We would like to thank the reviewers and the editor for their carefully reading and the very constructive criticism. We respond to all comments below.

<u>RC2:</u>

In my opinion, the manuscript offers an interesting evaluation of such a sand layer structure as a temporally high resolution proxy for wind. With a correlation of up to 0.63, the barcode structure of the sand dunes provides a valuable wind proxy. While the novelty of using such a barcode structure as wind proxy and reasonable correlations are promising, the paper requires some significant re-work wrt. to the clarity of text, the paper's structure and the robustness of statistical evaluation. I therefore think the manuscript may be published after major revisions.

Major comments:

<u>Text:</u> The structure of the manuscript, the motivation for this study and the scientific context should be improved (see below).

<u>Statistics:</u> The authors present an interesting evaluation but without a proper validation. Although the sample size is relatively small, significance testing (p-values) would be important to provide at least some estimate about the robustness of the results/methods. The author's attempt to test the link between temperature or precipitation and sand mobilization/transport and hence a potential disturbance of the wind signal by other factors is, not fully convincing. The causal link remains unclear here whether temperature and precipitation simply co-vary with wind or really affect sand mobilization. This is a serious problem for most other paleo-wind proxies as well as e.g. dry periods may be confused with stormy periods or wet periods simply co-vary with windy periods etc. This aspect should be addressed or at least discussed in more detail. The use of a LOESS regression is not convincing given the low sample size and unclear relation. Correlations for the whole wind season should be added as the barcode has an annual resolution.

Specific comments:

<u>Abstract:</u>
One or two leading sentences, why it is important to study the wind climate in the past and why it is important to use proxies even for shorter timescales, would make the paper more appealing to read. The abstract should be improved by focusing on main aspects of your work only, without too much detail.

> We rephrased and shortened the abstract and hope it now reads more clearly and focused.

<u>Introduction:</u>
Generally, the structure here is a bit chaotic. I would recommend to re-write it following a clear structure like why it is important to study the wind climate, what has been done already using which data or methods (the references on page 2-3), what are the conclusions and open questions from these studies so far (see e.g. Rutgersson et al. 2015; Feser et al. 2015; Christensen et al. 2015 for reviews), the problems of the data/methods and need for using different proxies, how Aeolian transport can be used here followed by here we present...or similar.

We will re-structure the introduction according to the suggestion of the reviewer.

Page 2, line 1-2: References needed, many studies investigated it already (e.g. Christensen et al. 2015 for references)

We will add a reference.

Page2, line 4: References needed, this has been done already (e.g. Rutgersson et al. 2015; Feser et al. 2015)

We will add a reference.

Page 2, line 23-24: Not only the location changes, a reference could be e.g. Lindenberg et al. 2012

We will add a reference.

Page 2, line 29 to Page 3, line 9: This is all quite technical for an introduction. What do these studies tell us about past wind climates? What are their main conclusions so far? What are open questions a dune proxy may help to answer?

We hope to have addressed these questions in the re-written version.

Page 3, line 2-9: A detailed explanation for this data product is not really needed here. A short note might be more interesting here like e.g. that even the use of long-term reanalysis data like 20CR has been shown to be problematic regarding long-term trends (Krueger et al. 2013 etc.) and dune records may therefore help to get more consistent results.

Since this is a quite interdisciplinary work we think it is useful to explain reanalysis data, even as early as in the introduction. The readers not used to handle model data may miss the difference between free-running climate simulations and meteorological reanalysis, and the links between the latter and observations. Hence, we would prefer to keep this explanation.
However, we added the mentioned short note regarding 20CR.

Data and area
Acknowledging the geological context of a site-specific analysis, I would suggest a classical structure 2.1 Area/Leba Dunes, 2.2 Climatological characteristics, 2.3 Meteo data, 2.4 Dunes

We changed the order accordingly.

2.1. Meteorological data
I think it is enough to write "we use a numerically downscaled reanalysis dataset … " with two or three sentences which reanalysis and regional model, spectral nudging and resolution used in coastdat2. For details you can refer to Geyer 2014. More relevant for this study are the properties and validation of simulated wind (Weidemann 2014).

We shortened the explanation about coastDat2.

Page 3: line 29: "usually kept to a minimum". This is not correct, all available data is used for each time step. Reanalysis hence gives the best possible estimate for each time step which may lead to artifacts if the type, quality or number of observations changes over time. Rather a frozen data assimilation scheme is used to minimize these effects. For your region and time period, all the issues are not really relevant after ~1980.

