

Review of manuscript “Bayesian inference based on algorithms: MH, HMC, Mala and Lip-Mala for Prestack Seismic Inversion” by Richard Perez-Roa, Saba Infante, Gabriel Barragan, Raul Manzanilla

In this manuscript the author compare four methods of prestack seismic inversion using synthetic and real-world seismic data. They aim at analyzing the methods with respect to accuracy and computational performance.

The authors implement and apply the methods but add nothing new to the methodologies. In the last paragraph of section 2.0 (i.e. line 106) they claim to “develop a general approach to seismic Bayesian inference”. However, they merely state the Bayesian theorem and that it can be approximated numerically using Markov chain Monte Carlo algorithms. This is in my view not a development of a new approach. In section 3 the authors describe the used algorithms quite well but that is also not original work. Section 4 describes an existing method of hydrocarbon prospection, as well.

The manuscript’s overall quality for the reader is not convincing. There are different quantities given the same letter, some abbreviations are not introduced while others are introduced several times, at some places there are incomplete sentences, and punctuation and parentheses (e.g. when giving references) is sometimes erroneous and in parts inconsistent. Vectors are not consistently written in bold face, equations are poorly formatted, there are some contradictions within the text (e.g. around line 175, where the authors speak about the costs of MH and HMC), the scaling factor in x-axis labels of figures 1 and 6 is doubly present, table 7 (albeit being small) is distributed over two pages, the caption of table 5 is inside the table, a.s.o... In this respect (seeing the large number of small and not so small inaccuracies) it lacks the quality I expect of a manuscript (especially since it is already version 2 and things like this should be corrected after the first round of reviews).

The interpretation of the obtained results is quite short and shallow. There is only one synthetic test case. Several cases with different velocity and density profiles and contrasts that are at the edge of the methods’ capabilities could help to improve the assessment of the methods. When does one or the other method fail, what velocity or density profiles can be recovered? The authors merely present the numbers (i.e. RMSE and computation time) but say nothing about the interpretation of the inversion results. With which method can a reservoir be found (in which of the test cases)? What are the uncertainties in the location and dimension of the reservoir? What is more costly, a false positive or a false negative?

I suggest, the authors think of a number of relevant test cases that can prove the capabilities of each method as well as their limitations. Then when rewriting the manuscript take care of the above mentions inaccuracies. Give a more in-depth interpretation of the results than merely stating the numbers in a table and in the text. Put the computation time into perspective for a real application. Are the times given here obtained on a laptop or HPC system? Is the example given a small, medium or large inversion, i.e. is the computation time of a little bit over an hour for the slowest method an issue or is it small compared to the time of obtaining the observational data and does not play a role? What do the other results like acceptance rate differences mean for real applications? I think these are issues that should be addressed in the discussion but the authors merely repeat the results.

The authors need to find a deeper research question they want to answer with their study that goes beyond taking four methods, implementing them and giving some statistical values for each method.