The authors present a novel approach to visualizing flow in fractured rock using the combined analysis of borehlole GPR reflection profiles and cross borehole tomography. They show that even with the limited access provided by two boreholes, they are able to observe the migration of saline tracers using time-lapse radar observations. Their GPR results are supported by additional studies conducted at the same field location. The manuscript is written well and the illustrations are of good quality. My comments are primarily editorial in nature but I also have one technical concern expressed at the end of my comments. I believe the manuscript is suitable for publication after addressing the comments below.

Introduction ~line 30. On the topic of assessing the fracture surface area contributing to heat exchange, you may consider looking up the publication by Hawkins A.J., Becker M.W. and G.P. Tsoflias (2017) Evaluation of inert tracers in a bedrock fracture using ground penetrating radar and thermal sensors, Geothermics, 67, p. 86-94, http://dx.doi.org/10.1016/j.geothermics.2017.01.006.

Line 46: change “propagation of water” to “flow of water” or “infiltration of water”.

Line 119: Is the INJ2.4 interval located in the plane of the GPR sections (i.e. the plane defined by boreholes Geo3 and Geo1) or is it off the plane and by how much distance? Please clarify.

Line ~135 & 140 GPR acquisition experiments 1 & 2: In the description of data acquisition, report the length of the two GPR reflection profiles, and the length of the tomographic section.

Lines 145-155: I found this part of data acquisition description difficult to comprehend and visualize. I had to read it multiple times. Maybe it is just me, but you may want to clarify better.

Line 171: Spherical spreading amplitude compensation is distance (or time) to the second power, not a linear correction.

Figure 5: Please Mark the location of the injection interval.

Figures 5 & 6 of the GPR profiles are oriented at 90 deg. rotation compared to the survey schematic depicted in figures 3 & 4, and the tomography results figures 7 & 8. I suggest figures 5 & 6 are rotated to the same orientation as the other figures so they will be easier to compare, especially the figures showing time steps fig 6 vs. fig 8.

Lines 425-429: Conductivies are reported in mS/m whereas in section 2.3 (lines 119-121) conductivities are reported in mS/cm. Please use consistent units throughout the manuscript.

Lines 430-435: I am extremely skeptical of the aperture estimates. I really do not think that the observations presented can support such detail. Tomography cannot give fraction of mm imaging. There are too many uncertainties and unknowns. Even full waveform inversion would be a stretch to provide this level of precision. Another simple question is what frequency do you use in equation 7? If you use 250 MHz (the dominant frequency of the antennas) you are overestimating frequencies. Typical GPR data is lower than the antenna dominant frequency due to attenuation. So the conductivity estimates are likely off. You’ll need to provide a lot more evidence to convince me of the aperture estimates.

I think the authors have done excellent work to this point. However, estimating fracture apertures from this data is not convincing, even if the calculations give realistic results. I suggest this section is not included in the manuscript.

Thank you for your contribution. George Tsoflias