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Interactive comment on “Fully probabilistic seismic source inversion – Part 1: Efficient parameterisation” by S. C. Stähler and K. Sigloch

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Received and published: 19 August 2013

The positive comments: The reviewed manuscript presents a very interesting analysis and contains a few very important elements which makes the paper extremely valuable. First of all I fully support the author's idea that presenting inversion results (most of temporary seismological analysis) without even a simplified a posteriori error analysis becomes unacceptable. The progress in computational methods allows to perform such analysis for many important seismic studies. Secondly, authors have performed a quite non-trivial analysis of the a priori information to include it in a coherent and quantitative way to the inversion schemata. This is by no means a trivial task and usually treat as “less important” part of inversion concentrating on techniques of data-reconstruction (fitting) only. However, we have to keep in mind that from the inversion

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point of view the a priori data as equally valuable as the “measured-data” and thus must be treated properly. If we forget about it we may end up with a lot of problems with a proper interpretation of inversion results. The third point I wish to raise is that evaluation of the source time function provides a very useful seismological information on the rupture process and it would be nice to have it included in seismological catalogs.

Now critical comments: The main point I have found out problematic in the manuscript is adding an exhaustive comments on the tomographic inversion in section 3.1 (pp 10-11) when Bayesian inversion methodology is discussed. I understand, that this part is used to justify the choice of the likelihood measure, but simultaneously it brings the reader out of the main stream of the paper and introduces some mess. I would suggest to move the discussion on a measurement of waveform fitting to appendix and maybe extend it. The same concerns section 4.3. The next point I wish to raise is the argumentation on the positivity of the STF (page 13, lines 5-15). I do not fully agree with the proposed argumentation that $STF > 0$ follows from proportionality to the stress + one direction rupture. If it would be a case what about processes with e.g., isotropic component which often occur in mining tremors. In such a case STF has been found by Domanski and Gibowicz also positive. The main point is, in my opinion, that positive STF means release of seismic energy while negative values correspond to its absorption. The next point is a sampling method used by authors. I am not fully convinced if the classical Metropolis-Hasting (MH) algorithm is really no efficient in this particular application. Authors state in section 3.3 that with 18 parameters using the MH algorithm becomes problematic. However, I use it in velocity tomographic analysis in mines (based on travel times only not waveforms) very efficiently even if number parameters reaches 1000. Thus my feeling is that this conclusion is rather a projection of the properties of the Neighbourhood Algorithm (NA) which encounters a real problem when number of parameters is larger than 10-20. This is because the NA algorithm is a kind of “geometric sampler” which numerical complexity increases very fast with size of the sampled space. On the other hand the MH algorithm is model space-size independent and performs equally well with tens and thousands of parameters. Finally

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I wish to point out, that the STF analysis were performed by Domanski and Gibowicz for many seismic-induced events with magnitude range 2.5 - 3.5 using the Empirical Green function approach. The results were published mainly in Acta Geophysica and Acta Geophysica Polonica as well as refernces can be found in the review paper by Gibowicz (Advances in Geophysics, vol 51, 2009).

Finally, besides the critical comments I find the paper very interesting and absolutely worth of quick publication.

Interactive comment on Solid Earth Discuss., 5, 1125, 2013.

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5, C444–C446, 2013

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