Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2020-47-RC2, 2020

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Wide sensitive area of small foreshocks" *by* Chieh-Hung Chen et al.

Anonymous Referee #2

Received and published: 25 June 2020

The manuscript presents results which, in my opinion, can be very relevant for the forecasting challange. However I find that they are not well presented and the discussion appears quite confusing for the following reasons: 1) The first part of the manuscript is devoted to study spatio-temporal patterns of seismic activity before and after events in a given magnitude range, for Taiwan and Japan. There are many papers which report a similar increase of seismic activity before large earthquakes. The key point is if the observed increase can have a prognostic value or it can be explained within normal aftershock triggering. I just suggest some papers where this point is detailed discussed, other references can be find therein (Lippiello et al., Scientific Reports 2012, de Arcangelis et al. Physics Reports 2016, Lippiello et al., Pure and Applied Geophys. 2017, Lippiello et al., Entropy 2019). In my opinion many of the results of sec.3 are not really interesting since they are probably artifact of the adopted stacking procedure. Furthermore they are not strictly related to what for me are the main findings (see my point

2). Therefore, I believe that this section can be moved to the supplementary materials whereas in the main-text the authors can just summarize some results and discussing recent literature on this specific point. 2) Conversely, I strongly encourage the authors to move fig.S5 from the supplementary to the main-text. I am really impressed by this figure. In particular I find striking the result of the left panel which, if i correctly understand, is for a single M6.6 mainshock and therefore is not contamined by spurious effects caused by the stacking procedure. This figure shows a change in the dominant frequency from roughly 10ˆ{-4}Hz up to 30 days before, to a much larger value before the mainshock. What I find really interesting is the analysis at a fixed frequency (around 10ˆ{-4}Hz) as function of the time from the mainshock. In this case you find that the mainshock occurrence time is a minimum" point in the sense that the amplitude ratio at the given frequency decreases before the mainshock and increases after, in a quite symmetric fashion. Comparing with the central panel, which is substantially the same of Fig.3, the authors find a similar pattern at a similar frequency for 4<M<5 mainshocks. In this case however the decrease of the amplitude ratio before the mainshock and the subsequent increase after is less pronunced. The same holds for 3<M<4 where the changes of the amplitude ratio are even less pronunced. This is really interesting since it suggests that you can correlate the slope of the amplitude ratio (at a specific frequency) with the magnitude of the incoming mainshock. I invite the authors to focus on this very important result and I suggest some checks to support the scenario. i) I don't fully understand the smoothing procedure: "The common-mode vibration is sliced ....". The really important point is that the amplitude ratio plotted at time t only contains waveforms recorded up to time t. In other words, it is fundamental that quantities evaluated before the mainshock are not contamined by the mainshock signal. ii) The authors use the signal from 33 seismometers. What happens if I consider a smaller number? In particular how much results depend on the distance between the seismometer and the mainshock? iii) There is some reason to take the first 20 principal components. What happens if one changes this number? iv) Is there any pattern observed for a single M4+ earthquake, without stacking their signals?

3) I am not totally convinced that the mechanism of resonance is the one responsible for the above observation. In my opinion this is a weaker point which can be also moved to supplementary, keeping a small discussion in the text.

Summarizing, I believe that the direct analysis of seismic waveforms can contain more information than the one extracted from seismic catalogs. This is for instance shown in recent publications (Lippiello et al. Geophys. Res. Lett. and Lippiello et al. Nature Communications 2019). In this direction, the PCA method used by the authors is very promising. I invite the authors to a global rewriting of their manuscript in order to better stress the main results. I also invite the authors to perform the suggested or similar checks to support their findings.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2020-47, 2020.