Dear Authors,

It was pleasure to read this paper. It could be a contribution to the actual knowledge in the field of possible precursor signals of the earthquake activity, but I have a number of severe reservations right now.

The goal of your paper it is quite clear, namely to conduct a correlation analysis among earthquakes occurred in Sichuan region (China) and satellite TIR anomalies highlighted by RST methodology and RETIRA index.

Main comments

Whole paper needs to be rewritten in a better English. In particular some sections, like the paragraph 3.2 (RST methodology) or paragraph 5 (Discussion), are not clear and only after various readings the paper can be understood.

The major part of citations should be revised, both in the form (sometime given name is used instead of surname like at line 6 of page 2) and in content (some wrong citations have been used or some important citations miss, as for RETIRA index).

To identify thermal anomalies possibly related to impending earthquakes, you used LST (Land Surface Temperature) products retrieved by the radiance collected by MODIS sensor on board of the polar satellites EOS/AQUA and EOS/TERRA.
Taking in mind that Authors who proposed the RST approach shown the advantages offered by the use of sensors onboard of geostationary platforms instead of sensor onboard of polar satellite packages (see the paper Filizzola, C., N. Pergola, C. Pietrapertosa, and V. Tramutoli (2004), Robust satellite techniques for seismically active areas monitoring: a sensitivity analysis on September 7, 1999 Athens's earthquake, Phys. Chem. Earth, 29(4–9), 517–527, doi:10.1016/j.pce.2003.11.019), since 2004 the major part of RST applications to thermal monitoring of seismogenic areas have been carried out using TIR satellite records acquired by sensors onboard of geostationary satellites (as also you have reported in the your paper). Now, my question is why you prefer to use EOS/MODIS data instead that TIR records collected by sensor on geostationary platforms (e.g. the Japanese MTSAT satellite)?
Moreover, LST (Land Surface Temperature) products have been take in account. LST products are very useful to reduce variability of atmospheric water vapor, but in the computation of LST several approximations are necessary (e.g. emissivity, total water vapour content, ecc.), which should produce errors (also of 4-5 K degree) in the satellite LST estimations. Taking in mind, that thermal anomalies possibly related to seismic activity are of low intensity, wrong LST estimation could mask and/or generate false anomalies. Have you an idea of the impact of this errors on the your analysis?

Earthquake catalogue (China Seismic Information; http://www.csi.ac.cn/) used to verify possible correlation with TIR anomalies is inaccessible. Please provide a correct URL. Anyway, consulting a different seismic catalogue, i.e. UGSG catalogue (https://earthquake.usgs.gov/earthquakes/search/), using a similar criteria (M≥3.5; Depth >0; region from 25°N to 40°N and from 95°E to 110°E; time since August 1, 2002 up to April 15, 2018) I found 2369 earthquakes, respect to 3615 seismic events reported in the your paper. A comparable numbers of seismic events, i.e. 3828, is obtained when the USGS catalogue is consulted starting by 1965. Have you use seismic data from 2002 or from 1965? In the first case (i.e. 2002) how you explain this difference (2369 vs 3615)? In the second case (i.e. 1965), because MODIS data are available since 2002, how is possible found some relations among TIR anomalies and earthquakes (before 2002)? In this last case, please provide a correct analysis.

About the performed correlation analysis among the appearances of TIR anomalies and earthquake occurrences, you should be in mind that working in the optical band, a wide presence of meteorological clouds, as well as the

lack of satellite data, do not allow to give continuity to the observations, which is necessary to identify possible TIR anomalies or to fully appreciate a possible space–time persistence of previously occurred TIR anomalies, producing in this way a possible overestimation of missed events. Please, consider this suggestion and provide a more convincing analysis. As consequence also your conclusions should be reconsidered.

Specific comments

Page 1 - Lines 18-19; In the abstract, you announced that a refined RST data analysis and Robust Estimator of TIR Anomalies (RETIRA) index were used, but in the text I have not read any new improvements to the RST methodology, if not those reported in Eleftheriou et al. (2016). Otherwise please explain better the refinements made to the RST technique. Moreover, add the reference of RETIRA index.

P1 - L25; Please provide the complete name of PPV, FDR, TPR and FNR.

P1 - L26; The sentence "the prediction ability of RST in Sichuan area is limited" is too strong!

