

## ***Interactive comment on “Elastic envelope inversion using multicomponent seismic data without low frequency” by C. Huang et al.***

**Anonymous Referee #2**

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The paper is relatively well written and structured.

My main concern with this manuscript is the similarity with the paper of R. Wu et al., 2014. To me, it is too close to grant a publication in a scientific journal. For instance the Figure 1 of the manuscript is similar to figure 6 of Wu’s paper... The conclusion of doing first the envelop inversion then waveform inversion is the same that in Wu’s paper. A straight forward application of the approach to simple synthetic elastic data instead of acoustic data is not sufficient. This could be sufficient for a conference presentation but not for a paper publication in my opinion.

To be publishable, the authors should add a significant aspect. This can be the study of the limitations of the approach. It was noticed in the 80’s by Moosegard, I think, that the envelop has a strong disadvantage because it does not preserve the sign of the

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reflections. This is not discussed in this paper (nor in Wu’s one), but I think it is one of the reason we are not really applying it (using the envelop in FWI is not a new idea and as been tested by several groups, the authors could also look at some EAGE and maybe SEG conference papers by the group of Prof. Chauris where they have tested a number of this kind of approaches and also discussed a bit the limitation). This can also be by add a real data set example.

I add two final points. Is it realistic to consider elastic propagation without density variations ? I think it would be more relevant to consider variable density. The gradient presentation is not really understand as it is presented. what is the relevance of equation 5? A straight forward applications of the adjoint state method leads normally to a clearer presentation.

I am sorry but I don’t recommend publication.

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Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 1757, 2014.