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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

Y. Jie and G. Guangmeng

guogm@igsnrr.ac.cn

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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

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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.

R: As the reviewer request, here we give the image at 06z. The details will be explained in part 5.

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.

R: We agree that multi-parameter will make the result more reliable and robust. Here in this paper, we just focus on one parameter SLHF, as Matthew just focuses on LST in his paper.

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 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”.

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R: This is a very good correction. Our original sentence has somewhat misleading. This correction pointed out what we want to express accurately.

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

R: Thanks the reviewer for these English words corrections. We accept these corrections.

5. Please verify how long the cloud persists over the epicentral area; in particular, what about 6Z time on 9 October 2011?

R: The satellite image shows that this thick cloud is continuously moving from 0z to 6z.

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.

R: We agree the reviewer's suggestion in part 3: In Fig. 4 we notice that in correspondence with the great cloud in 55N, 135E there is an SLHF value of less than 100, so much less than in 54N, 124 E, so the effect of clouds to SLHF is not always to increase its value. So the cloud was there, or was not there, it has no relation with the high SLHF. SLHF is mainly controlled by wind speed and temperature. Here we give some evidences: 1. The weather station at E123.9, E54 recorded 5mm rainfall on Oct 9, 2011. So a high SLHF here is normal and reasonable. 2. In epicenter area, the V wind changed from 4m/s at 1000hpa to 13m/s at 850hpa, which means a strong vertical convection. At point 55N, 135E, where the reviewer think there is a thick clouds there but no high SLHF value, the wind change is small, about 2m/s to 4m/s from 1000hpa to 850hpa. 3. Seen from the satellite image of visible band, clouds at epicenter area have some rise and fall which means a high convection, while cloud at 55N, 135E is

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very homogeneous, which means little convection. So low SLHF value at 55N, 135E is reasonable. 4. Even the thick clouds move away from the epicenter area, there are still many thin clouds and fogs left in the epicenter area. 5. In SLHF data, there are many examples that the high SLHF area did not move within 6 hours. It is a very common phenomenon.

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 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.

R: We have written in paper: Because of the limited data, we did not know whether this cloud is related with the quake. Again, this paper's aim is not to study the relation between cloud and quake, the aim is to remind scientists to notice the clouds and weather interference. We prefer to that our paper cited by other scientists, not by ourselves. So it is not curiously.

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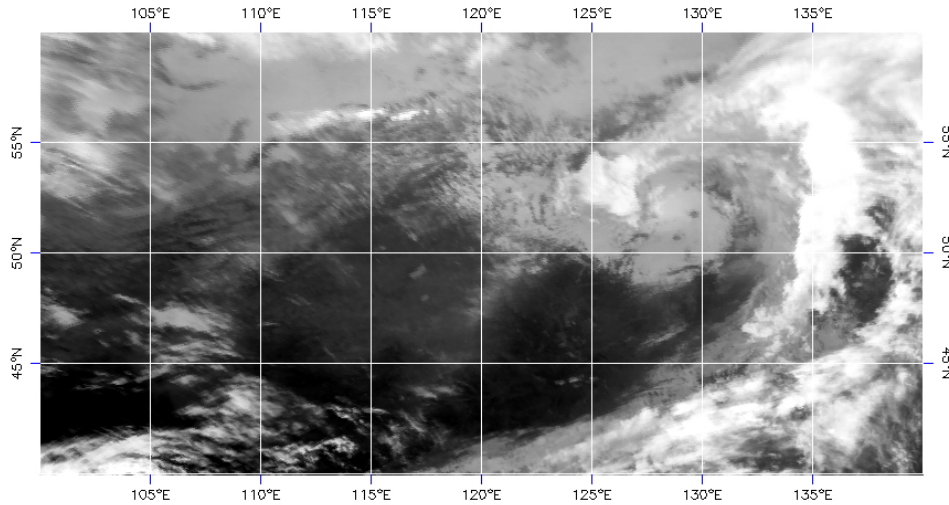


Fig. 1. Satellite image of Russia and Northeast China at 06:00 UTC on 9 October 2011

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