Review of the paper:

Nonlinear Wavefield Characteristics of Seismic Translation and Rotation in Small-Strain Deformation from Moment Tensor Simulations

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The authors present an updated version of their previous (long time) submitted work. Despite the topic is of interest to the community, I have rejected the paper in several previous occasions. This time, however, the paper has been substantially improved and clarified. The message of the paper is quite clear now and the contribution is new. The equations have been reduced to the minimum while keeping clarity and correctness on the presentation. Thank you for that.

This time, I only have minor suggestions that, in my opinion, will improve the clarity of the contribution:

Line 30: excitation efficiency. What is that?

Lines 195 and 196: it is not fully correct that when changing the waveform, one does not change the travel time. In many applications one measures travel times using envelopes, i.e., the maximum of the envelope, because measuring the beginning of the waveform is practically impossible to determine and cross-correlation measurements only work on similar waveforms. This means that the maximum of the envelope is a good indicator of waveform travel times. Please rephrase the sentence of lines 195-196 considering these notes.

Line 279: what is rectilinearity? Please define it.

Line 285: Same here, what is the Spearman's rank correlation coefficient. Please explain with clarity what it is and the reason of this choice.

Line 326: What do you mean by mixed sources? What is that?

Line 331: What is this time-frequency energy? Hilbert transform frequency, i.e., instantaneous frequency or Fourier frequency? Please clarify. In addition, please define inside the text that time-frequency-energy difference plots are the non-linear minus the linear. It is only defined in the figure caption.

Line 368: Fundamental seismic sources?

Please remove the part of specfem2d simulations. One cannot compare a 3D code with a 2D. The physics of wave propagation in 2D is different from 3D. One can never reach any fit comparison with different physics of wave propagation. Please remove this part completely and do not mention in the manuscript any 2D comparison. It makes no sense.

The benchmark with sismowine is fine, that gives the validity to the code that needed it. I, however, can see that the code of the authors shows some high frequency components in their waveform. This is usually normal and, in my experience, can be simply removed by filtering, so please do that. It should be ok then.

The Discussion is very informative and clearly written and Conclusions very clear too. The contribution of the work is clear and scientific. To the best of my knowledge, there is no other paper like this one published.

I thank the authors for the long and painful work done. I am sorry that it has been this painful. I do believe that now is a good and informative theoretical paper that gives a new message and motivates the community of rotational seismology to push forward.

With best regards,

A reviewer