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The Cryosphere Discussions

Interactive comment on "Seasonal variations of glacier dynamics at Kronebreen, Svalbard revealed by calving related seismicity" *by* A. Köhler et al.

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We thank Fabian Walter for his very helpful review. In the following we will comment on all points separately.

RC: ... better integrate their work into existing literature of calving seismology: (1) Based on typical calving seismograms the authors should better describe their detection algorithm. (2) It would be helpful to discuss to what extent the SOM's are an improvement over more simple waveform discrimination.

AC: We added a more detailed discussion and comparison of our results with existing literature, which we agree was missing before. We improved the description of

our method and discuss the benefit of SOMs. It is mainly the ability to deal with multidimensional data, which may be needed if a single feature (like frequency content) cannot discriminate between classes. The use of SOMs or (unsupervised) clustering in general is also a more objective way to define class borders.

RC: (1) I think a central issue is the frequency content of calving seismograms: At several instances the authors seem to imply that their geophone lacks sensitivity at lower frequencies (e. g. Lines 19-20 and Lines 24-26). It should be clearly stated at the beginning (Section 2.1) of the paper, what frequencies (i. e. ïňĆat response) were recorded? Later, it should be discussed, if the record includes frequencies, which are typical for calving events (e. g. O'Neel et al., 2007; O'Neel and Pfeffer, 2007; Amundson et al., 2008; Walter et al., 2010). This is especially necessary when describing the 'temporal characteristics': Why was a spectral ratio of the 12-19 Hz and 0.5-25 Hz bands chosen? After all, the authors stated that their sensor had little sensitivity at 'lower' frequencies? Similarly, I suggest motivating the other choices for 'temporal characteristics'.

AC: The frequency content is indeed an important feature for discrimination between different glacier-related seismic events. We revised our paper addressing this issue in more detail including the comparison of our results with existing knowledge about seismic calving events. This is mainly done in Section 5.2. Since we work with geophone data having a natural frequency of 10 Hz (information was added), we cannot resolve the frequency band completely that has been shown to be important to identify typical calving events (e.g. 1-3 Hz events). However, we found indications that we observe similar signals as previous studies. We added motivations for the choice of all features.

RC: (2) A large catalog of seismic events can also be divided into classes by simply requiring limits on frequency content, signal length, single-to-noise ratio, etc. The bimodal distributions shown in Figure 2 of the present manuscript or found by West et al., (2010) suggest such a simple sorting procedure. Can the authors state how efïňĄcient this would be compared to their SOM analysis? Also, if I understand correctly, Figure

3d suggests that most Class 1 events could be found by simply constraining the signal length. If so, this should be commented.

AC: Cluster definition benefits from simple bimodal distribution for individual features. In this case, the clusters borders will be very similar to thresholds one would choose for individual features manually. However, if the distribution becomes more complex, a cluster algorithm will find a more statistically reliable and objective cluster border than by manual definition using a single feature.

The bimodal distribution in case of our features is mainly due to two populations which are false detections and glacier-related seismic signals (Fig 2). However, we found that for Classes 1,2, and 3, it is more difficult to define a class border manually based on the distribution only. We also state in the revised version that a simple requirement of one or more features (such as frequency content and length) can still be a very effective detection method if well-discriminated classes exist. The intention of our study is to present the idea of unsupervised analysis as a tool to identify signals classes automatically which is particularly useful if no pre-knowledge about signals exists at a new site.

Figure 3d (now 3f) shows that Class 1 is mostly defined by long signal lengths. However, it also shows that other adjacent clusters are characterized by long durations too, which are later identified as false detections (cluster 23-25). More than one feature is therefore required to recognize Class 1 events (i.e. spectral content and Signals to Noise ratio in addition). Again, with the knowledge gained by unsupervised clustering, one could implement a simple classifier using three thresholds for SNR, spectral ratio and signal length. However, to define those threshold and to identity the features which are needed to define Class 1, we think SOM visualization and clustering is a very efficient tool.

RC: Finally, the low matching rate between visual and seismic observations is surprising. I would expect the largest events in Zone 1 to show up in the seismic record

somehow. Did the authors also check their seismic record visually? Perhaps this has something to do with the STA/LTA triggering algorithm, and a frequency-based detection (O'Neel et al., 2007) could do better.

AC: We cannot use frequency domain detection due to the limited bandwidth of the instrument (see above). Furthermore, the STA/LTA trigger setting was tested visually to ensure that we do not miss clear seismic events. Our hypothesis to explain the low matching rate between Class 1 and visually observed calving events, which we added in the revised manuscript, is that due to the limited bandwidth of the geophone we may miss many of the typical calving-related seismic events which have their main frequency content below 10 Hz as shown by previous studies.

RC: SPECIFIC COMMENTS Introduction: The reference on Line 2 seems dated. Is there a more recent estimate?

AC: The sentence and reference is adjusted.

RC: Furthermore, I suggest moving the discussion of glacial earthquakes further up (around Line 11).

AC: We have changed the order of second and third paragraph in this section.

RC: Section 2.1, Line 17: 'localization' → 'location'

AC: Done.

RC: Section 2.2 (page 3296): Did you also hear 'crevasse opening' (brief cracks)?

AC: Yes, acoustics signals could be heard. The catalog of direct observations is based on viewing as well as hearing.

RC: Discussion of characteristics (pages 3300 and 3301): Is there a dependence between the characteristics and if so, does it matter? Specifically, I would expect a longer time series to have a higher number of runs. Similarly, it seems that a higher-frequency seismogram would have a higher number of runs.

