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Interactive comment on "Pre-seismic Thermal Anomalies from Satellite Observations: A Review" *by* Zhong-Hu Jiao et al.

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On the surface, this is a fine review paper with a great deal of information presented in a comprehensive way, obviously with the intention to provide an overview of the most widely quoted interpretations of pre-earthquake thermal infrared anomalies.

What is the role of peer review? Is it to check whether the authors have well covered their chosen topic according to majority consensus or is it to check whether the authors have/have not penetrated through the thicket of misunderstanding that is rampant in their discipline. My evaluation of this review paper is primarily a critique that points at some very widespread and common misunderstandings in the Remote Sensing community when it comes to signals that the Earth emits prior to major earthquakes.

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Since the authors cite one of my papers prominently at the beginning (Freund, F.: Pre-earthquake signals: Underlying physical processes, J. Asian Earth Sci., 41, 383-400, 2011), I feel compelled to point to some shortcomings of this manuscript – serious from my perspective. For me the question arises:

In the Introduction, on L 27-33, the text reads:

"Tectonic earthquakes are caused by the sudden dislocation of active faults due to surging tectonic stress (Freund, 2011). In addition to the considerable amount of strain energy released during the earthquake itself, the stress energy continuously accumulates during the preparation process of the earthquake. The activity of faults in earthquake-prone areas often results in the growth of surface microfissures and gas ionization effects, following with changes in water content, underground gas, and earth electromagnetism around active faults. To some extent, these changes lead to pre-seismic thermal anomalies in seismogenic areas, such as regional warming and increased greenhouse gas concentration, which can be observed through satellite sensors."

In my cited 2011 publication I had presented at great length that the most important processes during the earthquake preparation process are (1) the build-up of tectonic stresses and (2) the activation of omnipresent defects in all crustal rocks, which releases electronic charge carriers, called "positive holes". (1) is obvious and accepted by everybody. (2) is based on a large body of work that I have published since the 1980s, first in a basic material sciences context, unrelated to earthquakes, but subsequently applied to earthquakes and, specifically, pre-earthquake processes.

Though the authors of this Review cite my 2011 paper, they seem to have missed or misunderstood its contents. This is obvious from their list of follow-on processes to which the authors draw attention, namely "…the growth of surface microfissures and gas ionization effects, following with changes in water content, underground gas, and earth electromagnetism around active faults… regional warming and increased greenhouse gas concentration," This list indicates that the authors are intent on reviewing the relevant literature without taking into account my work. Why, then, do they cite my 2011 paper at such a prominent place?

Frankly, despite the near-universally cited 1968 BSSA paper by Chris Scholz "Microfractures, aftershocks, and seismicity", nobody has ever presented evidence that microfracturing is taking place in the Earth crust, either at the surface, in shallow depth or at great depth. Nobody seems to have ever raised the question, whether it is possible for rocks at seismogenic depth (7-45 km and deeper) to undergo microfracturing. I mind you, every fracture event, micro or macro, is possible only, if the volume can expand. The reason is that, by definition, fracturing creates new surfaces. Creating new surfaces is possible only if and when empty space is created between the two sides of the crack. However, at the depth of kilometers to tens of kilometer, the overload of the rock column is such that the amount of work to be done (thermodynamically) to increase the volume of the stressed rocks is very large. Hence, the chance of creating any fracturing, micro or macro, is very small. Nonetheless the geoscience community, including the authors of this review, blankly accept the microfracturing maxim.

If one digs deeper into why microfracturing is so popular, an interesting story emerges. Geophysicists have for decades noted increases in the electrical conductivity of the rock volumes deep in the crust that are being stressed prior to major seismic events. Nobody could explain such increases except by assuming that brines were penetrating into the stressed rock volumes. Hence, the assumption that fractures must be opening deep below allowing water to rush in. This facile explanation was so tempting that nobody seems to notice that this assumption contradicts the fact that, below 5-7 km depth, the open porosity of rocks disappears. The reason: the difference between hydrostatic and lithostatic pressure becomes so large that open porosity cannot be maintained – even not over geologically short time scales.

Throwing in the words "gas ionization effects" reinforces the impression that the authors have not made an effort to inform themselves about HOW air ionization at the Earth surface takes place. Likewise, what do they mean by writing "with changes in

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water content, underground gas (and) earth electromagnetism around active faults"? These are meaningless words unless substantiated by some physical insight into the underlying processes. I have gone to some length describing the underlying physical processes in my 2011 paper, including electromagnetic processes and air ionization at the Earth surface. I have the impression that the authors of this review have not made an effort to familiarize themselves with these processes.

