Response to Dr. Geller

This paper is basically OK but minor to moderate revision is needed. The paper concentrates too much only on a specific evaluation of particular precursor claims. It would be useful to put this in a more general context of the long and unsuccessful history of quake prediction research (see Kagan, 1997, and Geller, 1997, both in the Dec. 1997 issue of GJI).

A: Thank you very much for your suggestion. Although we only focused on surface latent heat flux in the present paper, we have been employing this evaluation procedure on another parameter (i.e. land surface temperature) and trying to find some more meaningful facts in the future. We have consulted these two papers that you mentioned above and incorporated them in our manuscript. Please see line 61 ~ 63.

Also, it would be useful for the authors to try to outline a protocol for hypothesis testing of surface latent heat flow claims going forward, as a guide for future work in this field.

A: Thank you for your comment. We added a protocol in the section 5.

In lines 108 to 109 the authors define 00, 01, etc. This should be moved to a separate table, to make it easier for readers to find.

A: The description of classification has been moved to Table 2. The related table numbers also have been modified.

The discussion in lines 242-244 of the “68-95-99.7” rule is oversimplified. This is a best case. It is well known that nominally statistically significant results can actually be noise because of factors left out of the model (see P. Anderson, Jan. 1992, Physics Today).

A: In fact, the “68 -95-99.7” rule is referred to show the intrinsic characteristic of SLHF data. Many former researchers stated that the values which are beyond $\mu+2.0\ \sigma$ or $\mu+1.5\ \sigma$ can be considered as thermal anomalies. However, according to our study, the correlation of SLHF anomaly and seismic activity is relatively low (due to chance) and largely depends on several factors including data and parameter. In other words the so-called abnormal values might be regular fluctuations due to seasonal factors instead of seismicity. We agree with the reviewer’s opinion that some significant results may result from other factors which are not included in the present model. Further work including the variations in seasonal wind and ocean current will be carried out. More information can be found in section 5 of the revised manuscript.

The English should be edited if possible. There are many typos (e.g. “fisrtly” in line 119) and many examples of awkward wording (“should be payed enough attention” in line 274).

A: Thanks for your kind warning. Two native English speakers have helped us to re-edit this paper. All of those corrections were highlighted.

I would be happy to review a revised manuscript if asked to, but if the editor thinks the revision is OK then I would have no objection to its being accepted without being sent to the referees.

A: Thank you very much for your valuable suggestions.