

# Response to the reviewers

## 'Significant multi-decadal variability of German wind energy yields' (wes-2019-8)

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Dear Julie Lundquist, Sonia Jerez and anonymous reviewer,

we want to thank you for your efforts in assessing our manuscript. We are convinced that your critical, fair and thoughtful comments helped us a lot in further improving the manuscript.

We understand that both reviewers agree on the relevance of the topic (anonymous reviewer: "addresses an important subject", Sonia Jerez: "paper addresses a key aspect of wind energy variability"). Both reviewers also find the paper to fit within the scope of WES by suggesting major or minor revisions.

At the same time, the reviewers disagree on some details of the manuscript and we discuss these details below. Please note that it was not always possible to reconcile contradicting assessments. In some cases we consequently could not modify the manuscript in line with the reviewer's suggestions as this would have led to conflict with the other reviewer's assessment (and our own).

In this response we address all comments and explain how we handled them. Throughout this text, **red** denotes deletions from the original manuscript and **green** denotes additions. Citations from the reviews are given in *italics*.

### Reviewer 1

*This manuscript addresses an important subject and helps in the "Global Stilling" debate. The analysis of wind trends has to separate between multi-decadal trends such as those forced by the North Atlantic Oscillation and the (expected) reduction in overall wind speeds due to the decrease of the polar-tropical temperature gradient. The reduction of wind speeds in Central Europe has implications on the economic benefits from wind turbines erected during the (necessary) transition process towards a fossil-free energy infrastructure.*

### Response by the authors

We thank the reviewer for his/her assessment. We believe that there might be some misunderstandings and would therefore like to clarify two aspects.

First, the paper does not provide evidence for multi-decadal trends. We mention the upward trends in (C)ERA20C (e.g., in Table 1) and discuss that they are likely spurious following the argumentation in an earlier paper by us (see p.9 ll.4f). Instead of reporting long-term trends, the paper reports long-term variability that is consistent across all current 20th century reanalyses.

Second, we are not aware of convincing evidence for the reviewers claim of an expected reduction in overall wind speeds due to a reduction of the polar-tropical temperature gradient. To our best knowledge, the claim is in disarray with modeling studies which report different signs of change in different regions rather than a general decline (e.g., Tobin et al., 2016; Karinauskas

et al., 2018). With respect to 'global stilling', we refer to our reply to specific comment 1 where we discuss global stilling. Please also note the changes of the manuscript that are reported there.

## Comment 1

*Unfortunately, the manuscript is not well balanced. Title, Abstract and Introduction rise the expectation that this paper mainly deals with the separation between multi-decadal variability and Climate Change. But only half of the conclusions reflect findings belonging to this issue. The other half of the Conclusions focusses on economic aspects of our future energy supply and speaks about transmission lines across Europe and "decadal energy storage systems". How such systems could look like is not detailed (unfortunately).*

## Response by the authors

We thank the reviewer for bringing up these points. We agree that the discussion focused too heavily on the market design and general aspects and removed one paragraph to correct for this (see below). We also extend the Introduction as suggested by the reviewer (see Comment 3 for a more detailed discussion and the changes).

With all due respect, we disagree with the reviewer's interpretation that Title, Abstract and Introduction raise the expectation of separating between multi-decadal variability and climate change. Instead, the paper documents the existence of multi-decadal variability of wind energy generation. This is reflected in the title and abstract, which both do not mention climate change at all. Climate change impacts on renewables are mentioned in the introduction (ll. 13-19) in order to contextualize our finding with recent efforts in the community.

We believe that reporting the economic impact of multi-decadal variability on wind park revenues as discussed in Sec. 4.4 and Fig. 6 is a valuable extension of the analysis. It reflects the information needs of the different stakeholders in wind energy. We therefore decided to keep this part unchanged.

## Changes in the manuscript

lifetime revenue spread at the order of b 10 in Germany alone.

The effect of wind variability on revenues obviously depends on the market design. Instead of guaranteeing a constant price for wind energy, adaptive prices that fall in times of high generation and decline in times of low generation could dampen the economic effect of multi-decadal wind variability. We speculate that a higher price of CO<sub>2</sub> emission allowances in combination with an end to guaranteed renewable feed-in might be a possible route forward. The increased CO<sub>2</sub> emission allowance price would guarantee that renewables are favoured over fossils for mere economic reasons and it would also ensure sufficiently high market prices. During decades of high (low) wind generation, the average market price would fall (increase) thereby smoothing the variability of revenues and reducing the risk for investors. However, this strategy would only constitute an interim solution as it relies on a substantial share of non-renewable generation. In a future zero emission energy system, all variability from wind generation needs to be balanced by other means, for example through sector coupling, flexible demands or large scale storage (Brown et al., 2018). It might become necessary to ponder decadal energy storage systems or to use the atmosphere as a carbon storage (Wohland et al., 2018b).

