Solid Earth Discussions

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Comment on se-2021-156

Tom Kettlety (Referee)

Referee comment on "An efficient probabilistic workflow for estimating induced earthquake parameters in 3D heterogeneous media" by La Ode Marzujriban Masfara et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-156-RC2, 2022

This is an interesting study on full waveform inversion of induced seismicity source parameters, using a new approach to calculate the posterior distributions. This manuscript is well written, with nicely presented figures. The structure led the reader through the inversion scheme relatively clearly, and explained the procedure in a way that was very instructive. It seems to be a step forward in joint inversion of source parameters on higher frequency data, and would be a valuable tool in the analysis of induced seismicity.

Not being an expert in the full waveform inversion, some points below may be naive. I am happy to be informed if they are! I did appreciate the time taken to explain the scheme in detail, but this may be too verbose for some with greater knowledge of the specific field.

The majority of induced seismicity microseismic monitoring takes place with relatively dense local arrays, some of which are downhole. The dominant frequency of recorded signals can be larger than the 1-3 Hz data the study limits itself to. This is especially true of downhole data, where high (>~1000 Hz) sampling rates enables events with corner frequencies in excess of 100 Hz to be routinely detected. The paper claims to be more effective at “higher” frequencies, but would still require significant loss of signal through filtering. How badly are the results affected by this loss of signal? How badly does the inversion perform when, say, ~10 Hz data is used?

Without testing any real data, even for a small sample of events from Groningen, it is difficult to justify the conclusion stated in lines ~15 and ~401. It certainly seems to work for this single synthetic event, but maybe the conclusions shouldn’t be overstated. I certainly look forward to the upcoming paper where this is used on Groningen data, but do wish this manuscript included at least a couple of tests of real data to validate the inversion scheme and compare to conventional methods used to locate and invert for focal mechanism.

In the Groninegen velocity model, is anisotropy included? It can make the MT inversion highly degenerate, especially when it comes to inverting for volumetric components, and should be brought up in the introduction.

A couple of small comments and spotted typos:

- Is the semi-colon in the wrong place at the start of line 111?
- Line 179: the references to Figure 1 (a) and (b) appear to be the wrong way round, and (b) is misquoted on line 185. This made Figure 1 more difficult to interpret.
- What fraction (%?) of the MT do you use for MT prior stated on line 331?
- You say on line 412 "significant computing power" to calculate reference seismograms, but how long did SPECFEM take in this case? Would be good to reference in relation to the one minute quoted to run the workflow.