AC: Different features may have redundant information. The advantage of SOMs is that the component plane plots (Figure 3) visualize those dependencies. If component plane plots look very similar, the corresponding features are strongly correlated and contribute no new information to the clustering process. In our choice of features, we try to avoid correlated features to keep number of feature low. However, in general it does not matter (besides computational cost) for SOM generation and clustering to use redundant features.

We normalize the number of runs with the signals duration which makes this feature a “rate” of runs and thus independent of the duration feature. However, we agree that frequency content and runs may share some redundant information.

RC: Section 4: It would help to deïňĄne ‘training’ at the beginning of the section. At the end of the ïňĄrst paragraph (near ‘binomial distribution’) I would brieïňĆy mention the work by West et al., (2010).

AC: We mention briefly what is meant with training and also refer to West et al (2010) in the revised version.

RC: Section 5, Line 18: It would help to brieïňĆy explain the binomial test.

AC: We have added an brief explanation in the beginning of Section 5.

RC: Lines 20-25: Is visibility a factor?

AC: Of course there are always events at the front which are missed due to weather conditions (wind, fog). However, we think that our catalog of direct observations is fairly complete. An indication is that there are more direct observations than seismic detections.

RC: Lines 26-27: This seems contradictory to the ïňĄndings of O’Neel et al. (2007, paragraph 51).

AC: The type of avalanche observed at Kronebreen is probably not strong enough to

generate a seismic signal recorded at the geophone.

RC: What is the motivation for the choice of the percentages of the individual classes?

AC: The percentage of matches in each class is supposed to represent how many seismic events correspond to a direct observation. It is a sort of probability that the particular class is related to calving. The two threshold (5%, 30%) for Class 1 and 2 are empirical values based on the result of the clustering and comparison with direct observations. Clusters have been grouped into one classes since they share similar recognition rates and signal characteristics. In other words, we did not define the percentage thresholds first and then defined the classes, but the manual grouping of clusters motivated the choice of threshold. They are not supposed to be hard and statistically reliable class borders.

RC: Section 5.3: How was the noise level calculated?

AC: We compute the RMS of seismic amplitudes in a time window beginning 20 seconds before signal onset to avoid contribution of seismic events on noise level. Information has been added.

RC: Section 5.4: Line 21: ‘clearly’ seems too strong considering the low matching percentage.

AC: Removed.

RC: Section 5.5: First paragraph: I suggest including one or two references on how ocean conditions seem to influence calving in Greenland (e. g. Holland et al., 2008; Amundson et al., 2010; Murray et al., 2010).

AC: References have been added.

RC: How were calving front positions changes detected and quantiïňĄed?

AC: We added more details in Section 2.4: “...Weekly pictures are used to track the front position. The position of the camera does not allow for accurate, absolute measurements of front position changes. However, simple image to image comparison using stable reference points provides relative frontal change as estimated by cumulating binaries (+1 for advance, -1 for retreat). This results in a non-scaled timeseries of retreat/advance relevant for analyzing our seismic-calving proxy. ..."

RC: What is meant by 'starts to plateau while continuing to retreat' and 'visually translated'. The last sentence on Page 3308 is not clear to me.

AC: We clarified these sentences.

RC: Section 6: Lines 22-25: 'peaks in velocity corresponding to small peaks in calving related seismicity': this does not seem to be always true (Figure 5).

AC: We changed to: "... some peaks in velocity corresponding to small peaks in calving-related seismicity"

RC: Line 28: 'velocity is constant and at its lowest values for the year': this seems ONLY true in 2010.

AC: We changes to: "...velocity is constant and low compared to the summer months."

RC: What could be the reason for a difference to 2009? Page 3310, Line 4: what is meant by 'calving activity itself'?

AC: Site conditions are slightly different (coupling, position) which could introduce a bias. However, noise level seems to be similar on average in 2009 and 2010. Furthermore, decreased seismicity in 2010 seems to be consistent with less visual observations at the front for that year. The reason for decreased activity in 2010 remains however unclear and more data is needed to discuss it. "Calving itself" means that the state of the glacier front is also controlled by previous calving events which increase or decrease the probability for future calving. We clarified this sentence.

RC: Figure 3: This figure is too small.

AC: Figure is too small due to the scaling introduced by the production office to fit both

the caption and figure on the same page for the discussion paper. Figure will probably appear larger in the final layout with normal page sizes.

RC: Figure 5: This figure is too small.

AC: See above.

RC: Are the two '0' markings on the vertical axis of the upper panels the same in 2009 and 2010?

AC: No, the initial front position for 2009 is different to 2010. It is the position obtained from the first photo in spring in each year. Information was added in figure caption.

RC: The vertical axes of the upper panels need units.

AC: It is a relative front position given in binary units as described above and in Section 2.4. It is a qualitative description of front position changes.

RC: What is the noise peak in 2010?

AC: Noise peaks could be due to instrument problems or large rain events.

RC: In the caption, the different panels should be described separately.

AC: Done.

RC: Throughout the paper I think it is important to better distinguish between calving and non-calving glacier-related seismograms.

AC: We tried to emphasize more the fact that our Class 1 events are probably related to calving and Class 2 represents an other type glacier-seismic event, not necessary a direct signal of a calving event – maybe fracturing.

Interactive comment on The Cryosphere Discuss., 5, 3291, 2011.