Earth Syst. Dynam. Discuss.,
doi:10.5194/esd-2016-71-RC2, 2017

Earth System Dynamics Discussions

Interactive comment on "A wind proxy based on migrating dunes at the Baltic Coast: statistical analysis of the link between wind conditions and sand movement" *by* Svenja E. Bierstedt et al.

Anonymous Referee #2

Received and published: 16 February 2017

Review of Bierstedt et al. – A wind proxy based on migrating dunes at the Baltic Coast: statistical analysis of the link between wind conditions and sand movement

Our understanding of past decadal to long-term changes of wind statistics is very limited. Longer observations or robust wind reconstructions are mostly rare or absent. One idea to overcome this problem is to use Aeolian transport of dust or sand particles as a proxy for variations in atmospheric circulation and wind climate. Different often local applications such as grain size analysis in sediment cores from peat bogs or dunes etc. have been used. Making use of the layered structure of sand dunes, the authors test here statistically to which extent the annual layers can be linked to driving atmospheric variables, predominantly wind.

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:

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

Statistics: 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:

Abstract:

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.

Introduction:

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.

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

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

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

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?

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.

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

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).

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 artefacts 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 $\sim$1980.

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

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

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

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.

Page 5, line 31: compiled

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.

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.

Page 7: line 2: Please explain in more detail how such a ratio or difference might look like and why, I cannot follow here. What are the x-axis units in Fig. 8?

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.

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

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.

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

Page 9, line 17: “this season and direction” – It makes sense to use the best combination but you should reconsider whether the best combination might not be the full wind season (e.g. SONDJFM) as mentioned before. As you mentioned the dunes in Lithuania, it would make sense to also provide your regression model for re-use or reproduction of results.

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

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?

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.

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

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.

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.

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.

Figures:

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.

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

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

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

Additional references:

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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

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