> This sentence was deleted to shorten the description of coastDat2 addressing your preceding comment.

Page 4: line 12: The imperfect NCEP forcing and its coarse resolution is another relevant source of error here

> We added: "Other errors may occur due to the imperfect forcing data set NCEP and its coarse resolution."

Page 4, line 33: Could you give the typical size here (dominant sand fraction, μ m or a range)?

> We added: "The sands are fine-grained (with a diameter of 0.2 to 0.3 mm (Ludwig, 2017) and well-sorted..."

Page 5, line 16: What about wind? Reference here to Ludwig et al. for more detailed seasonal information (Fig. 5 in that paper)

> We do not want to rely solely on the weather information given by Ludwig et al. (2017) as they based their conclusions on only one station, where wind from some directions is potentially perturbed by the nearby forest.
> Therefore we added: "Regarding wind direction, the Baltic Sea area shows a predominance of westerly and southwesterly winds for all seasons with a second maximum for north-easterly winds during spring for mean and for extreme wind speeds (Bierstedt et al., 2015). Similar results where obtained by Ludwig et al. (2017) with observational data of one station located close to the dunes."

Page 5: line 28: Is there not any experimental / theoretical range giving a rule of thumb which wind speed can mobilize which grain size or mass? If so, you could use it to physically verify the realism of your statistically estimated thresholds.

> We added: "Ludwig et al. (2017) calculated this threshold to be 4.4 m/s for the finest, dry sands and 10 m/s for moist material."

Page 5, line 31: compiled

> We changed this accordingly.

Statistical Methods
It is convincing and very nicely shown, that one barcode interval reflects one year in Ludwig et al. However, the separation into junks of three months for wind is in the end subjective and artificial wrt. to the annual dune activity. Consider e.g. that your

seasonal correlation analysis may suffer from intra-seasonal changes in wind activity over time (e.g. Lehmann et al. 2011). I would hence suggest to add first a correlation analysis for the whole wind year (e.g. ONDJFM). You could then replace the JJA figures in the multi plot figures 4 and 5 with the full wind season. Then you can continue and show also whether the full wind season or rather a fraction of the season yields the highest correlation.

> We also re-did the analysis for the whole wind year (SONDJFM). However, the results in this case show lower correlations and less differentiated results for the black and white layer. Because one goal was to analyze seasonal differences the further analysis was kept in the 4-season separation, although the results for the whole wind year will now be discussed in the manuscript.

The same applies for the wind directions. The correlations for the wind octants in combination with the small sample size in this study may be quite sensitive to small random changes to the neighboring octant (as can be anticipated from the wind rose figures in Ludwig et al.). I think you should test in addition quadrants of 90∘(e.g. W= 225-315∘or SW=180-270∘). It is certainly interesting to make the detailed tests in this study but one aim should be to find the optimal setting with the best fit to the dune data rather than limiting it to very strict seasons and directions.

> The coarser separation into wind directions was, in a similar way, already prescribed by Ludwig et al. (2017), although they used a different wind data set (observations) we would rather like to avoid repetitions. The suggestion by the reviewer is indeed very logical, but unfortunately we face the limitation of the short record. If the wind data are further stratified according to finer direction resolution, the sample size for each direction bin will become really small, compromising the statistical analysis.

Page 7: line 2: Please explain in more detail how such a ratio or difference might look like and why, I cannot follow here.

> We will expand this explanation in the new version according to following scheme:
> Old version: ”In addition, we try to find an optimal ratio between the number of westerly and easterly winds that better describe the thickness of the black interval.”
>
> New version: “Due to the described winnowing effect of easterly winds (see Sect. 2.1.2), we additionally investigated the idea of an optimal ratio between the number of westerly and easterly winds that better describe the thickness of the black interval.”

What are the x-axis units in Fig. 8?

> Old figure caption: “Scatter plot of the difference between westerly (W, SW, NW) and easterly (E, SE, NE) winds and the black interval thickness. The red line shows…”
>
> New figure caption: “Scatter plot of the difference between the number of westerly (W, SW, NW) and easterly (E, SE, NE) winds and the black interval

thickness. The red line shows…"

Page 7, line 3: I see the point of exploring the outcome of a LOESS fit here. But the result does not look useful. Based on Fig. 8, a linear regression (digitizing your data in Fig. 8, I got y=0.00053x+3.388; r 2=34%; p=0.0012) looks more convincing although it remains unclear to me, what it means. With the low sample size, LOESS regression is very sensitive to outliers. As it does not yield any equation, the fit cannot be reproduced by others without having the original data. I would therefore suggest to stick to a linear fit, give confidence intervals and explain the outcome.

