



Interactive
Comment

Interactive comment on “Brief Communication: An exclusive example of surface latent heat flux variation before Russia M6.1 earthquake” by Y. Jie and G. Guangmeng

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This short paper presents a case study where a satellite-based measurement of a surface latent heat flux (SLHF) variation occurring before a large earthquake (2011 M6.1 in Russia) seems to be actually due to a concomitant cloud coverage. These authors emphasize that comparing the high temporal resolution SLHF data and the simultaneous cloud satellite image is important to make a final conclusion of earthquake precursor. Essentially the work would confirm some doubts already pointed out by Blackett et al., 2011.

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1. About the use of a higher temporal resolution.

I suspect that the use of a higher temporal resolution (6-hour averages instead of 24-hour averages) could be misleading. The SLHF time series has a clear daily variation, with the greatest value at midnight, and the lowest one at 18Z (typical maximum and minimum of this area of the world). The presence of a cloud exactly at midnight is not a proof of data analysis fault, because it could be a mere coincidence: in particular, please note how the value at 06Z on 9 October 2011 is the highest among all the other days values at that time. Therefore, it would be of great help to see if the cloud is still persisting in that position also after 6 hours, when the SLHF is still anomalous with respect to all the other days. A picture of the clouds at this time would be fundamental to support the real possibility of the cloud contamination to the SLHF data analysis. In some way the analysis with daily mean values is more robust because takes into account all values of the day, to which not only the (anomalous) 0Z value contributes but also the (still anomalous) 06Z, both the highest values (for their corresponding times) in the period of observation.

2. About the applied algorithm.

I think that the algorithm that these authors use is not the best (these authors refer to older works by Qin et alii) because it is a single-parameter analysis. More recent works (e.g. Qin et al., 2013, 2014 and He et al., 2012) improve the method to identify a precursory pattern before a large earthquake extending the analysis not only to SLHF but also to other physical parameters, in order to have a more robust result. In particular, the work by Qin et al., 2013 shows some compelling evidence for the presence of real precursors, based on application to a number of earthquakes globally.

3. About the main conclusion.

Jie and Guangmeng say (p. 352, line 9) that “We find that the high SLHF is due to a thick cloud”. I think that this sentence is not correct. If we see Fig. 4 we notice that in correspondence with the great cloud in 55N, 135E there is an SLHF value of less than

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100, so much less than in 54N, 124 E, so the effect of clouds to SLHF is not always to increase its value. A more reasonable conclusion would be (after verified the previous point 1): “The anomaly SLHF variation is contaminated by a thick cloud seriously so we cannot establish whether the anomaly is connected with the earthquake or is not”.

4. Minor points.

The English is rather poor and should be improved. I provide below just some suggestions:

-In the title please write “. . .before 2011 M6.1 Russia earthquake”

- p. 348, line 5. Please write “. . . with satellite cloud image. . .”

-p. 348, lines 16 and following : “. . rock fracture. According to some authors (e.g. Freund and Ouzonunov, 2009), prior to an earthquake the stress accumulation results in a thermal infrared emission, which enhances. . .”

-p.349, line 17. Please write “. . . SLHF data is daily. Here . . .”

-p.349, line 20. Authors speak about “. . .previously published results”. This seems that previous papers studied SLHF of M6.1 Russia earthquake. Is it true? If it is, please cite some references. Otherwise if you refer to works analyzing SLHF but for different earthquakes, you should mention this.

-p.349, line 22. Please write “ The SLHF data is provided . . .”

-p.350, line 1. Please indicate the time (at least hour and minute) of the day of the earthquake.

-p.350, line 99.Please write” try to find any possible anomalous day, considering the daily values from 1 September to 30 October 2011. . .”

-p.350, line 10. Please write “. . . We can see from Fig.1 that the SLHF on 9 October . . .”

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-p.350,lines 12-13.Please write “... multi-years mean values which represent the normal background, to get SLHF as given by Qin et al. (2011):”

-p.351, line 9. Please write “This conclusion agrees apparently well with Dey and Singh’s result (2003) ...”

-p.351, line 12. Please write “00:00 UTC, we check the satellite ...”

-p.351, line 28. Please write “do the data mean, or one will get a wrong conclusion.”

-p.352, lines 5, 6. Please write “... All the results are well in accordance with previously published researches for other case studies. ...”

-p.352, line 12. Please write “...precursors. A similar situation is the occurrence of thermal anomalies before earthquakes as widely studied. ...”

-p.352, line 23. Please write “exclude possible false precursors.”

-p.352, line 25.Please write “... Their help is greatly ...”

-p.356. Fig.2 Caption. Please write “SLHF daily change ...”

-p.359, last line of Fig.5 caption. Please correct “epicentral area.”

5. Concluding remarks.

The paper needs more analyses: a cloud at midnight on the anomalous day is not enough. Please verify how long the cloud persists over the epicentral area; in particular, what about 6Z time on 9 October 2011? If the cloud was not over there, how explaining the still anomalous value at that time? Moreover, please consider that more recent papers on the topic use more parameters, and the influence by clouds is generally different over each parameter. Some statements about this aspect are also important to provide more insights to the potential reader. Finally, to be provocative: in case the authors confirm the persistence of the cloud over the epicentral area, are they sure that the presence of the cloud is not connected with the impending earthquake? This

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is what practically say not only Morozova (1997) and Shou (1999) as mentioned in the text, but even the same authors in another recent paper (Guangmeng and Jie, 2013), which is curiously not mentioned in this work.

References

Blackett et al., Exploring land surface temperature earthquake precursors: A focus on the Gujarat (India) earthquake of 2001. *Geophys. Res. Lett.*, 38, doi: 10.1029/2011GL048282, 2011.

G. Guangmeng and Y. Jie, Three attempts of earthquake prediction with satellite cloud images, *NHESS*; 13, 91-95, 2013.

Qin et al., A deviation-time-space-thermal (DTS-T) method for global earth observation system of systems (GEOSS)-based earthquake anomaly recognition: criterions and quantify indices, *Remote Sens.*, 5 (10), 5143–5151, 2013.

Qin et al., Surface latent heat flux anomalies quasi-synchronous with ionospheric disturbances before the 2007 Pu'er earthquake in China, *Advance Space Research*, Volume 53, Issue 2, 266–271, 2014.

He et al., A nonlinear background removal method for seismo-ionospheric anomaly analysis under a complex solar activity scenario: a case study of the M9.0 Tohoku earthquake, *Adv. Space Res.*, 50 (2), 211–220, 2012.

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