Interactive comment on "Obtaining reliable localizations with Time Reverse Imaging: limits to array design, velocity models and signal-to-noise ratios" *by* Claudia Werner and Erik H. Saenger

Anonymous Referee #2

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The paper deals with the capability of the time-reverse imaging method(TRI) for source locations. In particular, it investigates the sensitivity of the method to the network position, size and aperture, velocity model and signal-to-noise ratios. The aim of the work is to define the criteria under which the TRI can be successfully applied. In terms of its aim, the paper would be a significant contribution to this important topic.

The paper is logically organised, but it is not written very clearly - this is the main reason why I suggest a moderate to major revision. My main comments are given below and annotated pdf attached:

1) The paper could be shortened and the main findings presented in a more systematic

way. At times, I have an impression that the authors describe everything what has been done, rather then summarising their main findings. For example, some of the obtained results are very inconclusive (e.g., in some cases the method work better with the less stations or with the noise contaminating signals). My intuitive conclusion would be that it is best to use a random network distribution, spanning a range of inter-station distances with as many stations as possible. I'm not asking explicitly to test such a case, but if it is not too difficult, it could be a good addition

2) "Localization" should be replaced throughout the manuscript with "source location". also, "localization quality" is actually the "location accuracy". It would be good to find a native speaker to read the paper before re-submission if possible.

3) Defining the criteria to assess the performance of TRI is not clear enough. I did not understand what are categories I - IV. Also, some parts are unnecessarily repeated

4) The proposed method is not suitable for shallow sources because the authors mute the upper part of the model. This is an important limitation which should be explicitly stated in the conclusion

5) When the authors are talking about the real data from Southern California, they actually use synthetic data. This is fine but needs to be better explained. Clearly: "To mimic a real case scenario from Southern California, we simulate .... The advantage of using synthetic data when testing a method is because we know what the true answer is...."

6) The discussion part should be more systematic. It is currently divided by recent literature and it is comparing the results from this study with the literature. Instead, it should be divided by the nature of the results, where the literature is cited as needed.

Taking this points into account (and those from the attached manuscript) would significantly improve the paper and make it a very useful source of the information for the scientific community.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2018-76/se-2018-76-RC2-supplement.pdf

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