The final author comments

The authors would like to thank the referee sincerely. There are really some places in the manuscript not clear enough and are pointed by the referee, so we believe the final manuscript must be much better from the valuable comments.

1. **p. 5299, l. 4: "Atkinson", not "Aktinson"**
   Yes, the name was misspelt, now is revised in the final version.

2. **equation 1: The f_{max} model is outdated; most people now use the κ_0 model, in which the last term on the top line of the equation is replaced by $\exp(-\pi\kappa_0 f)$.**
   Yes, the f_{max} model is outdated, however, we can read it on acceleration Fourier spectra directly, and we were trying to represent all local characteristics from the records in this paper. In the term of $\exp(-\pi\kappa_0 f)$, κ_0 is also local parameter, we are working on it and have published some results such like the following two, and thinking to take them into account in equation 1 with some consideration on distance relation and something.
   Xiaodan Sun, Xiaxin Tao, Shusu Duan, Chengqing Liu (2013). Kappa (κ) derived from accelerograms recorded in the 2008 Wenchuan mainshock, Sichuan, China. Journal of Asian Earth Sciences, 73 (2013):306-316.
   Zhengru Tao, Xiaxin Tao and Wenqian Li (2015). Kappa(κ) from records of small earthquakes in North China. Advanced Materials Research, 1065-1069: 1469-1473.

3. **p. 5301, l. 12: What value was used for f_{max} in the inversion?**
   We use f_{max}=5Hz, which is the average value of f_{max} obtained from the records we adopted. It is lower than those in some other references, but the same value is adopted in the motion attenuation relationship for Japanese seismic hazard map. It is mentioned clearly in the final version now.

4. **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.**
   $A(f)$ is near surface amplification factor and could be estimated by a transfer function of regional crust velocity gradient, the $A(f)$ given by Boore and Joyner (1997) for generic rock site ($\overline{V}_{30}$=620m/s) since the prediction in this paper is for rock site, and keep the same in both inversion and prediction. It is mentioned in the final version.

5. **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?

The source model is originally presented by Masuda (1982), and we improved it further (1) fit *a* and *b* values from strong motion records, (2) apply it in strong motion synthesis to eliminate the size effect of sub-source. For example, see the following paper. The comparison with Brune and Atkinson's could be found in the paper as

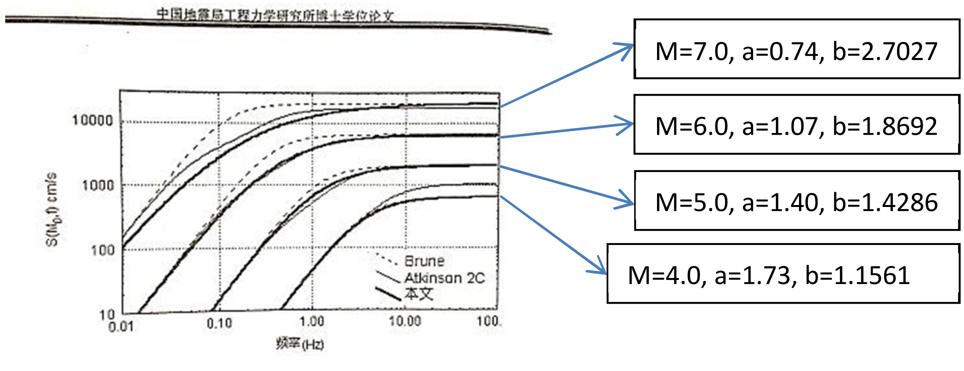

图 3.1 震源谱模型比较

TAO Xiaxin, SUN Xiaodan, WANG Guoxin（2008）. A dynamic corner frequency based source spectral model, Proc. of the 14WCEE. S02004

6. **p. 5301, l. 15: You should state that you did not use Atkinson and Boore's values for R_1 and R_2, 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.**

Yes, we did not take the values of R_1 and R_2 given by Atkinson and Boore, but take them as coefficients to be determined in the inversion, since they are related certainly with the regional earth crust structure. It is mentioned obviously in the final version.

