



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

**Baptiste Frankinet et al.**

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Dear Reviewer 1,

Sorry for the time taken for doing this review. Thank you very much for your review, which helped us improve the manuscript. Please find answers to your questions here below.

Baptiste Frankinet, for the authors. — 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,

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not Antarctica. Please correct this.## Thank you for this remark, indeed, the study of "Nanni et al" is not directly correlated and we removed it, and corrected the sentence: "Cryoseismic catalogues and seismic observations can be correlated with numerical models of eastern Antarctic ice dynamics to constrain subglacial properties of a specific area." ## 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 wind 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 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.## Thank you for this comment. Our model parameters ( $a_1$ ,  $a_2$ ,  $b_1$  &  $b_2$ ) are computed for all frequency bins and for each 0.5 m/s step. They are represented on Figure 3a, which shows a strong frequency-dependent relation. In turn, this means that computing the simulated RMS values is important when comparing with seismic rate. On the attached Figure R1, we plot an example with showing other frequencies in the example of Figure 3b, but we fear this makes the graph much less readable, and redundant with Figure 3a.

## 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  $a_1(f)*x$  (for  $x < 6$  m/s) and  $a_1(f)*6$  m/s +  $a_2(f)*x + b_2(f)$  (for  $x > 6$  m/s), I guess. These are crucial details, which need to be added to the manuscript.## We have added a connection from the formulas for the linear regressions to measured quantities and then added the equation 1, for the model formula clarity. We believe now the link between the model parameters determination (Figure

3a) and the modelled spectrogram (Figure 4) is more clear. ## 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.## As explained above, the wind speed has different effects on different frequencies. It is therefore important to use the modelled RMS values to compare with the seismic rate. On attached Figure R2, we plot the number of events per 6 h vs the average wind speed per 6h, adding them all together on one plot makes the figure less readable. We also plot (Figure R3) your suggestion of the ELIS RMS vs Wind speed, but we believe the modelled RMS is more appropriate in the manuscript as it demonstrates the usefulness of computing it. The results of Lombardi et al are showing large diurnal RMS variations, that they link with local icequake activity. This activity therefore is largely responsible for the observed RMS, even though each individual icequakes are short duration.

## 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.## Thank you for this comment. Indeed, the size and context of the ANT array suggest that the wind field might be different overall. We rewrote

this section and replaced Figure 6 by the non wind-corrected data. We adapted the discussion about potential wind-induced, or icequakes-induced noise sources. The usage of the 5th percentile has the objective to define the baseline of the changes, and in the modelling (no longer used now), we used only the a1 and a2 parameters, so the “changes”, and not the baselines (b1 and b2).

## 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.## The results of Lombardi et al are showing large diurnal RMS variations, that they link with local icequake activity. This activity therefore is largely responsible for the observed RMS, even though each individual icequakes are short duration. We believe our observation of the different patterns of icequakes activity deduced from the RMS amplitudes is relevant to the study of the icequakes in the area. If the icequakes would be very shallow events due to the thermal expansion of the ice, then indeed those are probably not relevant for, e.g., basal processes, but they are still witnesses of the state of the ice. ## 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.## Thank you for this comment. We now included a comparison of our wind-noise results to the Lott et al., (2017) wind-noise study. The comparison with other stations/bases would be interesting in future studies. We believe our methodology is simple and can be easily reproduced elsewhere. We also now added the importance of the fact that wind doesn't seem to significantly affect the frequencies between 0.1 to 1.0 Hz, often used for ambient seismic noise-based imaging and monitoring. ## ELIB vs ELIS: Sometimes, the manuscript refers to station ELIS, sometimes 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.## Yes, in fact we made mistakes using ELIB instead of ELIS. At PEAS, there were 2 stations : ELIB (borehole) was the first installed in February 2010 followed by ELIS (surface) in February 2012, due to technical difficulties ELIB use was discontinued, therefore the data used in our study was from ELIS and not ELIB. I corrected my mistakes and replaced ELIB by ELIS where it was wrong. Also, it would be very interesting to see the difference between the borehole and the surface seismometer, it could be done but for Figure 2, we used a complete year of seismic records (2017) that was recorded by ELIS but ELIB.

