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Interactive comment on "Moment magnitude estimates for Central Anatolian earthquakes using coda waves" *by* Tuna Eken

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In this work, the author uses a multiple-scattering approach to infer the source spectra of small to moderate earthquakes recorded in central Anatolia using the observed energy envelopes of seismograms. The method, originally developed by Sens-Schoenfelder and Wegler (2006), is based on isotropic, scalar radiative transfer theory and makes use of a generalized inversion technique. The author obtains source spectra that are generally well fitted by the classical omega-squared source spectrum for the largest one. For smaller events, there is considerably more scatter in the exponent of the spectral decay at high frequency. The author shows that there exists a reasonably good correspondance between the local Magnitude ML and the coda-derived moment magnitude. I think that the study confirms the overall interest in using coda waves to

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study the source of small earthquakes. It also suggests that the physics of smaller events might be different from the one of larger earthquakes. The analysis is sound and the study will be useful to convert local magnitudes to moment magnitudes in future investigations of the area. Therefore, I support the publication of the manuscript after the following questions/points have been addressed.

1) In introduction, I would suggest to distinguish more clearly between parametric approaches (such as the one developed by Mayeda and co-workers) and physics-based approaches (Wegler and co-workers). In the present version, what distinguishes the two methods is not really clear.

2) It is written that the method of Wegler does not rely on coda normalization. I think that this is an overstatement: although the authors do no explicitly "coda-normalize" their data, I still think that the separation of source and site effects still relies heavily on the fact that at long lapse-time in the coda, the energy distribution homogenizes spatially. If I am mistaken, please explain why.

3) I would recommend to split section 2 into two sections: Geology on the one hand and Data on the other hand

4) In the data section, it would be useful to state the final number of utilized paths after applying the selection criteria for the coda

5) In the Method section, there are a few typos in the Eqs, please verify. I would recommend to explain how g is updated in the inversion process. Furthermore, I think that it would be useful to discuss the possible trade-offs among the unknown in the system of Eqs (4)

6) In the studied area, I imagine that there are strong lateral variations of geology and therefore that the scattering coefficient depends on the source station pair. Is this taken into account in your inversion? If so, could you comment on the spatial variations of g in the studied area.

7) Is Figure 4 discussed somewhere in the text? I could not find. If the Figure is not useful, you should suppress it. If it contains information, please discuss it more carefully.

8) On L.250 and elsewhere, it is said that the radiation pattern has only a minor influence on the coda, an assertion with which I agree on the whole. Nevertheless, the separation of scattering and absorption also uses the energy contained the coherent wave which is strongly affected by the radiation pattern. If you have used techniques such as MLTWA in the past, you have probably observed that the largest fluctuations in the data occur in the window containing the ballistic wave. Therefore, it is not completely clear to me how the radiation pattern affects the data inversion. Could you comment on this point?

I also upload an annotated manuscript where I have made additional remarks, mostly pertaining to grammar and/or typos, but also to the presentation of Figures.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-8/se-2019-8-RC3-supplement.pdf

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