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Interactive comment on "Travel time based thermal tracer tomography" *by* M. Somogyvári et al.

Anonymous Referee #2

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Dear authors,

The paper "Travel time based thermal tracer tomography" describes with synthetic examples how early time arrivals in thermal tracer tests can be used to invert for hydraulic conductivity in a heterogeneous aquifer. The paper is relatively clear, well-structured and well-written. It demonstrates nicely the method and proposes an interesting analysis on experimental parameters that may influence the results, such has the temperature contrast or the injection rate. I appreciate the effort made to demonstrate the range of applicability of the method. I have a series of comments which could, from my point of view, help improving the manuscript through moderate revisions.

1. I think that the methodology section "Tomographical inversion procedure" should be better articulated. More specifically, there is no apparent link between "travel time

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Discussion paper

inversion" and "early diagnostics". For example, it is not clear to me why the step 3 of early diagnostic is needed if we invert for first arrivals. I think that an additional subsection stating the inverse problem in terms of equations, clearly showing the data and model parameters used (K or log K), the forward operator, and how the inverse problem is solved is necessary, especially for a paper proposing a new inversion methodology.

2. Throughout the paper, the level of misfit (RMS, error weighted data misfit, chi-square, for example) between observed and calculated data is never given (scatter plots would be interesting, too). I think this is an important point for several reasons: 1) the comparison between different inversions is only valid if they have a similar level of misfit 2) the use of the staggered grid approach probably lead to a level of misfit which is higher than one would obtain with a finer grid, because the staggered grid is not able to reproduce fine scale structures 3) there is no insurance that the mean of the inversion on the different staggered grid actually explains the data (as the mean of a posterior distribution is not necessarily a sample of this posterior), therefore a forward simulation on the final results (fine grid) should be performed to assess the RMS and 4) This is a commonly used value to assess the quality of an inversion (standard in geophysical inversion).

3. The methodology makes some assumptions. For some of them, no reference is given and no demonstration is made in the paper, so that nothing really supports the statement. I would like those assumptions to be more thoroughly discussed, for example the limitations arising from them, how they could be overcome (use of geophysical data, joint inversion of hydraulic and tracer tomography, joint inversion of salt and thermal tracers, regularized inversion, etc.).

Some of these issues as well as some other comments are described in my specific comments.

Specific comments

1. P3L40-41. It might be worth mentioning geophysical technique here in addition to

DTS. See for example Hermans et al. (2014).

Hermans, T., Nguyen F., Robert, T. and Revil, A. 2014. Geophysical methods for monitoring temperature changes in shallow low enthalpy geothermal systems. Energies, 7, 5083-5118.

2. P8.L151-153. See my comment 3 above. For example, would it be possible to invert jointly for porosity and hydraulic conductivity? Even if smaller, the effect of porosity might not be negligible as it appears as squared (effect of the retardation coefficient), what is the effect on the hydraulic conductivity distribution? Also, the gradient (does fix means constant?) will have an influence near the injection well, where the obtained solution is sometimes relatively erroneous. The gradient could be computed at the end of each iteration as input for the next one. Please discuss.

3. P9L171-172. Is the peak breakthrough time already refer to the derivative of temperature? Precise to avoid confusion with maximum temperature that would be obtained with a Dirac or time-limited injection. What do you mean by real breakthrough time? Maybe show the equation of T', too.

4. P10L188. Do you have a reference that shows the non-sensitivity to those parameters?

5. P11L208-210. Why is this third step needed? Do you invert for the peak time or the early time? Is it this corrected peak time which is calculated through equation 2? Please make the link between the two sections.

6. P12L219-220. Do you mean hydraulic conductivity instead of velocity? I guess that velocity refer to the analogy to the traditional use of the SIRT algorithm in seismic and GPR inversion (equation 1), but I think it would be more consistent to use hydraulic conductivity everywhere (as in the figure for example), considering that other parameters (porosity and gradient) are not taken into account. How do you update the values (reference)?

7. P12L221-224. Give a reference on inversion problems such as Menke (1989) or Aster et al. (2005)

Menke, W. 1989. Geophysical data analysis: discrete inverse theory. Academic press, Inc. San Diego, revised Edition.