> Maybe the reason for the application of LOESS was not clear enough in the old version of the manuscript. We understand this comment but a linear regression would not serve our purposes in this case. This is related to the previous comment of the reviewer. A linear regression would only show the influence of the difference between westerly and easterly winds. We already know that westerly winds are the driving force as the dune is moving towards the east. Hence, this would not give us new information. Our goal was to see if there is an optimal relation between westerly and easterly winds which promotes the thickness of black layers, so that smaller or larger ratios would produce also a smaller thickness. For this, we need to identify a nonlinear link between this ratio and the layer thickness. Unfortunately the obtained results are not robust so that such an optimal relation cannot be confirmed. We try to explain this more clearly in the new version.

Page 8, line 3: "slight positive correlation" – Which value? Give a p-value.

> Old version: "The colder seasons winter (DJF) and spring (MAM) show slight positive correlations for both intervals,…"
>
> New version: "The colder seasons winter (DJF) and spring (MAM) show slight, albeit not significant, positive correlations for both intervals (DJF; r=0.17-0.23 and MAM; r=0.19-0.24),…"

Page 8, line 4: "indicates an increasing bar thickness during wetter periods". And how does that match with soil wetness and compactness mentioned before? I think this only tells you that more storms co-vary with more rain, but there is no causation more rain = more sand transport. This should be at least discussed if the low sample size does not allow a comparison like drier storm seasons vs. wetter storms seasons in comparison to the barcode. Maybe you could make a quick test for your period if/how wind above your chosen threshold is correlated with precipitation and temperature.

> We think this aspect is already discussed in our 'Discussion and Conclusion' section on page 11 line 24 -29. However, we added another sentence (underlined) and hope to clear this comment:
> "Regarding precipitation, the results showed positive signs for the white and black bars for winter (DJF) and spring (MAM). Borówka (1980) stated that some rain might improve the transport due to turbulence, which makes more sand grains available.
> We argue that the influence of precipitation on sand transport, and hence the dune processes, depends on the seasonal wind conditions. For example it might be possible that precipitation and wind co-vary, which is especially likely during

winter and spring when stronger cyclones come into the Baltic Sea region.
Ludwig et al. (2016) describe a secondary dune on top of the primary dune
consisting of the white and black interval. These secondary dunes seem to be
affected by precipitation due to erosion. This idea is supported by our results and
shows that in wetter seasons the secondary dunes might be eroded into the
primary dune and hence results in thicker dune intervals."

Page 8, line 5: "non-negligible" – Please give a value and p-value here. Why only the
black?

Old version: "Autumn is the only season showing some non-negligible correlation
for black intervals (0.33)."

New version: "Autumn is the only season showing a non-negligible, albeit not
significant correlation for black intervals (r=0.33; p=0.09)."

Page 9, line 17: "this season and direction" – It makes sense to use the best combi-
nation but you should reconsider whether the best combination might not be the full
wind season (e.g. SONDJFM) as mentioned before.

We calculated the results for the whole wind year (SONDJFM). However, the
whole wind year results show lower correlation values and less differentiated
results for the black and white layer. Because one goal was to analyze seasonal
differences the further analysis was kept in the 4-season separation.

As you mentioned the dunes in Lithuania, it would make sense to also provide your
regression model for re-use or reproduction of results.

We added slope and intersect values in Table 1.

Page 10, line 2: To which extent could you use the deviation of the black-white ratio
from being relatively equal (~1) to say sth. about years of more easterly or more
westerly years? I did not really get that point from the manuscript.

This part of the manuscript is about the question whether there exist an optimal
ratio between westerly and easterly winds which might promote the thickness of
the black layer. We wrote on page 10, line 4-7: "As already mentioned, westerlies
transport white and black particles together to the east, where they deposit and
build a new interval. Easterly winds on the other hand winnow only the lighter
white grains and transport them backwards to the west, hence a black interval
forms. This effect suggests that there might be an optimal difference of days with
west and east winds per year that results into a thicker black interval."
So this section is not about the black-white ratio, but on the possibility of the
ratio westerlies to easterlies being recorded on the thickness of the black layer.

