Solid Earth Discuss., 5, C506–C510, 2013
www.solid-earth-discuss.net/5/C506/2013/

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

S. C. Stähler and K. Sigloch

staehler@geophysik.uni-muenchen.de

Received and published: 12 September 2013

We thank Prof. Debski for his kind and constructive review. We will address his suggestions in detail in the following. The reviewer's comments are indented in italics.

> *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 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 we a lot of problems with a proper interpretation of inversion results. The third point I wish to rise 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.*

We agree with the statements above.

> *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.*

We found the choice of the misfit functional an important part and wanted to motivate it further by pointing out the connection to seismic tomography. Since the method might also be interesting to non-tomographers, we will rework this section and consider moving it to an appendix.

> *The same concerns section 4.3.*

We have to disagree here. To derive the influence of source inversion on the uncertainties of seismic travel-times from the waveforms is a radically new concept and should be presented centrally. However, since a second paper on the same method is to be submitted in the same journal in the next weeks, we will move this section to the other paper.

The next point I wish to rise 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 occure in mining tremors. In such a case STF has been found by Domanski and Gibowicz also positive.

The connection between the stress direction and the rupture direction strictly only holds true for homogeneous stress fields and pure double-couple sources. Most intermediate-size earthquakes can be treated as such. Our statement was limited to those contexts. We will clarify this in the final paper.

The main point is, in my opinion, that positive STF means release of seismic energy while negative values correspond to its absorption.

We are not sure whether we can agree with this statement. In the context of a DC source, a negative STF would be equal to a source with a slip vector pointing backwards, which would obviously not absorb seismic energy. Also, in the context of volcanic seismicity, negative STFs have been observed, due to dyke inflation and later collapse, see:
Chouet, B., P. Dawson, T. Ohminato, M. Martini, G. Saccorotti, F. Giudicepietro, G. De Luca, G. Milana, and R. Scarpa (2003), Source mechanisms of explosions at Stromboli Volcano, Italy, determined from moment-tensor inversions of very-long-period data, Journal of Geophysical Research, 108(B7), 2331, doi:10.1029/2002JB001919.

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 problems 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.

The reviewer is right. The Metropolis-Hastings-algorithm can be applied to inverse problems with high dimensionality. Our statement on p.1139 was too broad and will be changed. However, we still see several problems of the MH algorithm for our situation:

1. The MH is difficult to implement for multivariate distributions. This is especially true, when the parameters are different physical quantities and follow different distributions as is the case in our study. The step width affects the convergence strongly and finding a perfect one is a very delicate choice.
2. The correlation of models has to be taken into account, when estimating PDFs from the ensemble. This is a bigger problem than for the Gibbs sampler, which the NA is based on.
3. The MH is rather bad at crossing valleys of low probability. We are expecting multimodal probability distributions, especially for the source depth.

These problems are all not impossible to overcome, but are ones that the NA handles naturally. A comparative study between the NA and the MH method would be an interesting topic of further study and could also involve other sampling methods, like parallel tempering. We will update this section in the paper.

> *Finally, 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)*

We will consider inclusion of these papers into the discussion and the comparison in table 4.

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

We thank Prof. Debski for his detailed review.

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