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Hydrology and Earth System Sciences
Discussions

Interactive comment on "An extended trajectory-mechanics approach for calculating the path of a pressure transient: Hydraulic tomographic imaging" *by* Donald W. Vasco et al.

Anonymous Referee #1

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Note to editor: I posted my comments in plain text mode, but the formatting is lost. I requested Editorial for suggestions on how to send PDF/WORD files while maintaining anonymity. I can send these files directly to you if needed.

____ There are several issues to consider when assessing an inverse modeling approach. They must all be addressed in order for an inverse modeling approach to be considered as a viable option in application. I believe this paper addresses only a subset of issues, and additional work is required prior to publication. I will address these issues in the form of Q&A. The title of the paper is confusing, as it puts an emphasis on the solution to the

Eikonal equation. Now, if this is indeed to goal, then the paper should be resubmitted to any math journal. But as stated in the abstract, the paper attempts to present an inverse modeling/subsurface characterization approach. As such, it must address a wide range of issues (which, to a large degree are ignored), and this is its main weakness. Here are several issues to consider: 1. Is the math solid? I did not check the math, but the authors have been at that for many years and the results look reasonable. So I trust it to be OK, but it is neither relevant nor interesting for HESS readership. 2. Does this paper present a complete approach for inverse modeling? a. Uncertainty Quantification (UQ): UQ is ignored. Uncertainty quantification is not mentioned, let alone addressed. Tons of ink was spilled in writing on the significance of UQ, which I won't repeat here. b. Regression/optimization: Does an image produced following an optimization-based approach provide a realistic representation of reality? Not even by a long shot. Especially when UQ is ignored. c. Data integration: Field surveys usually provide multiple types of data. An inverse modeling approach MUST provide a way for integrating all types of information into a coherent image of the subsurface. We do not have this feature in the proposed approach. This would be acceptable for non-invasive surveys, but not for tomographic surveys. d. Spatial coverage is very limited: The survey boreholes (doublet) are very close to each other, thus covering a small volumetric fraction of regular size domains under investigation. This implied the need to develop multiple doublets, which is difficult to imagine. How far apart can the boreholes be? And when you have multiple wells, is it reasonable to ignore core data? See 2(c). e. Use of penalty terms and linearization: See discussion following equation 23. How do they affect the solution? What are the implications in terms of application? Which type of formations respond well to these math manipulations? f. Conclusion: The paper fails to present an acceptable inverse modeling strategy 3. Is the case study presented appropriate? And what can we learn from it? a. The survey boreholes are very close to each other (2-3 meters). I can imagine that when drilling a borehole, one would obtain a few cores and testing in the lab. With the wells being so close to each other, one can contemplate some sort of interpolation (e.g., geostatistics-based) of the core data.

Question is then, would tomography do better? This must be shown. What I would to see is a comparison between the following 3 options: (1) core data only, (2) geophysics only, and (3) combination. b. The authors provide a 2D solution (they solve the Eikonal equation in a vertical plan) to a 3D problem, as there is no guarantee that nature would follow the 2D geometry. It would be important to evaluate how good such (2D, planar) an approximation is: what type of formations are amenable for such treatment, or not? What's the ideal and max spacings between the boreholes in real-life applications? If the type of geology is not an issue, this should be shown. And that brings us to the following point: c. The authors chose a layer-cake formation for their case study, which is favorable to the 2D approximation. We need to see an application that does not play so favorably into this 2D/3D issue. d. Following on item 3c, the second line in the abstracts claims applicability to "general porous media". What does "general" mean? And what is the factual basis for this statement? On p. 2 it is stated that the method only applicable to smoothly varying formations, which raises some concerns about accuracy of the "general porous media" statement. The case study does not support the generality statement. e. Conclusion: Case study is not adequate.

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