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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.

F. Walter (Referee)

walter@vaw.baug.ethz.ch

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This work by Köhler et al. describes the application of Self-Organizing Maps (SOM's) to a seismic record of iceberg calving in Svalbard. This is an interesting and timely study, because sorting through large continuous seismic data sets from glacial regions can be substantially enhanced with such an unsupervised technique. Furthermore, the authors demonstrate how a one-station record can be efficiently mined with SOM's, which is a significant improvement over simple event counting.

Like the other reviewers I mainly suggest that the authors 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.

(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. flat 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'.

(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 efficient 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.

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.

SPECIFIC COMMENTS

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

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

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

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

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.

Section 4: It would help to define 'training' at the beginning of the section. At the end of the first paragraph (near 'binomial distribution') I would briefly mention the work by West et al., (2010).

Section 5, Line 18: It would help to briefly explain the binomial test. Lines 20-25: Is visibility a factor? Lines 26-27: This seems contradictory to the findings of O'Neel et al. (2007, paragraph 51). Page 3303: What is the motivation for the choice of the percentages of the individual classes?

Section 5.3: How was the noise level calculated?

Section 5.4: Line 21: 'clearly' seems too strong considering the low matching percentage.

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). How were calving front positions changes detected and quantified? Page 3308: 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.

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). Line 28: 'velocity

is constant and at its lowest values for the year': this seems ONLY true in 2010. What could be the reason for a difference to 2009? Page 3310, Line 4: what is meant by 'calving activity itself'?

Figure 3: This figure is too small. b) It may be helpful to use 4 colors, only.

Figure 5: This figure is too small. Are the two '0' markings on the vertical axis of the upper panels the same in 2009 and 2010? The vertical axes of the upper panels need units. What is the noise peak in 2010? In the caption, the different panels should be described separately.

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

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