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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Importance of three-dimensional grids and time-dependent factors for the applications of earthquake forecasting models to subduction environments" *by* C.-H. Chan

Anonymous Referee #2

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The paper evaluates future earthquake occurrence in two regions, one in the northeast of Taiwan (Ryukyu) and the other off Japan (Kanto) with two different models: one time-independent (Woo's method) and one time-dependent (rate-and-state friction model), in 2D and 3D.

I found the paper basically clear, but there are several elements that need clarification, as I point out in what follows.

Formula (1) gives a rate lambda that has the following dimensions: number of earth-

quakes/(time x area). This is consistent with the dimensions of the Kernel in formula (2): number of earthquakes/area, but it is not with units of Figure 2 (number of earthquakes/time/Volume). Is it by purpose, or is it a mistake? Please explain. Further, explain clearly what lambda is in formula (1). Is it the seismicity rate for earthquake of magnitude M or with magnitude exceeding M, as it seems?

For reader's better understanding, it would be useful to provide tables where all parameters used in the estimation of the two models are gathered. For example, provide estimates of TM, NM, PL, c and d. Moreover, explain: did you try with different power laws? how did you estimate the coefficients c and d?

From your data, it seems that the bandwidth H(M) is very concentrated about the hypocenter/epicenter xi since c is at most 1 km, and d is less than M. The consequence is that H(M) goes rapidly to 0, and decays more rapidly for larger magnitudes. When the distance x-xi is much larger than the bandwidth, the Kernel becomes constant (not depending from the position). A discussion of the kernel properties could be useful.

In formula (5) ΔR (M,x,t) could be better denoted as ΔRn (M,x,t). Since the rate-and-state friction model requires the knowledge of the focal mechanism parameters, it's worth stressing that it can be applied only when these are available, which usually happens only for large earthquakes. The author mentions that ΔCFS was calculated in a homogeneous half-space: please provide the assumed P and S wave velocities.

Specify better what the locution "target depth" means for the rate-and-state friction model computations.

The importance of the depth factor is partly surprising. I explain why. In all predictions, the less you predict, the more your predictions are reliable. More details one adds, the more he risks to be wrong. Applying this principle to the seismicity predictions, one would expect that restricting predictions to the epicenter location one would obtain results more accurate than the ones got when predicting the hypocenters (since the

depth parameter is added). This is not the case for this paper. This should be explain. My opinion is that in the calculations, the kernel parameters were estimated for a 3D case and applied both for 2D and 3D, which of course is unfair. The 2D case should be treated with a proper 2D kernel.

The Kanto case is treated too quickly. It seems that only results for the rate-and-state friction model are shown. Figures 8 and 9 should be illustrated and discussed better. In particular, the curves “kernel function + rate/state” of Figure 9 should be explained. Are they the result of a combined method?

The section 5.3 on application on seismic and tsunami hazard must be either cancelled or treated more accurately. Forecasting epicenters and hypocenters of large tsunamigenic earthquakes is a very relevant result. However, it is not clear how this can be achieved with the present methods. Results should be shown where seismicity distribution is given for band of magnitudes larger than 8 (like in Figure 6, where they are given only for M up to 7.9).

My overall evaluation is that the paper needs major revision.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 7527, 2015.