## Corrections to the Minor comments:## L37: I corrected accordingly and replaced PE by PEAS in the whole manuscript (Princess Elisabeth Antarctica Station) to avoid confusion. L38: I rephrased and added an explanation of why Antarctica is extensively used in meteorite finding: "The PEAS allowed investigation in the field of meteorites as spotting them on the emptiness of Antarctica is simpler than mixed up with vegetation and rocks". L46: This station increases the sparse coverage of seismic stations in the Sør Rondane mountain range in Antarctica L64-65: At PEAS, a few anthropogenic noise sources exist year-round like wind turbines, and seasonal human activities out-

side and inside the buildings during the summer L99: Yes, I failed to mention that the first instrument installed in 2010 was installed in a 13 m borehole and due to failing of the instrument and the high-maintenance cost, it was then replaced in February 2012 by a broadband seismometer at the surface. L102: It is continuous, I wrote partly because the data was continuous for 2019 and 2020 but had yet to be downloaded from the station. L118-119: Yes, you are right, I use Obspy and made a mistake describing it. I corrected it. L138: I didn't write it correctly. By maximum average, I mean that the Weather Station records and the maximum wind speed recorded every 10 minutes which is averaged over an hour. I rewrote it to "the average of the maximum" L138: 25 m/s represents a threshold whereas over the period studied, the wind-speed didn't exceed it. L145: Yes, I made a mistake by saying ground velocity, I corrected it to ground acceleration. L160: Explain the Perseval's theorem? L164: It is also the RMS amplitude used in Lombardi et al., 2019 L196-198: The new sentence is as follows : "If these events were equally distributed over the time-period,  $\sim 7.9$  events could be detected each day but, the numbers of events found over this period were not, as for example, during the 3rd of February 5 events were detected whereas, on the 4th of February, not a single event was recorded dur to a drastic increase of the RMS." L199: Yes, in fact, I meant that some of the temporary stations, i.e.: ANT3 observes greater icequakes due to its location near higher ice-flow speed channels. I shortened the sentence to avoid confusion. L258: Yes, I made a mistake with the units explained. I corrected it.

## Answers to the General comments & Figure comments:## I modified in the whole manuscript "seismic velocity" to "ground velocity" and "thermal icequakes" by "thermally-induced icequakes".

Table 1 and 2: I merged Table 1 and 2. Figure 4: corrected the citation on the figure and modified the caption accordingly. Figure 3: I modified and expanded the Figure 3 to make it more readable. Figure 5: I changed the red y-axis label from "RMS" to "Modeled RMS". Figure 6: I added labels (a) => (n) and changed the caption to better

explain the graph.

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2020-267>, 2020.

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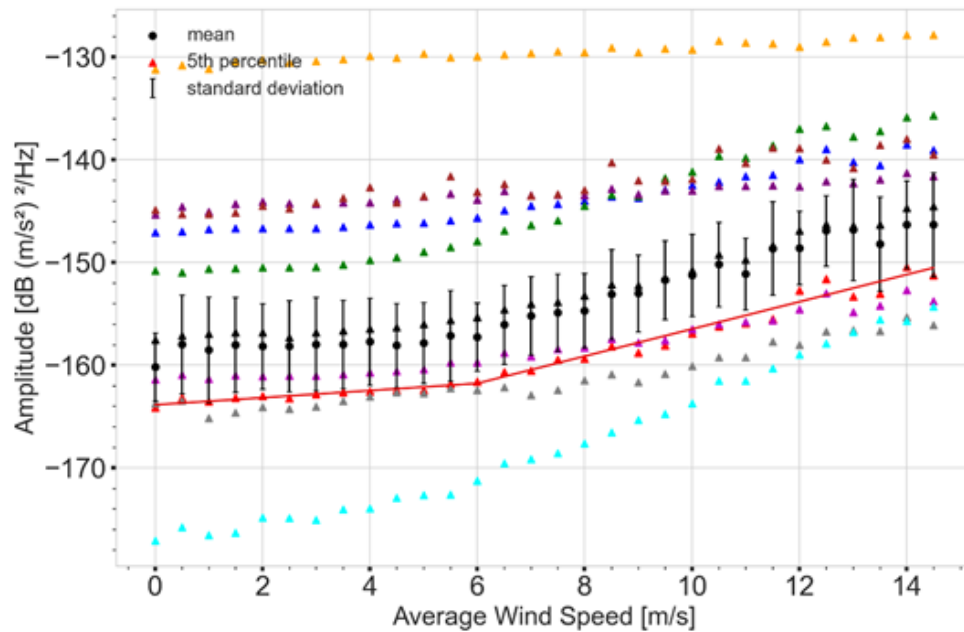


