

In the following, we specifically comment on any questions, concerns or suggestions that reviewers have about the manuscript. We do not replicate below or address any of the positive comments of the reviewers other than to say that we agree and that we thank all three reviewers for their time and effort with our manuscript.

Author Comments to Jamie Molaro

"Have possible sources been identified yet?" (referring to the specific wind-temperature-event cluster that occurred on July 25).

Yes, as we continue to look at this data; we consistently note similar correlations between specific weather occurrences, temperature and events. A full analysis of this data is beyond the scope of this methods paper, but we can mention more clearly that this day is an example of typical days when events occur.

"The location data is much less clear, though that is to be expected using AE methods with real rock, rather than a homogeneous cube. It is not surprising that the events occur mostly in the northern hemisphere. The lack of E-W bias seems likely to be at least partly a function of the boulder size. Perhaps the cited modeling by collaborators can help to explain this. There does appear to be a slight bias towards the western hemisphere, but was not mentioned by the authors. Perhaps it is not statistically significant"

We are not sure what the reviewer means in terms of E-W being a function of boulder size. The data presented are for a single boulder, so we are unable to make comparisons using the data presented for rock size. We can mention this explicitly in the text. Yes, there does appear to be clustering in the western hemisphere, but we are not sure as to its significance and thus did not mention it. We present this data here to give the reader a general sense of the quality and robustness of the dataset.

"It is also interesting that the authors have identified tensile strains parallel to the surface. This would not be expected from modeling, and has important implications on the limitations of models, and how they may be improved. In general, the strain seems to track with the rock surface temperature, as expected. It would be interesting to know if the measurements show signs of permanent deformation during insolation cycles."

We have noted the same thing but feel that the depth of discussion necessary to speak to these points on strain is beyond the scope of this methods paper. We can, however, at a minimum mention that these results are at odds with models of such things and that it possibly is providing new insight into rock behavior. We also certainly have the capability to note permanent strain.

Reviewer #1

“ minor corrections On page 381 the “double dot” following the “various materials..” should be replaced with a single dot. One more suggestion: in Fig. 6, since we have the 3D array of AE hypocenters, the third subplot (view from the top, for example) would be nice.”

We concur with these suggestions.

Reviewer #2

The following specific comments and suggestions are offered for the authors' review:

“383, 28 – The authors are correct that establishing direct correlation between AE data and actual damage is extremely difficult. It may be relevant to readers to note that while the mode of cracking that occurs can be determined (with very complex signal processing), the AE data cannot distinguish between crack initiation and crack propagation, nor can a single event be directly translated to a change in crack length.”

We agree with this suggestion.

“393, 21 – The authors state that the surface of the rock specimen experiences tensile strains when expanding due to temperature increase. Theoretically, in a homogeneous body, compressional stress is experienced during expansion, and tensile during contraction. The authors should explain why it is their data shows a non-intuitive behavior”.

As was mentioned previously, we recognize this result as being counter to published models of stress and strain and a significant result of our dataset, however, we feel that to get into this discussion would require a complete analysis of the data and review of this modeling literature that is beyond the scope of this methods paper.

“Additionally, the strains being measured are at the boulder’s surface, normal to which grain motion is largely uninhibited. The authors should discuss whether these measured strain values represent a comparable experience to that of the boulder’s interior where the majority of the AE events occur.”

Again, we feel that this level of discussion of the implications of our data is beyond the scope of what we can reasonably address herein. We appreciate the complexity of the dataset and the possible interpretations that result from it.

“Figure 5 – The text in this figure is small and extremely difficult to read. Additionally, the authors should explain more clearly the orange boxes displaying “Total Events,” and what “total” means if more than one box is displayed on each plot.”

We agree with this suggestion.

“373, 11 – Molero should be Molaro383, 4 – spare parenthesis at “equipment, sensors), and: : :”

We agree with this suggestion.

“383, 15 – The authors note that an AE “hit” is also referred to in the literature as a “count,” but this is inaccurate. According to the sensor documentation from PAC, a hit is registered after the signal exceeds a pre-defined threshold for the first time, and refers to the entire acoustic wave. The counts (or AE Threshold Crossing Counts) feature indicates the number of times during a hit that the signal rises above the threshold. See the figure below from the PAC documentation. The parenthetical statement that is in error should be removed and the remainder of the manuscript should be checked to ensure the correct and desired terms are being used throughout. For example at (395,25) the authors state a finding from a previous study. It is unclear as to whether or not they are discussing counts correctly in this paragraph due to this mis-definition.”

We thank the reviewer for pointing out this important distinction. We are indeed referring to “hits” as they are defined in the PAC literature and as the reviewer defines them above, not as counts. The written statement that they are the same thing, is however, incorrect and should be fixed. On page 395 line 25, when we say “counts of AE events”, we just mean numbers of events that have been counted. We can clarify this text given the specific definition of “count”.

“388, 9 – “that used” should be “that were used”

391, 19 – The authors refer first (line 3) to a four-month period, but state here that the test period is three months. Later (page 393) the authors refer again to a four-month period. The length of the period used for this preliminary analysis should be made clear.

392, 8 – “an prominent” should be “a prominent”

We agree with these suggestions