P4 - L34-36; I not understand the sense of this sentence "Moreover, Tronin indicated that the anomaly was sensitive to crustal 1000kmearthquakes with a magnitude more than 4.7 and for distance of up to 1000km" in this position.

P5 - L14-24; Cloudy pixel, as well as pixels declared as edge clouds, should be exclude before the computation of $\Delta V(r,t)$ otherwise effects due to cloudiness are not removed and false TIR anomalies could be generate.

P5 - L25-31; How you identify the extreme weather events (e.g. blizzard)?

P6 - L9-13; The reference Saraf et al. (2009) is correct? I not found no mention about effects of cloudy pixels on ΔV in this publication.

P6 - L13-20; *Cold spatial average effect as reported in* Aliano et al. (2009), Genzano et al. (2010) and Eleftheriou et al. (2016) could affect the whole TIR scene. Rightly you have take in account this effect, but in opposite way of Genzano at al. (2015) or Eleftheriou et al. (2016) you work at pixel level instead of whole scene level. In this way, the above mentioned effect could be not removed in the computation of reference fields.

P6 - L32-35; The sentence "This process should be paid more attention, because in the past papers, this process is always ignored." is wrong. In all applications of RST approach, kσ-clipping method (always applied) guarantee to remove outlier (i.e. extreme events) from the computation of reference fields. Please, consider to rewrite better the sentence.

P6-P7 (Change detection step); Although you have announced the computation of ALICE index, the index reported in the equation 13 should be the RETIRA index (correct equation can be found in Filizzola et al. 2004; Tramutoli et al. 2005). Please, correct it.

P7 L9-23; The criteria used to identify TIR anomalies are the same introduced for the first time in Genzano et al. (2015) and in Eleftheriou et al. (2016) in order to indentify Significant Sequences of TIR Anomalies (SSTAs). Please consider to call it in similar way, mentioning these two publications.
Moreover, starting from a mathematical point of view you have consider to set a threshold K equal 2, if you have a normal distribution (Gaussian) a 2 times the standard deviation could be sufficient to identify anomalies. In addition, RETIRA index (as well as ALICE index) give the possibility to evaluate in term of SIgnal-Noise ratio (S/N) the intensity of anomalies (see Tramutoli at al. 2001 or Tramutoli et al. 2005 for more details). Please, take in mind this suggestions when choose threshold k.

P9 L3; Period B is the same of period A (i.e. from2002.09 and 2007.12)?

P10 Fig.4; Figure shown are not a good example of TIR anomalies possibly associated to earthquakes. In the example on the right part (TIR map of 2010/10/22) earthquakes seems not satisfy the rules announced in chapter 3.3. Moreover, to show the whole sequence of TIR anomalies, not only one day with TIR anomalies, could help the reader to better understand the concept of Significant Sequence of TIR Anomalies.

P11 Fig.5; As reported in the caption "The cells in the blue rectangle mean that this day is affected by a large area of clouds, ...", now, some days with TIR anomalies belonging to several sequences of TIR anomalies (i.e. 2, 10, 27, 32, 35, 36, 37, 45, 50, 58, 59) are affected by a wide cloudy coverage, all this lets thinks that TIR anomalies due to meteorological effects are not removed from the analysis (as suggested in Eleftheriou et al., 2016).

P12 L21-33. Rightly, you are reported that cloudy coverage could prevent to observe with continuity the presence of TIR anomalies, this is a intrinsically limitation of satellite technologies which work in the optical band, and not of RST methodology. Please revise your sentences.

P13-16 Paragraph 4.3 (The evaluation of earthquake prediction ability for RST); The performed analysis not have any sense if carried out in this way. Mainly, the analysis on the rate of earthquakes which correspond ("TPR") or not ("FNR") to TIR anomalies it is very complicated to perform, because gaps in observations, due to the lack of satellite data or to a wide presence of meteorological clouds make impossible to give a continuity to the observations, which is necessary to identify possible TIR anomalies or to fully appreciate a possible space–time persistence of previously occurred TIR anomalies, as consequences the relation one to one (earthquake-TIR anomalies) that you are looking is corrupted by this limitation. Anyway, before to comment the results of a some kind of sensitivity analysis this circumstance should be announced.