Fig. 1. Figure\_R1



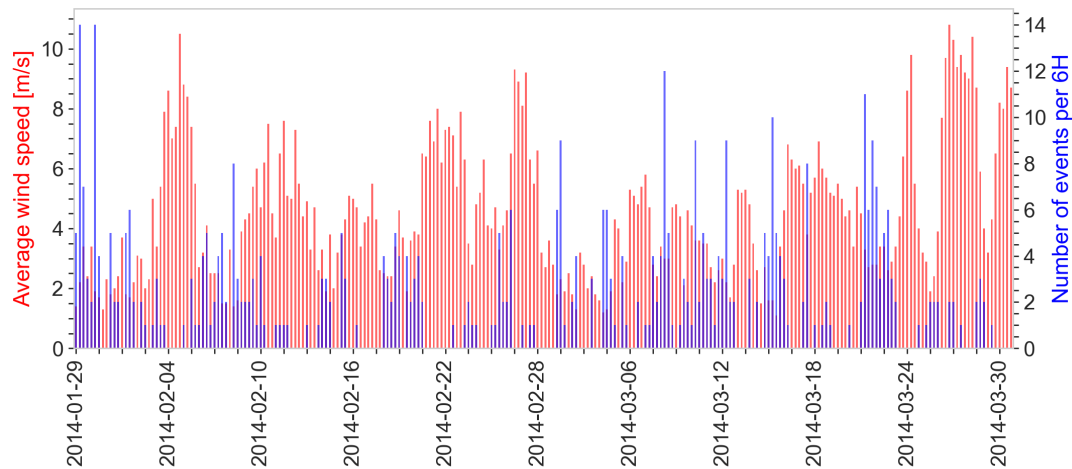
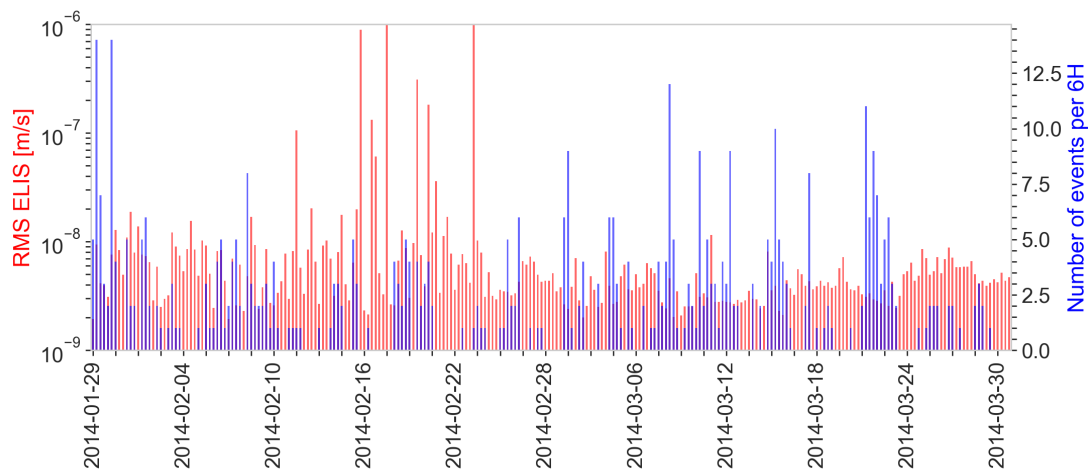


Fig. 2. Figure\_R2



**Fig. 3.** Figure\_R3