

# ***Interactive comment on “Monitoring long-term changes of glacial seismic activity with continuous seismological observations: a case study from Spitsbergen” by W. Gajek et al.***

## **Anonymous Referee #1**

Received and published: 16 February 2016

Review of Gajek et al. “Monitoring of long-term changes of glacial seismic activity with continuous seismological observations: a case study from Spitsbergen” Under consideration for publication in The Cryosphere, 16 February 2016

This manuscript describes work classifying seismic events recorded near glaciers in Spitsbergen and describes variations in the occurrence rate of these events. The authors identify correlations between seasonal weather data and the seasonal occurrence of these events and an increase in the number of events. Their method to identify the origin of detected seismic events is new to glacier seismology.

Despite these efforts, there are a variety of issues with the present manuscript that I suggest the authors resolve prior to publication. These items limit the reproducibility

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of the work, the originality and significance of the conclusions, and the extent to which the work can be understood. My major concerns, which I expect will require significant time and effort, are briefly summarized below, with more minor line edits following. Following significant revision, a new manuscript may be appropriate for publication.

Major comments: + The methods are extremely difficult to follow. It is unclear how the NED will evolve over time, or how the noise function was calculated. The event classification criteria (in two numbered lists on p. 5) are ad-hoc and are presented without justification. The explanation of the fuzzy logic algorithm is very hard to follow and there is not nearly enough information provided to allow for interpretation of Figure 3. Inclusion of sample waveforms, illustrating the different criteria, would be of great help. My understanding is that “ice vibrations” are calving icequakes. Is this not the case?

+ There is no description of the origin of the weather data or how the positive degree days are calculated. These should be part of the methods.

+ The value of the fuzzy logic criteria is not clear, since about 60% of seismic events are not classified, nor is it clear how the different types of events differ from each other. Why do the authors believe this approach was useful? Perhaps if the method were more clear, its impact would likewise be more easily appreciated. How do the authors know to attribute the “Not identified” events to the nearby glaciers?

+ The conclusions are not new. Kohler and others (Polar Research, 2015, 34, 26178) published a paper last year drawing on the same seismic signals, using more seismometers and applying more compelling analyses to these data. Kohler and others convincingly link the icequakes to calving events and reveal a seasonal cycle nearly identical to that reported in the present manuscript. Luckman and others (Nature Communications, 2015) also produce time series of frontal ablation rates that will contain calving events with similar calving events. The present authors cite both of these studies, but it is not clear how the present work is different than or similar to these existing

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studies. The authors have the opportunity to advance our understanding of calving seismicity and calving through more careful comparison to these existing studies. As it stands now, the conclusions are both weaker and more inconclusive than the conclusions of previous studies.

- + The descriptions are unnecessarily qualitative in a number of locations within the text, for example when adjectives such as “major” or “minor” are applied without definition.
- + The quality of the writing needs improvement prior to publication.

Line edits follow: p. 1 L 12: remove the first “the”

p. 1 L 13: “over many years” is redundant

p. 1 L 20: What is “energy flow analysis?” Energy of what? This is not described in the main text.

p. 2 L 16-19: Please provide more context about these “ice vibrations,” since they appear throughout the present manuscript. Comparison of the Gorski literature with other papers published on glacier seismicity (by O’Neel, Bartholomaus, and Kohler) suggests that the ice vibrations might be calving icequakes.

p. 3 L 25-26: Please define what you mean by “major” and “minor” here.

p. 4 L 1-2: What do the authors mean by this?

p. 4 L 4-5: This conflicts with the earlier statement that the seismic data is available in the IRIS DMC databases.

p. 4 L 20-23: How is this an energy density? Do the authors use velocity seismograms? Subtracting the noise from the absolute value of the ground velocity doesn’t make an energy.

p. 4 L 24-25: Please provide more information regarding how the noise function was calculated. How was the noise fit? What’s the size of the moving window? How do you

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know that no event occurred (i.e., based on what criteria)?

