

Interactive comment on “An MCMC Bayesian full moment tensor inversion constrained by first-motion polarities and double couple percent” by Mehrdad Pakzad et al.

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The authors present a moment tensor estimation approach using Bayesian inference based on polarities, double-couple percentage and seismic displacement waveforms. The method is applied to a synthetic case as well as to two real data cases.

In general, the article is hard to understand due to countless language style and formulation issues and needs significant copy-editing. Thus, maybe many important aspects of the work got “lost in translation”, but for me the authors did not present clear enough what is the novelty and strength of their method. To do so, they could compare test results of no-constraints inferences with results where they used these constraints.

C1

The overall structure of the article is unfortunate and the authors jump back and forth between describing sampling algorithms in the results section and the methods. Also previous (published) work is discussed in the method section as well as in the introduction.

Also the authors have to be careful with established terminology in the literature (e.g. Metropolis test- is called the Metropolis acceptance criterion) as well as mixing of data and theory errors and how they map to parameter uncertainties. This causes many sentences in the manuscript to make no or little sense in their current form.

One BIG major issue is in the section when the authors introduce the “coarsening” concept- also called tempering in published literature (used by several MCMC sampling algorithms such as Parallel Tempering/ Replica Exchange- or Sequential Monte Carlo). The tempering parameter (γ) in the manuscript has to be $\gamma = 1$ if the obtained samples are supposed to be from the posterior probability density (PPD)- if this is not the case the samples are not from the PPD and estimated parameters and their marginals are meaningless. In published literature the tempering is used in order to help exploration of the Markov Chain(s) to avoid getting stuck in local minima in the solution space, but MUST be switched off once one wants to sample from the PPD. Thus, the presented results of the synthetic test and the real data cases needs to be revisited and revised- apparently the authors also used $\gamma = 1$ as indicated by figures 3 and 6, but maybe there the Markov Chains need to sample longer.

In general, the authors need to explain more throughout the manuscript for example: 1. the parameterisation of the moment tensor 2. how the Greens functions are calculated 3. demonstrate influence of the polarities

In the attached document I gave many remarks, but they are to no avail complete. Best regards, Hannes Vasyura-Bathke

Please also note the supplement to this comment:

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<https://www.solid-earth-discuss.net/se-2019-191/se-2019-191-RC2-supplement.pdf>

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2019-191>, 2020.