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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Deep Learning of Aftershock Hysteresis Effect Based on Elastic Dislocation Theory" *by* Jin Chen et al.

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The manuscript presents a method to forecast aftershocks based on DNN (deep neural network) methods. The authors use the SRCMOD finite fault database and the ISC (International Seismological Centre) seismic catalogue to train the DNN. They use 19 finite fault source models of which 15 are used as training data and 4 are used as validation data. The authors present results for two case studies: the 2008 Wenchuan earthquake in South China and the 2011 giant earthquake offshore Japan. I disagree with the main findings of their study and I would recommend major revision of the manuscript and resubmission. My comments are summarised below in two sections a) major and b) minor. A) ÎlJajor comments: 1) on earthquake data presentation: for each case study how many aftershocks are contained in the

ISC database vs. hysteresis model predictions. Make a table corresponding to the time-intervals considered in this study, i.e, with 1-d/no, 1-week/no, 30-days/no etc. Also, show a depth distribution of events per time-interval according to the ISC catalogue. 2) to obtain a more accurate comparison of aftershock locations with hysteresis model predictions, I recommend to the authors to use relocated earthquake catalogs. It is obvious that the ISC catalog contains artifacts regarding the location of events (see cross-alignments in Fig. 6) but the purpose of the ISC catalog is not to compare aftershock prediction models with physics-based (Coulomb) predictions. 3) The authors compare aftershock decay rates results (hysteresis model) with Omori (Utsu) empirical laws but do not present the actual results of their application in this manuscript. So I would like to see an aftershock decay diagram (Utsu vs hysteresis) for both case studies (Wenchuan and Tohokou). 4) The 2011 (Tohoku) hysteresis modeling did not take into account the change in the properties of the elastic medium, i.e the 2011 earthquake ruptured the subduction interface between continental and oceanic crust. Given the configuration of the plate geometry offshore Japan, it is easy to count (within the ISC location uncertainties) how many ISC aftershocks occurred on the subduction interface, inside the upper (Eurasia plate) or inside the slab. 5) The hysteresis model fails to predict no aftershock zones close to the mainshock as it occurred in the Tohoku case, i.e in Fig. 8 the area surrounding the mainshock (a few km at all directions; in fact the main asperity and towards the east; see Hayes 2011, https://link.springer.com/article/10.5047/eps.2011.05.012) is coloured the same as the areas where aftershocks are predicted. 6) the hysteresis model overpredicts aftershocks, see 1-day etc. spatial distribution for the Wenchuan case (Fig. 5) where over 50% of the predicted area to be covered by afteshocks is in fact void of aftershocks. Indeed, aftershocks are distributed parallel to the main rupture and most of them occur on the hangingwall of the thrust and below the cos-seismic slip surface (Tong et al. 2010; https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2009JB006625) which is in good agreement with physics-based models (see for example the Athens 1999 earthquake case https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2007JB005504 or the

L'Aquila 2009 earthquake case https://onlinelibrary.wiley.com/doi/full/10.1111/j.1365-246X.2011.05279.x . 7) a complication regarding the Tohoku case is the Mw=7.9 aftershock https://web.archive.org/web/20110412003136/http://neic.usgs.gov/neis/bulletin/neic_iiav.htm and its own aftershocks. That dataset should be added in Fig. 9 and compared with hysteresis model predictions. 8) It seems that the DNN shows a marked decay in the expansion rate at about 30-days following the mainshock. I suggest to investigate this pattern further and provide a more quantitative approach, i.e. is it 29, 30 or 31 days etc? is it a function of the magnitude of the mainshock?

minor comments: line 43: Stein et al., 1983 should be corrected to Stein and Lisowski, 1983. line 50: the reference to Phoebe et al. should be corrected according to https://www.nature.com/articles/s41586-018-0438-y. The same comment applies to line 421 of the manuscript. line 50: the 2018 Nature article attracted a 2019 reply on the same journal https://www.nature.com/articles/s41586-019-1582-8 line 65-67: These views treat the crust as a continuoum body while it is widely know that the crust is cross-cut by discontinuities/dislocations where tectonic strain is accumulating. line 169: The paper by Yi et al. (2011) deals with the period before the Wenchuan mainshock. line 282: ...Chengdu, Mianyang, Deyang, Guangyuan and Ngawa... Are these provinces? regions? towns? line 604: Fig. 10h, is this dataset (at 37.5 km depth) consistent with plate geometry (plate boundary is the yellow line, i.e. trench at -5 km)?

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