General comments:

In this work the authors intend to show the reliability of a method, proposed in a previous work, to evaluate, approximately, the time variation of the probability that in a given homogeneous seismogenic area, occur at least an earthquake with a magnitude greater than a predetermined threshold within a certain time.

This work does not deal with due care all the most problematic issues: seismological, statistical and of signal analysis, which affect the reliability of the results.

However, the work is interesting both for the information it provides on an important Italian seismogenic area, both for the methodological ideas it proposes, and may be published after the authors made the necessary corrections.

Need for a fundamental review of the English form from a native speaker. Therefore I do not suggest any variation of linguistic form, even in cases in which the interpretation of the text is unclear.

Specific comments:

From P. 4422 onward: The authors should specify the magnitude scale at which the value is related.

P. 4423, lines7-10: This comment seems to me questionable, but it is certainly not necessary.

P. 4423. line 20: On the basis of what is said in the paragraph "Method" this parameter should describe the time variation of the number of earthquakes, with magnitudes above a certain threshold, that occur in the unit of time.

P. 4423. line 22: It would be more correct to say: seismic energy released per unit of time by earthquakes with magnitude greater than the magnitude of completeness.

Eq. 1: The first parameter used to monitor the time evolution of the seismicity of the investigated area is the logarithm of the *number of earthquakes per unit time*. The number of earthquakes per unit time should be estimated, in fact, by the argument of the logarithm in equation 1; I think that this formula is written incorrectly: i is used as the index of summation, it is therefore be an integer, its minimum value is set equal to t-w; it would be appropriate to explain how the time has been measured to meet this condition. I believe that even the upper limit of the summation should be a time, not a number of earthquakes in a time interval. It seems to me, also, that the argument of the summation should not be the same index i.

P. 4425. line 12: Authors should say whether the magnitude values listed in the catalog are directly related to the moment magnitude scale, or whether they were transformed by an empirical correlation law.

P. 4425. line 13: The meaning of the times t and w should be explained more clearly. As mentioned in the next paragraph it seems that w indicates the length of the estimation time window and t the advancement step of this window.

P. 4425. line 16: Justify the formula used to estimate the standard deviation of log N.

Eq. 2: Also this equation should be corrected as can be seen comparing it with the equation 1 in Papadopulos and Baskoutas (2009) or with the equation 3 in Zavyalov (2005).

Eq. 3: also equation 3 should be revised and properly discussed. It is not clear if the authors want to determine the average energy of the earthquakes occurred in a given time interval (about one year in this application) or the energy released per unit of time (one month in this application). Authors should also indicate the empirical correlation law by which they have calculated the energy of the seismic events.

The general suggestion that I would give to the authors, aimed at redrafting the description of the three parameters is: to split, initially, the whole analysis period in a number of indexed contiguous elementary time windows, with amplitude equal to the advancing step. It should also be easy to specify the size of the calculation window in terms of this index.

P. 4426, lines 12-13: The area indicated in the text does not coincide with that shown in Figure 2. Which is the correct one?

P. 4426, line12: The authors state that in the studied area, there are two major seismogenic volumes. Assuming independence between the seismogenic processes taking place in each of them, it would be appropriate that the authors also analyze, separately, the seismic activity of the two seismogenic volumes.

P. 4426, lines 19: Fig. 4.

P. 4426, lines 21: For normally distributed errors the confidence level of the interval +/- sigma is about 68 %.

P. 4426, lines 23: Vertical lines.

P. 4427, lines 1: Averaging window.

P. 4427, line 2: How have been optimized the analysis window width and the advancement step?. What is effect of the variation of these parameters on the final curves. How the minimum magnitude for which there is a significant correlation between estimated high probability intervals and actual occurrence of earthquakes depends on the width of the analysis window,?
Would be interesting that the authors show the results obtained using, at least, another smoothing window of different length.

P. 4427, lines 5-10: It is not clear the meaning of this sentence. Better express the concepts. In particular, it is not clear because the curve relative to the first parameter is not considered reliable, since it is clearly correlated with curve 3, as we expected because of the narrow range of variability of the energy of the earthquakes considered.

P. 4427, lines 17-20: It would be interesting to analyze the trend of the parameter log $E^{2/3}$ in the absence of the earthquakes reported in Tab. 1 and Fig 5.

P. 4428, lines 5-8: It seems that there is not an exact match between the data of Table 2 and the abscissas of the relative maxima and minima identified in Figure 5. Why?

The description of the trend of the signals should highlight the most significant aspects of it; for example, the regularity of the distances between consecutive relative minima and maxima., the range of variability of the parameter, etc.

The association of intervals where the curve has decreasing trend with periods characterized by low probability of occurrence of events of great magnitude should be supported by further theoretical considerations (possibly) and referring to other applications reported in the literature.

P. 4428, line 20: Three parameters were calculated, but the trends are probably significant only for two of them.

P. 4428, lines 23-25: I believe that the authors should specify: the process of preparing an earthquake with a magnitude greater than a given threshold. It is not clear the meaning of the statement of lines 25-27; reformulate or remove.

P. 4429, lines 5-7: Authors should also provide estimates of the variability of the two periods of which provide the average value.

P. 4429, lines 8-11: It does not seem that there is clear evidence that at the end of 2013 we can place the beginning of a period of low probability of occurrence of events with M> 4.

P. 4429, lines 11-14: This work is useful for the assessment of the method FastBEE but certainly not enough to declare that this method allows the evaluation of the seismic hazard at a given time in a specific place.