Our study raises new questions.

## 77 **Comment 2**

78 *Another conclusion is that the atmosphere could be used as a carbon storage. What does this*  
79 *mean? Once again, no explanation is given.*

### 80 **Response by the authors**

81 We thank the reviewer for this comment. We agree that this paragraph was too loosely coupled  
82 to the rest of manuscript and decided to remove it in order to put more focus on the main  
83 message of the manuscript (see Comment 1).

84 Nevertheless, we would disagree that no explanation was given. The sentence that the reviewer  
85 refers to reads: "It might become necessary to ponder decadal energy storage systems or to use  
86 the atmosphere as a carbon storage (Wohland et al., 2018b)". It does contain a reference  
87 to another publication in which we discuss the potential co-benefits of a negative emission  
88 technology called Direct Air Capture and renewables. Applied to multi-decadal generation  
89 variability, Direct Air Capture could be used to remove CO<sub>2</sub> in windy decades which would  
90 allow for carbon emissions from backup power plants in calm decades.

### 91 **Changes in the manuscript**

92 See Comment 1.

## 93 **Comment 3**

94 *The task of a major revision of this manuscript has to be to prepare the discussion in the second*  
95 *half of the Conclusions by additional paragraphs in the Introduction.*

### 96 **Response by the authors**

97 We thank the reviewer for this suggestion. Following his/her comment, we extended the In-  
98 troduction which now contains additional information about the market design and renewable  
99 portfolios. We also mention the global stilling phenomenon now (see also next comment). We  
100 are confident that these changes improve the legibility of the manuscript.

### 101 **Changes in the manuscript**

102 p. 2, l. 1f.

103 While planning is typically based on 20 year lifetimes, real-world experiences suggest that  
104 turbines can be operated even longer (Ziegler et al., 2018). The current german market  
105 design privileges renewables over conventional generators via a guaranteed feed-in and  
106 wind park operators are compensated for congestion-related curtailment. This implies  
107 that there is no market incentive for planners to increase the system-friendliness of their  
108 wind parks. In particular in cases where a trade-off has to be made between total energy  
109 generation and system-friendliness, planners and investors will likely prefer the former  
110 over the latter.

111 p. 2, l. 3f.

112 This fact is increasingly accounted for in energy system models (a recent overview is  
113 provided by Ringkjøb et al., 2018). Portfolios of different renewables and large-scale trans-  
114 mission can mitigate generation variability (e.g., Heide et al., 2011; Schlachtberger et al.,  
115 2017). Underlying wind ...

116 p.2, l. 15 f.

little emphasis has been put on the natural low-frequency variability of wind energy (with the notable exception of Bett et al., 2013, 2017). Natural low-frequency variability could also help to explain trends in surface wind speeds computed over a few decades (commonly referred to as global stilling, Vautard et al., 2010) if the period featuring the trend coincides with the downward sloping fraction of multi-decadal variability. The fact that...

## Specific comment 1

*The "Global Stilling" debate which has been brought up again recently should be addressed in the Introduction and in the Conclusions. This would help to put the results of the present manuscript into the proper perspective.*

## Response by the authors

We would like to thank the reviewer for this very thoughtful comment. As already mentioned in Comment 3, we added the global stilling phenomenon to the Introduction. We also add a short discussion in the Conclusion, as suggested.

## Changes in the manuscript

p. 16, l. 6f.

This connection to a physical pattern of climate variability suggests that the peak is not a statistical artifact, despite its low statistical significance.

Wind power generation reached a multi-decadal maximum around 1980 implying that trend assessments starting in 1980 suffer from a sampling bias. The downward sloping fraction of multi-decadal variability should not be confused with a long-term trend and an extrapolation of the trend into the future is misleading. These results are relevant in contextualizing the global stilling phenomenon (Vautard et al., 2010).

Our results imply that in

## Specific comment 2

*Page 1, line 20: Comparing an estimated revenue from wind energy devices to the overall value of a car company is a bit strange for a scientific paper.*

## Response by the authors

We intended to provide an intuitive comparison for wind energy investments. We think that such a comparison is helpful because most readers will likely find it hard to put b€95 into perspective. In the respective sentence, we therefore compare the investments in wind energy to the current stock market values of two major companies in Germany.