p. 4 L 27: It appears to me that the NED as defined in Equation 1 would increase consistently through time. I don't see how these thresholds work to trigger detections in the monotonically rising NED values. How were these thresholds chosen?

p. 5 L 5-8: What are the justifications for these criteria? Glacier-produced calving icequakes can sometimes exceed 25 s (Bartholomaus and others, 2012 and 2015, in JGR)

p. 5 L 6: What kind of variability is intended here? in the spectra, or over time?

p. 5 L 18-21: It is hard to understand what the authors intend by these sentences. How are the amplitudes smoothed?

p. 5 L 22-23: This description could be aided by an illustration.

p. 6 L 3: What kind of event analysis? How were the events analyzed?

p. 6 L 7: What is "strong and steady energy flow"? This is language not traditionally used in seismology.

p. 6 L 21: What is the "strictly year-long pattern"? Do the authors mean "seasonal"?

p. 6 L 23-25: The assumption that the "not identified" events are glacier-generated because their occurrence varies seasonally is very weak evidence. How can the reader know that they're not rockfall, or river produced, or artifacts in the data? How is "false" different than "not identified"?

p. 7 L 10: What do the authors mean by "slightly blurred"?

p. 7 L 13: Fig. 6b shows PDD, not temps. But the PDD that's shown doesn't look like other typical PDD values. The positive degree days values are the cumulative daily temperatures above 0 degrees (as described in Hock 2005 and other papers). This looks to me like the number of days per month that exceed 0 degrees.

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p. 7 L 14-16: What mechanism is implicated here? This is extremely loose and imprecise language.

p. 7 L 17: Monthly temperatures are not shown. Please plot if discussed.

p. 7 L 24: “doubling” instead of “double increase”

p. 7 L 29: plot the annual PDD here.

p. 8 L 20: What are the authors implying here? What is the connection between the glaciated surface area and the number of seismic events? I believe that Kronebreen is a much faster-flowing glacier than Hansbreen. That might explain more calving at Kronebreen than at Hansbreen. What about the detectability of these signals? Are the seismic stations equidistant from glaciers? Perhaps attenuation might change the different detectability of the seismic signals.

p. 8 L 25: The glacier dynamics “do” differ, not just “can” differ.

p. 8 L 27-28: What is meant here? How do these glaciers “interact”? How do these interactions generate seismic signals? What is the proposed mechanism?

p. 8 L 30: “Luckman” instead of “Lackman”

p. 9 L 1-3: Please provide more context here with the Luckman and Kohler results. Are the authors implying that ocean temperatures might be promoting calving during the fall? What other evidence can be provided to strengthen this case? Are the results here different than the Luckman and Kohler results in some way?

p. 9 L 6: “Tremor” in seismology is a very specific type of seismic signal, see literature on volcanic tremor or tectonic tremor (and slow slip earthquakes). The authors should use a different word, such as “seismic signals.”

p. 9 L 8: What is the “true” duration time? “True” according to what analysis?

p. 9 L 15: What is meant by “noisy” signals? “Noisy” in what way? It doesn’t appear to

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me that the fuzzy logic method provided much value.

p. 9 L 18-19: I recommend removing this sentence, but if the authors choose to retain it, please provide more information about the benchmarking experiments. What kind of computer was used to run this approach?

p. 11 L 28: typo in “micro”

Figure 3: As presented, this figure is unsuccessful in adding value to the manuscript. What is an “exemplary input parameter value?” What are the x and y axes in each panel? I don’t understand what is being shown here.

Figure 5: The basis for affiliating the “not identified” events with the glacier needs more support in the text.

Figures 6: panel a: Is there an outage in the fall of 2009? This should be indicated if so. The units in black on panels b/c are unclear. It looks as though there is a complicated division taking place. Are the “mm/cm<sup>2</sup>” one unit? Units of precipitation should be mm or m. The “per area” is meaningless. Roman numeral months in the caption should be replaced by the month names.

Figure 7: same problems as Figure 6.

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