Some comments on Z. R. Tao, X. X. Tao, and A. P. Cui, Strong ground motion prediction for southwestern China from small earthquake records, submitted to Natural Hazards and Earth System Sciences (NHESS).

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This was not intended to be a formal review, although it seems to have evolved into one. I declined the invitation to be a formal reviewer, but I downloaded the paper and read it quickly. Here are a few comments that the authors and Editor might find useful.

This is a well written paper describing interesting research on an important topic. I have only a few comments related to details of what was done. The comments below are in sequential order as I read the paper, not in the order of importance.

p. 5299, l. 4: “Atkinson”, not “Aktinson”

equation 1: The f_{max} model is outdated; most people now use the κ_0 model, in which the last term on the top line of the equation is replaced by $\exp(-\pi\kappa_0 f)$.

p. 5301, l. 12: What value was used for f_{max} in the inversion?

p. 5301, l. 15: What values were used for A(f) in the inversion? Note that stress parameters determined in the inversion are contingent on all the other parameters in the model, including A(f); so any assumption could be made about A(f), but any simulation of ground motions using the stress parameters resulting from the inversion would also need to use the assumed A(f). It would be useful to put all of the model parameters into a single table.

p. 5301, l. 11: Your source model differs from the standard model (for which a=2 and b=1). How sensitive are your results to your choice of a and b? Also, the reference for your a and b is not readily available. Is there a more easily accessible reference? Could you send it to me?

p. 5301, l. 15: You should state that you did not use Atkinson and Boore’s values for R1 and R2, but instead let them be free coefficients in the inversion. But you also need to be explicit about what slopes you assumed for the three line segments.

p. 5302, l. 1: I’m not sure that I agree with your statement. The path parameters are not that hard to determine using standard methods. One problem in lumping many parameters into one inversion is that possible tradeoffs between parameters (such as the geometrical spreading and Q0, unless reliable data are available to distances of hundreds of kilometers) are obscured.

p. 5302, l. 3: I’m not sure what is meant by “since they are considered as unrelated with the size of the earthquake on an interested frequency band”. Many people have found that $\Delta\sigma$ depends on magnitude, at least for small events (although this is controversial). Also, the prose needs work; perhaps “earthquake in the frequency band of interest” is better. And you should specify what that band is (e.g., 0.1—10 Hz? 0.05—20 Hz?).

equation 2: Apparently you used no smoothing of the observed FAS. Most FAS show a lot of variability, so I would think that some smoothing would help stabilize the inversion. Also, you should state the frequency spacing of the FAS (did you use a fixed time-domain window?).

equation 4. This is new to me. Could you send me the reference?

p. 5302, l. 15: It is not at all clear whether you used both the small and larger earthquake data sets in your inversion. This is an important point. If you only used the small event data, then comparing the results to the “strong” motion data (section 4.1) is a very useful and interesting exercise, as it amounts to a “blind” prediction of the data. You need to have more discussion about this.

p. 5302, l. 16: Your simulation procedure differs from mine (e.g., Boore, 2003). I window random noise in the time domain, transform to the frequency domain, normalize by the modulus, apply the model spectrum, and then transform back to the time domain. Your description indicates that you normalize after applying the model spectrum, which would remove any overall amplitude due to magnitude, distance, etc. I am confused.

p. 5303, l. 2 (and p. 5305, l. 17, p. 5307, l. 5): “envelope”, not “envelop”.

p. 5303, l. 16: The legend in the bottom left graph indicates that the data are from 6.5<=M<7.0, yet the curve is evaluated for M=7. So it is no surprise that there is an apparent bias. It would be better to use a M for the curve that is some measure of the average M for the data.

Section 4.1: I suggest repeating the analysis for response spectra at periods of 0.2 s and 1 s.

p. 5304, l. 15: “near-field” is incorrect; you should use “near-fault” (high frequency motions are rarely in the near-field of faults---that is, within a wavelength).

p. 5305, l. 18: I suggest replacing “to inverse” with “to determine”.

Table 1: Explain the ranges shown below the column titles.

Table 2: State that the uncertainties are for $\log_{10} Y$, also, you should show fewer significant digits.

Figure 8: You should include average (and standard errors of the averages) for residuals in Rhyp bins.

Figure 8: It is standard practice to used mixed effects analysis to separate the residuals into within (intra) and between (inter) event residuals (e.g., see the NGA-West2 papers published in Earthquake Spectra; my paper [Boore et al., 2014] is available from the online publications page of www.daveboore.com. In plots of the residuals vs Rhyp this removes the effect of earthquake-to-earthquake variation, making it easier to see if there are trends associated with the path-dependent part of the problem. You should plot the inter-event residuals vs M to see if there are trends in the magnitude scaling (this is suggested by the large bias in the bottom left graph of Figure 8, which is not due to the similar bias shown in Figure 7, because presumably the magnitude of each event has been included in computing the residuals in Figure 8).

I find no mention of site response in your paper. Are all the sites on similar geology? If not, what adjustments were made to the data before inverting for the parameters shown in Table 1?

Reference

Boore, D.M. (2003). Prediction of ground motion using the stochastic method, *Pure and Applied Geophysics* **160**, 635–676.

Boore, D.M., J.P. Stewart, E. Seyhan, and G.M. Atkinson (2014). NGA-West 2 equations for predicting PGA, PGV, and 5%-Damped PSA for shallow crustal earthquakes, *Earthquake Spectra* **30**, 1057–1085.