Page 10, line 10-13: This fit makes little sense. Please replace LOESS with a linear
fit and give the equation, r^2 and p-value (should be very close to what I wrote above).
There is indeed no optimum (why should there?) but a linear fit is highly significant.
What could that mean?

Please, see answer above.

Page 10, line 17-20: The link between wind climate and sea-level is a bit more complicated depending on the region and timescale of wind/sea-level co-variations. The description here is too vague and some references should be given in addition. Note that most readers do not know anything about sea-level variations of the Baltic Sea.

> This section is indeed too short in the old version and we will expand it to make clearer also for readers not familiar to sea-level variability. Originally, it was intended to indirectly support the link between the dune layers and wind, in view of the dearth of wind observations in this area.

Page 10, line 29: And how does the link of the wind forcing look like= How can it be explained?

> We hope that the previous addition about the link between wind and Baltic Sea level makes this question clearer and another addition needless.

Page 11, line 15-16: Rather than being suspicious about coastdat2 here, I would highlight that the positive link to sea-level is very useful as sea-level data goes further back in time than reanalysis and might be also more reliable than spurious trends in 20CR (Krueger et al. 2013), which do not affect yet the short period in this study.

> We agree with the reviewer and added information about the longer time span of sea-level data. Nevertheless in our case it was not the intention to use these data, because the dunes only span 26 years. We wanted to make our results more robust by also applying observational data. Furthermore we think another note regarding 20CR might be confusing.

Page 11, line 25-26: Very speculative. With "some rain" it might be true but not with more rain. If the "some rain" effect would be important, you should expect to get a negative correlation in your evaluation, but it is positive. I would add that more rain might just co-vary with more windy conditions. The mentioned erosion is also a very good point here.

> Old version: "Borówka (1980) stated that some rain might improve the transport due to turbulence, which makes more sand grains available. We argue that the influence of precipitation on sand transport, and hence the dune processes, depends on the seasonal wind conditions."

> New version: "Borówka (1980) stated that some rain might improve the transport due to turbulence, which makes more sand grains available. We argue that the influence of precipitation on sand transport, and hence the dune processes, depends on the seasonal wind conditions. For example it might be possible that precipitation and wind co-vary, which is especially likely during winter and spring when stronger cyclones come into the Baltic Sea region."

Page 12, line 5: The p-values and adding an analysis of the full wind season ONDJFM might lead to an even more robust conclusion.

> For the sake of clarity we would rather prefer to mark significant (by the 0.05 significance level) results with a *. This was also a suggestion of the first

reviewer.

<u>Figures:</u>
Fig. 3+4+5: Use consistent tick marks on the y-axis. What means "- mv" in the figure titles? For all bar charts, you could consider using white, black and grey for white, black and mixed intervals. This would make it more intuitive.

We will change this accordingly.

Figure 8: I would rather use a linear fit. What are the units on both axis?

Please see answers above.

Figure 9: If possible, use bigger symbols for the gauge locations.

We enlarged the symbols.

Table 1: Why not give the regression model (slope, intersect) in addition, also p-values?

We added the slope and intersect of our regression model. Regarding p-values, please, see above.

<u>Additional references:</u>

Christensen, O. B.; Kjellström, E. & Zorita, E.: Projected Change—Atmosphere. In: The BACC II Author Team (Eds.): Second Assessment of Climate Change for the Baltic Sea Basin, Springer International Publishing, 2015, 217-233, doi:10.1007/978-3-319-16006-1_11

Lehmann, A.; Getzlaff, K. & Harlaß, J. (2011): Detailed assessment of climate variability in the Baltic Sea area for the period 1958 to 2009. Clim. Res., 46, 185-196, doi:10.3354/cr00876

Lindenberg J, Mengelkamp H-T, Rosenhagen G (2012): Representativity of near surface wind measurements from coastal stations at the German Bight. Met Z 21:99-106.

Rutgersson, A., Jaagus, J., Schenk, F., Stendel, M., Bärring, L., Briede, A., Claremar, B., Hanssen-Bauer, I., Holopainen, J., Moberg, A., Nordli, O., Rimkus, E., and Wibig, J.: Recent Change – Atmosphere. In: The BACC II Author Team (Eds.): Second Assessment of Climate Change for the Baltic Sea Basin. Springer International Publishing, 2015, 69-97, doi:10.1007/978-3-319-16006-1_4
Interactive comment on Earth Syst. Dynam. Discuss., doi:10.5194/esd-2016-71, 2016.