Aster, R., Borchers, B., and Thurber, C. 2005. Parameter estimation and inverse problems. Elsevier Academic Press, Amsterdam.

8. P12L226-228. The use of a limited number of parameters still limit each solution in the way that it is not possible to describe small scale heterogeneity, what would be possible on a finer grid with appropriate prior information and regularization (for example using non-smooth operator). See my comment on the misfit, too.

9. P13L245. The tomographic matrix? None of your equation mentions this. Hence, the necessity to describe the equations of the inverse problem. I guess, that by the tomographic matrix, you mean the matrix mapping the model into the data d=f(m), i.e. the forward operator.

10. P13L262-263. Confusing, please rephrase. I guess that “similar hydraulic properties” refers to the properties inside a given hydrofacies, but it could be understood as “the different hydrofacies have similar properties”.

11. P14L280-L283. Those values correspond to the analog. What values were used for inversion (constant value for the whole aquifer for porosity, thermal conductivity and heat capacity). Beta does not appear in the equations.

12. P15L305-307. Considering buoyancy and viscosity effects?

13. P16L332. The influence of the injection rate on the gradient i was neglected during inversion (i based on the boundary condition only)? What is the influence? Please precise.

14. P18L384. Globally, the results on a staggered grid really look like the results that

would be obtain on a finer grid with smoothing regularization. What is the benefit in terms of time of using the staggered grid approach? Such approach is probably not able to produce sharp contrasts as observed in the aquifer analog due to the high resolution grid, whereas a high resolution grid with an appropriately chosen regularization (e.g., MGS as developed in geophysics) can potentially lead to better results. Also, the staggered approach probably tends to average the results, so that high and low K are under-represented in the final solution. Also, there is no certainty that the mean of the 16 tomograms actually fits sufficiently the data.

15. P19L387-399. The null-space energy represents zones not covered by flow paths. The origin can be either the source-receiver positions or the presence of low K zones. In figures 5b, d, f, the null-space zone is always larger in the upper part where low K are observable. Therefore, a low-K value may be represented by a high null-space energy, but still be relatively reliable. A sensitivity approach (using the Jacobian for example) could potentially revealed this by showing that those parameters actually influence the data.

16. P20L413. In P1, the highly conductive zone is continuous, but not between any used injection-receiver pair. The method thus misses how this layer becomes thinner near the measuring well.

17. P20L414-417. Are the K values of inversion mean values? How are there calculated (arithmetic, geometric mean)? Which cells were considered (cells corresponding to the true zone)? Not straightforward as the values of K are continuous. I find relatively surprising that the inversion is not able to retrieve the homogeneous conductivity value of the bottom layer as it is fully constrained by 4 injection-receiver pairs.

18. P20L422-428. The effect of taking early times is not shown in the paper, since the method is not compared with a classic inversion of BTC. I would thus mitigate this statement.

19. P20L429-439. A suggestion for the figure would be to show the boundary between

facies on the inverted tomograms.

20. P23L489. Consider also a scatter plot (true vs. estimated) for a better visualization.

21. P24L516. It is never explicitly said that buoyancy and viscosity effect were considered in the simulation of the experiment on the 3D analog model.

22. P25L541. Why approximately constant and not constant? What deviations from constant are there?

23. P25L538-546. To me, the results of figure 9f is not that bad. It is closer to the truth near the measuring well, detecting the low hydraulic conductivity zone in the upper part. In contrast to figure 9e, it does not image the preferential flow paths into the zone of low hydraulic conductivity. It is true that it is not satisfactory near the injection well. To me, the most coherent solution might be figure 9d, showing that some distortions could already happen in figure 9e.

24. P29L622. Viscosity has a direct effect on hydraulic conductivity, so it should have an effect on the inversion based on advection, too.

Technical comments.

1. P17L343. Specify here how many pairs are left (instead of line 375).

2. P20L420. Replace vales by values.

3. P23L482. Do you mean figure 6b?

4. P24L516. Do you mean Figure 8?

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