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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Fault distance-based approach in thermal anomaly detection before strong Earthquakes" *by* Arash Karimi Zarchi and Mohammad Reza Saradjian Maralan

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We appreciate the time and effort that you have dedicated to providing your valuable feedback and your insightful comments on our manuscript. We have been able to incorporate changes to reflect most of the suggestions you have provided and will be accessible in our next uploading manuscript.

1) In literature, there are many proposed methods for strong earthquake prediction based on observed singularities of physical quantities. The non-prevalence of a specific one (or at least a limited set of them) means that the detection of the aforementioned

singularities in not enough to have an operationally complete method. What is usually missed is the performance of each proposed method during the periods or situations where there is no intense activity (or at periods with mild activity). This is linked to what we called "false alarm avoidance". The current manuscript suffers from this lack also: the authors did not present the results of their proposed method in the periods where no strong events existed in order to compare both results. If this is not done then this study is incomplete We can use the method on the previous year data for each earthquake which no seismic activities was reported to show and compare their results with the results generated from the year of the earthquake in the future manuscript.

2) In the case that authors can successfully implement the previous suggestions, the authors must justify the selection of some crucial parameters of their method such as: a) how the k coefficient was selected b) how the value of radius R was selected? Based on topographic criteria or due to some signal processing axioms? In order to catch up a very common answer, the significance of the proposed method could not be based on "empirical selection" and thus must justified in a solid framework.

a) Since for each earthquake that we investigate the other 5 earthquakes are considered to have already occurred and their details are known. Using the information regarding these 5 earthquakes we have found the optimal value of K for anomaly detection methods for our region. We have also compare them with similar studies that investigated same earthquakes.

b) Since we are using MODIS products of LST with spatial resolution of 1km the interval for R is set to this same amount so that no transformation had to be done. As we know strong earthquakes have large area of effect and many studies used MODIS data for their investigating we can assume this amount to be sufficient for our study. As for the range of R we have to consider that not only by increasing this parameter we can see the less effects caused by the earthquake on the outlying layers we also put several different landcovers with various temperature in one layer. This will result inaccuracy in later processing. Using different parameters such as Variance can help us better find

the optimal maximum range for R.

3) The authors claim the use of an ANN in order to estimate the EQ intensity. This could be acceptable only if the authors provide details about the implementation of the ANN that they used (topology, comparison to relevant implementations, performance, training data set, criteria for selection of training and evaluation data sets, scoring of individual runs, computing requirements).

We will add more detail results of our ANN such as its performance, comparison to relevant implementations, training data set and criteria for selection of training and evaluation data sets in the future manuscript.

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