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Interactive comment on "Wind-induced seismic noise at the Princess Elisabeth Antarctica Station" *by* Baptiste Frankinet et al.

Anonymous Referee #1

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In their study, Frankinet et al. investigate the impact of wind on the seismic noise levels measured at the Princess Elisabeth Station in Antarctica. They find that the seismic power increases with the wind speed and they determine empirical linear scaling laws between the two quantities that seem to be different for low wind speeds (<6m/s) and high (>6m/s) wind speeds. With these scaling laws, they develop a noise model as a function of frequency and wind speed. This model suggests, that icequake detection rates from a previous study at Princess Elisabeth station are biased by variable wind noise levels masking events at high wind speeds. The authors use the noise model constructed from a single station to correct its spectrograms and those from five other stations in the same region for wind noise, and discuss the resulting RMS amplitudes in the light of cryoseismic activity.

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Because icequakes and other cryoseismic signals are often characterized by low amplitudes, detection limits and temporal variations thereof are crucial for the conclusions drawn. In this regard, the study by Frankinet et al presents a valuable contribution as it quantifies the changing noise level due to wind potentially masking events of interest. The derived wind-noise model appears reasonable and it may help future studies to evaluate the influence of wind on seismic measurements, even though its wider applicability remains unclear, as the model is derived from a single seismic station. In some parts, the manuscript needs to be substantially improved in terms of explaining/clarifying the analysis and discussing the results as the calculation of the wind-noise model and the robustness of the results remain unclear. I feel that seismic power and seismic activity (in terms of events) is mixed up in the discussion and I think that the contribution of the wind-corrected spectrograms is very limited as they hardly give insights into the glaciological processes. I therefore have several major comments and a range of smaller issues, which should be addressed before consideration for publication.

MAJOR COMMENTS

L56-58: I think that this sentence does not correctly summarize the cited studies. Seismic observations help to constrain subglacial properties, but it is to date not possible to model/link seismicity with ice flow modeling. Also, the study of Nanni et al. investigates an Alpine glacier, not Antarctica. Please correct this.

L143 and the following: In this part of the manuscript, the basis of the wind-noise model is formulated, i.e. that seismic power scales with wind velocity at two different relations for find velocity greater and smaller than 6 m/s. However, this needs to be better supported with data, as this is only shown for a single frequency bin. I suggest to plot the 5th percentile (or median) measurements (red triangles in Fig. 3b) for for the whole frequency band (color-coded). This should give further evidence of the wind-induced noise as a function of frequency. By looking at Fig. 3B, one could also conclude, that seismic power is just dependent on wind for velocities greater 5m/s. Also, by looking at Fig. 2, there seems to be only a small increase in seismic power for wind speeds greater than approximately 20m/s.

L158-160: I think this part needs to introduce the model formula, which is used to calculate the output shown in Fig. 4a. First, the formulas for the linear regressions should be connected to the measured quantities (y=Amplitude [dB], x=wind speed [m/s]). Then, the parameters determined from the regression are used to create the model, which must be something like a1(f)*x (for x<6m/s) and a1(f)*6m/s + a2(f)*x + b2(f) (for x>6m/s), I guess. These are crucial details, which need to be added to the manuscript.

L188 and following: To further stress the point that increased wind speeds result in reduced event detections, I suggest to look at the recorded data and plot the event detections as a function of wind speed. This should show a drop in detections at higher wind speeds, in case the wind doesn't affect the icequake generation processes. Also, the red bars in Fig. 5 may actually be replaced (or compared) by the measured RMS of ELIS, which I assume is not much affected by the few short duration events per 6 hours.

L199 and following: Here, the wind speed measured at the base station is used to calculate wind-induced noise power, which is subtracted also from the five stations of the temporary network. The temporal variability of the wind-corrected PSDs are then discussed. I think that such an analysis is not well justified, as wind speeds might not be well correlated at the sites (as also indicated in the manuscript). For instance, station ANT6 is separated by about 50 km from the weather station and on the other side of a 4000 m high mountain range, which I expect to clearly influence wind conditions. In addition, the authors find that wind-corrected PSDs are still correlated with wind. This is not surprising considering that the wind-noise model is calculated from the 5th percentile of PSD observations, hence removing not the full contribution. These shortcomings must be discussed in the manuscript. Currently, this is only briefly picked up in the conclusions.

L225 and following: This section discusses the temporal PSD/RMS variations of the wind-corrected stations, but I think that this section does not have a very profound basis given the issues raised in the previous point. Also, I doubt the usefulness of analyzing RMS amplitudes in the context of discrete icequake events. The events presented in Lombardi et al. 2019 are of short duration (<1s) and weak amplitude (<1e-6m/s), hence, I do not expect them to cause a significant contribution to the RMS amplitude. Yet, this could be checked by running a simple STA/LTA trigger on e.g. station ANT6 during high RMS amplitude periods, which is argued to register more seismicity due to its deployment on blue ice. Overall, I feel that the discussion of RMS amplitudes, does not yield useful insights into glaciologically relevant processes. Given the uncertainties and potential overinterpretation, I suggest to significantly shorten the discussion of RMS variations in the light of ice flow dynamics. Instead, I suggest to discuss some other aspects as detailed in the following comment.

Wind-induced noise levels: I am missing a discussion of the wind-noise levels and comparison to other studies. For instance, how do the results compare to the findings of the cited study by Lott et al. (2017), who also analyze wind-induced noise as a function of wind speed? In this context, it would also be helpful to discuss the wider applicability of the derived noise level. Is it also applicable to other sites in Antarctica? If available, it would be also very interesting to study other colocated seismic and weather stations.