I can go on with my critic (which I offer in a constructive spirit) when I read in L49 "due to the unclear physical mechanism of pre-seismic thermal anomalies". I for one posit that the physical mechanisms are no longer "unclear". The authors' misconception comes from the fact that they don't realize the difference between "thermal anomalies" and "thermal infrared anomalies". The difference is huge– from a physics perspective. Saying "thermal anomalies" automatically implies a temperature difference, e.g. a "tangible" Joule temperature difference. Saying "thermal infrared anomalies" refers to the ONLY observables that infrared-sensing satellites can deliver: intensity and, to some extent, spectral distribution of the infrared emitted from the ground, from the lower atmosphere and from the top of the atmosphere. All of remote sensing depends upon the interpretation of these infrared emission processes.

Much of the remaining paper endorses, either implicitly or directly, the conventional interpretation of the different kinds of remotely sensed pre-earthquake IR anomalies. I'm convinced that the remote sensing community has been on the wrong track for most of the time, I but hesitate to express my concerns. The reason is that my concerns are so fundamental that, if rigorously applied, not much is left of this review paper to recommend. However, I want to help the authors.

For instance, on L428, late in their Review, under 3.6 Other methods, they introduce the night thermal gradient (NTG) method, first used by Nevin Bryant at JPL and then applied extensively by Luca Piroddi and Gaetano Ranieri in Italy as quoted in L430. Regrettably, the authors continue to use the blanket terminology "surface, soil and air temperature" without mentioning that they are actually talking about the "radiative

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temperature" derived from infrared emission off the surface, the soil and the air.

Why is the NTG method introduced so late in this review and under the title Other methods? The authors do not realize that, by using data from the European geostationary satellite (providing thermal images every 15 min) Piroddi's work has provided much more profound information. For instance, by analyzing a full year of night-time data for the entire Italian peninsula, Piroddi has shown (1) that regional TIR anomalies come and go over the course of time, in a matter of days, expanding over relatively wide areas, but only occasionally linkable to large seismic events, (2) that the TIR intensities wax and wane on time scales of hours, (3) that the TIR anomalies move across the landscape on time scales even shorter than hours, and – most importantly – (4) that the TIR anomalies have a clear tendency to be associated with hill tops and mountain tops. In fact, the intensity of the TIR emissions from valleys is much less than from the tops of adjacent mountains. If the authors of this review paper would have paid more attention to the work by Piroddi and his thesis advisor, Professor Ranieri, they would have noted that the populist interpretation of the TIR anomalies off the Earth's surface, namely that they are due to warm gases or greenhouse gases seeping out of the ground, must be fundamentally wrong. The NTG analysis clearly points to an alternative mechanism, for which I have laid the groundwork: IR emission due to the radiative de-excitation of peroxy entities at the Earth surface. I attach an extended abstract from the 2015 EMSEV Workshop, in which the preference of the TIR emission from mountain tops is unambiguously documented (at least for one well studied case, the M=6.3 2009 L'Aquila event).

All this also links to the Section 4 Issues with thermal anomaly detection. It is correct, as the authors note in L460, that the issue is "highly controversial", but they do not penetrate the superficial appearance of the widespread controversy. In L461 they use the word "warming". The casual use of this word reveals that they= authors do not understand the physical principle of the radiative nature of the remote sensing signals analyzed by the community.

In L512-514 the authors refer to the “unified LAIC model, widely promoted by Sergey Pulinets and his numerous collaborators. However, a close examination of the LAIC model reveals that it is based on ad hoc assumptions regarding radon. Radon has been proposed to the driver of the LAIC model even though, in the larger context, it is physically impossible that radon can play this role. If radon were responsible to the increase of air ionization prior to major seismic events, it would have to increase the normal air ion concentration from the “fair weather average” of about 200 per cubic centimeter to 20,000 to 50,000 per ccm. In average crustal rocks, radon is rarer than gold by 6 orders of magnitude. There is about one mole Rn in Earth’s atmosphere. Measured close to the ground or in holes in the ground, the pre-earthquake Rn emanation increases by a factor of about 10. Just calculate the number of Rn atoms per ccm of normal air and ask yourself, how the decay of these rare Rn atoms can cause a regional increase of the air conductivity by a factor 100-250.

In summary, this is a fine paper with lots of references, but it suffers from the fact that the authors do nothing but reinforce the mainstream conception that the question of the so-called “thermal anomalies” (which are in fact infrared intensity anomalies) is so complex that it cannot be solved. I disagree with this assessment. I regret that the authors have not been able to go beyond the simplistic and physically untenable explanations why there are changes in the IR emissivity of the Earth surface around earthquake preparation zones.

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