7. **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 Q_0, unless reliable data are available to distances of hundreds of kilometers) are obscured.**

In fact, we are not sure if the path parameters should be taken as parameters to be determined in inversion, so just three parameters were taken in a previous paper, as following. It is a test this time to see if the GA procedure could manage 5 parameters, and the result is acceptable, therefore we are going to publish the result. It is helpful, for the southwestern China region between Tibet-Qinghai plateau and Sichuan basin.

Zhengru Tao, Xiaxin Tao and Xiwei Wang. Seismology based Ground Motion Attenuation Relationship for Sendai Area. Advanced Materials Research. Vol.382(2012), pp.7-11.

8. **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 Δσ 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?).**
   Yes, the relation between Δσ and magnitude is controversial, and the authors believe it is not necessary to mention this problem, so the half sentence is deleted in the final version.

9. **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?).**
   Yes, actually we adopt the envelope of FAS as the objective function, it is a kind of smoothing and quite stable with frequency. The frequency spacing is 0.0122Hz, it depends on the time step 0.02 s and total number of amplitudes, 4096 mentioned in eq. 2. That means a fixed time-domain window 81.96s, this is described more clearly in the final version.

10. **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.**
    Yes, we need more discussion on this point, to clearly show the meanings of the paper. As mentioned in abstract, "a method is developed to predict strong ground motion by small earthquake records from local broadband digital earthquake networks". The limited records of regional strong ground motion are used in this paper just for checkout. One more sentence is added in conclusions to emphasize this in the final version.

11. **p. 5302, l. 16: Your simulation procedure differs from mine. 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.**
    Yes, our procedure is not exactly same as yours, but quite close to yours. The FAS of enveloped time history is scaled to the expect one directly and transform back to time domain. To make it clearly, we rewrite those sentences in the final version.

12. **p. 5303, l. 2 (and p. 5305, l. 17, p. 5307, l. 5): "envelope", not "envelop".**
    Yes, it was spelling error, and is revised in the final version.

13. **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.**
    Yes, it is exactly right, the reason of the fact that in the case of M_w=7.0, our result for Sichuan region is higher than the observed data, is we do not have data with magnitude more than M_w6.5 during this period. It is not hard for readers to recognize, so the attenuation curve keeps the same magnitudes as for Yunnan for comparison of the results in the two regions.

14. **Section 4.1: I suggest repeating the analysis for response spectra at periods of 0.2 s and 1 s.**
    Sorry, the prediction of this paper is just on the strong ground motion PGA which is widely adopted in seismic design in China, while the amplitudes at periods 0.2 s and 1.0 s of response spectrum is used in US. To make it more clearly PGA is added in the topic, abstract and introduction of the final version.

15. **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).**
    OK, it is revised as near-fault in the final version.

16. **p. 5305, l. 18: I suggest replacing "to inverse" with "to determine".**
    Yes, it is better, revised in the final version.

17. **Table 1: Explain the ranges shown below the column titles.**
    Yes, it is important and referrible, it is added in table 1 in the final version.

18. **Table 2: State that the uncertainties are for $\log_{10}$Y , also, you should show fewer significant digits.**
    Yes, the residual in this paper is defined in eq.5, in $\log_{10}$ units, its mean value and the standard deviation (SD) are of course in the same units. The significant digits are reduced from 4 to 2, in the final version.

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

20. **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,**

Stewart, Seyhan, and Atkinson, 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).

Yes, there is really a mistake in the residuals calculation, and it is modified in the final version. The authors would like to thank the referee very much for the careful review. The main goal of this paper is for the region without enough strong motion data, therefore in Sichuan and Yunnan regions, data are not enough to build attenuation relation or to check a prediction. We can just use these limited data for a comprehensive comparison, and cannot check the prediction in individual event since the data are so few in that situation.

21. **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?**

    Yes, ground motion predicted in this paper is on the engineering rock site, the data adopted in inversion are recorded also at engineering rock stations in the broadband digital earthquake networks. The strong ground motion records at obvious rock site are very limited, for most stations of the strong motion networks, the shear wave velocity structures are not available at this moment. To adopt all data for the check is helpful to get a whole view, even though the deviation should be larger than those only from so few data on rock site. For the near-fault motion synthesis during the Wenchuan Earthquake, two rock stations are selected, the result is quite good comparing with the records.