ELIB vs ELIS: Sometimes, the manuscript refers to station ELIS, sometims to ELIB. According to Table 1, these are two different stations, with ELIB being a borehole station, yet, the existence of such a station is not mentioned in the text. This issue needs to be clarified. Actually, it would be very interesting to see Figure 2 for both surface and borehole station to evaluate the effect of a shallow borehole (according to Table 1, ELIB sits in a depth of roughly 10m?) on the wind-noise level.

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MINOR COMMENTS

L23: in-glacier > englacial

L33: Whillans ice shelf > Whillans ice stream

L33: Reference formatting wrong, remove first name of author.

L37: Maybe use PEAS for the Princess Elisabeth Antarctica Station, as referring to PE, i.e. Princess Elisabeth, in the text is a bit misleading ;-)

L38: Start the sentence with "The PE/PEAS allowed investigations in the field of ..." and maybe give a bit more context on the meteorites as it the connection between Antarctica and meteorites is not obvious.

L46: Rewrite ". . . improves the sparse coverage of seismic stations ...".

L51: Move the reference to the end of the sentence and consider deleting "hitherto unknown" as it is expected to encounter microseismicity in glacierized terrain.

L54-56: Be a bit more specific here and distinguish between elastic deformation and static or plastic deformation.

L63-64: Is there "activity in the buildings" also in winter, when no humans are (apparently) around? Please specify.

L66: The link to tectonic earthquakes is misleading here, please delete.

L86: originates > originate

L90: aggregating > sorting (also in the following of the manuscript)

L99: Before it was mentioned, that the station was installed in 2010?!

L102: What does partly continuous data mean? Please sepcify.

L103: samples/s > Hz

L104-105: worldwide large earthquakes > teleseismic earthquakes

L108: “and” is missing, change to "... and is provided ..."

L108: Provide reference to the AEROCLOUD project.

L113: Remove “and” before “averaged”.

L115: power > seismic power

L116: What do you mean by baseline noise model? Maybe just mention that the Probabilistic PSD represents a statistical distribution of the PSDs.

L118-119: You correct the ground motion time series for the instrument response and then calculate the PSDs, right? At least that’s what implemented in obspy, I think.

L120: Be more specific (it is the new high/low noise level) and correct the reference formatting. In general, I think this whole paragraph (L114-L121) could be written more succinctly.

L122: onto > on

L122-124: I think these lines can be removed, as the next section already starts with the same information. Also the second sentence is a bit out of context and I guess it should be rather “removing” or something similar instead of “suppressing” the wind-induced noise from the observed data.

L137: “... for every 0.25 m/s wind speed ...” > for every 0.25 m/s wide bin of wind speed

L138: What do you mean by “maximum average”?

L138: What happens at wind velocities greater than 25m/s? The presented analysis suggests, that at least 10 observations at 25m/s are available, so I would guess that also observations for even higher velocities are available.

L145: As the 42dB refer to acceleration PSD, it should be a 100-fold increase in ground

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acceleration not velocity, right?

L160: Please provide a more information here, how you convert the PSD to the RMS. For instance, how do you integrate the acceleration PSD?

L164: Are the given amplitudes of the Lombardi study absolute amplitudes or also RMS amplitudes? For comparison with the wind-noise amplitude in RMS this is important, please specify. Also, the unit is wrong: 0.3micrometer > 0.3 micrometer/s.

L193: “icequakes activity rate” > icequake rate

L195: “. . . 472 events were manually detected by Thierry Camelbeeck.” Does this refer to another study?

L196-198: The meaning here is clear, but the sentence needs to be rewritten. The last sentence (“We can therefore observe ...”) can be deleted, as this is a general statement not specific to this study.

L199: From Fig. 1 it actually looks like all stations are installed in slow-moving (<40m/year) areas?

L255: Delete “a”. Change “. . . surpasses the detectability ...” to “. . . prevents the detection ...”.

L256: Delete “most”

L258: I think there is again a problem with the units (ground velocity vs acceleration vs RMS amplitude), at least the 100-fold increase in ground velocity does not fit the “0.5micrometers/s to 3micrometers/s” statement.

L261: Delete “future”.

GENERAL COMMENTS AND FIGURES

The introduction contains a lot of detail on the study site already, which could be moved

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to section 2.

I suggest to use the term “ground velocity” instead of “seismic velocity”, as the latter typically refers to material properties, i.e. seismic velocity in rock. Also, I suggest to use “thermally-induced icequakes” instead of “thermal icequakes”.

The references contain some flaws and should be carefully checked.

Fonts of Fig. 3 and Fig. 6 are too small.

Table 1 and 2 may be merged and the coordinates given in Table 2 adjusted to the format of those in Table 1.

Fig. 2, caption: explanation of channel names is not relevant and can be removed.

Fig. 3: From a conceptual point of view, it makes more sense to switch a) and b). The caption should be rewritten to make it better understandable. Also, 3a does not yet show the noise model, right?

Fig. 4a: “(Lombardi, 2019)” → “Lombardi et al., 2019”. Caption, first line: blue > white. Maybe also split in two sentences and reformulate.

Fig. 5, red y-label: change to “Modeled RMS” for clarity.

Fig. 6: labels a) – n) are missing. Also, provide more detail in the caption, e.g. explain blue arrows.

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