled by soil moisture probes under and next to the reservoir; second, the short distance between the wells and the submerged area, given the size of the submerged area (Figure 1), does not lend itself to calculations based on average distances, as done here. From what we know about GW/SW interactions, it can actually be expected that most of the infiltration occurs next to the shoreline of a surface water body. Third, the methodological comparison between natural and artificial tracers is not supported by a sound dataset, the two experiments having been carried out in different flow conditions. To get some insight into the two first issues, I would recommend the reading of Sophocleous' 2002 review ""Interactions between groundwater and surface water: the state of the science."

Consequently, my subjective feeling is that, with the current dataset, there is limited scope for publishing in a hydrological peer-reviewed journal. I realise that such an experimental setup involved considerable work, and that my feedback can be somewhat disheartening.

However, if the authors are motivated to conduct another experiment, I would recommend re-defining the research rationale and theory, and/or addressing directly a biogeochemical issue (an interesting rationale of the present work), to give some meat to their study. In my opinion, the current research hypothesis is not sharp enough to ensure a successful study. For example, do the authors want to evaluate how simple field and modelling techniques can be used to characterise channel/floodplain connectivity? If so, they should rather illustrate how to ensure better success with simple techniques (the failure of simple models in alluvial settings should be an "a priori" assumption, rather than a conclusion), and evaluate the impact of their model assumptions, e.g., through 2D or 3D numerical modelling. Regarding biogeochemistry, for example, they

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could inject at the same time both a saline tracer and a solute of interest to biogeochemists (e.g. nitrate with isotopically-labelled N to quantify denitrification), and compare the recovery of both tracers to estimate the biogeochemical uptake or retention through the levee. I am sorry for these heavy criticisms... I hope anyway that the authors will find a successful research avenue, and that other reviewers will highlight some positive points that I have missed.

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