## Specific comment 3

*Page 2, line 30-32: The evaluations in this manuscript are understood to be based on (grided) reanalysis data. Therefore, the reviewer does not understand why there is the necessity to extrapolate wind speeds from the 10 m level to hub height. There should be data from heights much closer to hub height. And if this large extrapolation has to be made, then the power law is not helpful. The exponent of the power law depends on height above ground, surface roughness and atmospheric stability. The surface roughness-dependency makes the exponent*

*wind direction-dependent. Especially due to the height dependency of the power law exponent, this law is only suitable for extrapolations over small height intervals. Anyhow, the extrapolation necessity and procedure has to be described in much more detail.*

## **Response by the authors**

We thank the reviewer for these methodological suggestions. We are fully aware of the assumptions behind a power law with a fixed coefficient and the deficiencies of such an approach.

Unlike the reviewer's expectations, however, wind speeds are not available at another height across the entire ensemble. While the ECMWF reanalyses contain 100m wind speeds, 20CR does not. In order to ensure comparability, we decided to apply the same methodology to all datasets and therefore based the assessment on 10m winds. We add a sentence to the manuscript to highlight this aspect of the approach (see below).

More importantly, we would like to emphasize that we validate our approach in Section 3 (validation) by comparison with an ERA-interim based timeseries that was validated with real generation data in 2015 and 2016. Owing to the good agreement between the 20th century reanalyses and ERA-interim, we are convinced that our approach is well suited for a country level assessment on long timescales (see also Fig. 2). In other words, the error introduced via the extrapolation from 10m to 80m does not matter here.

The methodology is detailed in another paper that we explicitly refer to in lines 29f. ("We derive nationally aggregated wind generation timeseries for the period 1901-2010 following the procedure detailed in Wohland et al. (2018a)". For the sake of brevity, we considered it more appropriate to only provide a brief overview of the procedure as the interested reader can access the details in the referenced publication.

## **Changes in the manuscript**

p. 3, l. 12f.

as the spread is usually very limited. Our analysis is based on 10m wind speeds. In contrast to higher level wind speeds, they are available for all 20th century reanalyses allowing us to apply the same methodology to all datasets and thereby ensuring comparability. We validate the approach in Sec. 3.

The longer

## **Specific Comment 4**

*Section 4.2: this Section contains interesting results. According to the title of the manuscript, the reader expects more explanations about how the founded trends are connected to the North Atlantic Oscillation. Why is there an anti-correlation between the winter trends and the summer trends leading to such strong variations in the seasonality? And why are the frequencies in Fig. 5 varying so much with the season? (major peak at 0.02 for winter and autumn, 0.03 for summer and 0.04 for spring?) If they all relate to NAO, shouldnt they have all the same frequency dependence? Much more discussion is needed on this interesting finding!*

## **Response by the authors**

We thank the reviewer for this comment and we want to reassure the reviewer that we are very interested in developing a process understanding which might help to answer his/her questions.

We believe that he/she refers to increasing or decreasing intervals of multi-decadal variability when using the term "founded trends". If this is not correct, we would like to ask the reviewer for a clarification on this point. We will not use the term "trend" here for a fraction of multi-decadal variability as we find the wording potentially misleading.

We do not really understand why the title suggests a link between multi-decadal variability and the NAO. The title does not mention the NAO.

So far, we do not have any process understanding why the multi-decadal variability of winter and summer generation are anti-correlated. Only in winter, we find a good connection between multi-decadal wind generation variability and multi-decadal variability of the winter NAO. Acknowledging the importance of the reviewer's comment and the overall value of process based understanding, we add two sentences in this regard to the discussion (see below).

The fact that spectral peaks occur at different frequencies in different seasons hints toward different underlying processes. We agree with the reviewer that this aspect has not been sufficiently clear yet and add a short paragraph (see below). To our best knowledge, there is no reason for all seasons to show spectral peaks at the same frequencies. We would be delighted if the reviewer could point us to any evidence or theory that points toward the necessity of spectral peak at the same frequency.

## Changes in the manuscript

p. 13, l. 12f.

during the historical period.

Spectral peaks generally do not exist at the same frequencies in different seasons. This implies that the relevant processes vary by season. While the winter NAO explains a large share of the winter variability, similar explanations can currently not be given for the other seasons.

Interestingly, the AR(1)

p. 17, l. 12f.

similar modes exist for photovoltaics and hydropower? In addition to the winter link between wind power generation and the NAO, other connections between multi-decadal renewable generation and large scale patterns of climate variability might exist. They could contribute to a process based understanding and should therefore be investigated in future work. Lastly, climate models are,

## Specific Comment 5

*Page 16, lines 14 to 15: Is the request for a perpetual redesign of power systems realistic? It sounds well, but how to do it?*

## Response by the authors

We thank the reviewer for this comment. With the sentence, we intended to highlight that an optimum system design does not exist due to climatic variability at timescales comparable to or even exceeding the lifetime of power system components. Instead, the optimum system itself varies with multi-decadal wind variability.

With respect to the implementation side, we clearly have no definite answers. It is our intention to underline that infrastructure decision making should use multi-decadal climatic information. Given that electricity and energy systems evolve constantly (by additions, replacements, retirements of individual components), the energy system is in constant change which could be used to react to changes in the climatological boundary conditions.

We added another sentence to make this point clearer.

## Changes in the manuscript

p. 16, l. 14f.

This variability calls for a perpetual redesign of power systems to follow climate variability. Even though the lifetime of individual power system components (e.g., transmission lines or power plants) is very long, additions, replacements and retirements occur frequently within the entire power system. These events theoretically allow for adaptive reactions to multi-decadal variability. ERAINT

## Specific Comment 6

*A more appropriate title could be: "Multi-decadal atmospheric variability and power system design". Then the reader would expect what is found in the Conclusions. But it then becomes debatable whether this manuscript still fits into the spectrum of this journal.*

## Response by the authors

We want to thank the reviewer for his/her suggestion to update the title. However, we think that his/her suggestion is less exact than the initial title as it (a) does not mention the spatial scale (Germany) and (b) does not mention the variable that is studied in the paper (wind energy generation). We would therefore prefer to stick to the initial title. We also think that the initial title does a better job at not creating wrong expectations (which the reviewer has been repeatedly commenting on).

## Reviewer 2 (Sonia Jerez)

*I think the paper addresses a key aspect of wind energy variability by focusing on its low frequency spectrum. The manuscript reads very well, the analysis is well designed, the conclusion are well rooted on the shown results and the figures are clear. I therefore recommend its publications almost as it is.*

## Minor comments

### 1

*Fig 1. Caption: ERA20CM?*

We deleted ERA20CM as it was not used in the paper.

### 2

*Fig 2. Correlation between daily records should be computed from detrended series with removed seasonality, shouldn't it?*

Thank you for this comment, Sonia. Our answer is maybe and no.

You are correct that the correlations could be even higher if the trends were removed beforehand. However, given the fact that we only look at 30 years of overlap here, the impact of the trends is not as strong as if we would consider the entire 20th century. We therefore argue that a trend correction is not necessary here.

Second, we do not see any necessity to remove the seasonal cycle. This is because seasonal variations are synchronous in the 20th century reanalyses and in ERA-interim. A comparison between, for example, winter ERA20C and winter ERA-interim values is thus a fair comparison.

### 3

*Table 1. 20CR version  $\rightarrow$  20CR*

Yes. Thanks.

## 4

*Fig 3. Maybe good to use the same y-axes for all subplots. In the context of lines 18-20 in page 9.*

We agree that using the same y-axis would underline the argument that changes are larger for the seasonality and the seasonal values when compared to annual values. However, it might also create the impression that changes of the annual timeseries can be neglected. We would strongly disagree with this impression because  $\pm 5\%$  is far from negligible. For instance, the current debates about wind farm blockage shows clearly that the wind energy sector is interested in in deviations of projected and actual wind energy generation at the order of 1% (Bleeg et al., 2018).

## 5

*Fig 4. Correlations are calculated between 20y mean anomalies, which means between 20y runmean anomalies, right? Are these means computed for the 20 years ahead (Equation 2)? In both cases?*

The correlations reported in Fig. 4b are calculated from the timeseries shown in Fig. 4a,c,d,e,f. The timeseries are calculated using Eq. 2 (annual values) or Eq. 3 (seasons). In both cases, a forward running mean is computed. The normalization applied to the timeseries does not matter for the correlation as a constant scaling of the timesiers has no impact on the correlation coefficient.

## 6

*Fig 4. Panel b maybe better at Fig 5.*

We believe that this is a question of style and there are good arguments for both options. Moving it to Fig. 5 would indeed mean that all spectra are shown in the same Figure. However, leaving it in Fig. 4 means that the entire argument related to the winter variability and the connection to the NAO is kept in one Figure. We prefer the second option as it emphasizes this interesting result.

## 7

*Page 16, line 10. ERA5 is expected to be entirely published BY THE END OF 2019.*

Thanks. We correct accordingly.

## References

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