Response to Anonymous Referee #2, wes-2020-134

The authors would like to thank the referee very much for the extensive review. We think that the remarks are very helpful and that they will help to improve the quality of our paper. In the revised manuscript, we considered the comments as follows (responses in blue).

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Summary: This papers aims to test two MCP (measure-correlate-predict) methods for a series of re-analysis products and analyze whether errors in the mean or in the variance contribute most to the total error or not. The paper is an interesting analysis and presents useful insights in the math behind the MCP methods. The paper is however difficult to read and needs major revisions.

Recommendation: major revision required

Thank you very much for the positive general feedback.

Major remarks:

1. My main concern is that the paper is not easy to read. Many parts of the text have very short paragraphs of only one or two sentences, so here and there the text is very fragmented.

   We have removed several line breaks, rephrased some sentences and made some linguistic improvements.

   Also the paper does not provide sufficient explanation of the parameters used. The further I got into the paper, the more often I had to go back to the beginning of the paper to find out what was exactly meant by different terminology. So more physical/dynamical meaning should be given to the parameters that are introduced.

   We have added some additional explanations in the revised manuscript. As we feel that too many repetitions would raise other problems, we had decided to give a general explanation of the subscript labels at the beginning of the methodology section (lines 129-132 in the revised manuscript). Considering this general explanation, we hope that the parameters used in the paper are now easier to understand. Additionally, we have worked with some references to equations and sections in order to guide the reader to the respective definitions if necessary.

2. The paper concerns only the MCP methods for mean wind and energy (u^3). But I was wondering whether extreme events in the wind field like ramps (up and down), low level jets and wind shears would also be interested to study, since they have a big impact on the wind energy production variability in time and on the wind turbine installations.

   Long-term correction is a procedural step in the preparation of wind resource assessments, which is described in various guidelines (e.g., FGW e.V. 2020; MEASNET 2016). It is our aim to publish relevant findings for this step of the procedure and we would like to focus precisely on this. Despite the mentioned aspects would certainly be interesting to investigate, we therefore decided not to include these in this study.

Extreme events do not play a central role for the statistical estimation of the wind speed distribution in Germany on land and, thus, the long-term wind resource. Therefore, wind ramps do not lie in the focus of our study. However, they are to some extent implicitly included in other parameters we investigate (e.g., variance of wind speed, or Err_{ED}).

Similarly, we did not include analysis on low level jets as they do not play a significant role at onshore sites in Germany.

The use of the rotor-equivalent wind speed would also make the seasonal variation of the shear interesting to study. However, this would be beyond the scope of this paper and should be addressed separately.

3. In terms of re-analyses that were used in the study I was surprised the COSMO REA family (https://reanalysis.meteo.uni-bonn.de/?Download_Data___COSMO-REA2) was not present, since it was especially made for Germany.

   We discussed taking these data into account, because both the REA 2 and the REA 6 data would in principle be very interesting for Germany due to their COSMO assimilation procedure. However, the global data (ERA-Interim) driving the DWD REA data already expired in 2019. This means that the data has no significance for long-term references of current and future wind farm projects and was therefore not taken into account by us. Besides that, the COSMO-REA2 data are only available for the period of 2007-2013 (according to the link you provide). The measurement campaigns from which we used the data for the analysis were carried out later than this period.

4. It is a pity that the paper only use the different re-analysis products as illustration for their mathematical exercise. I think for many WES readers it would be interesting to be more specific under which meteorological conditions which product “is best” or “performs less accurate”. Also it would be interesting whether more detail can be added about the physics/dynamics behind the variability. Is the error due to missed sea breezes, or Alpine pumping events or low level jets etc. The observational data across Germany is very rich so more of this kind of info might be extractable.

   In our study, we investigate the theoretical and empirical background of the seasonal effects (including reasons). We agree that a broader examination of under which meteorological condition which reanalysis data perform best, would be interesting. However, we feel that enlarging the study in this way would go beyond the scope of a clear and focused paper. Similarly, we agree that regional differences would be interesting to study, but too extensive to be included as part of this publication.

5. The Conclusion section should be rewritten since I find there is too much jargon in it. Conclusions are based on the beta parameters, but this makes the conclusion difficult to read as a separate text, which many people do. Please reword.

   We revised the conclusion, rephrased some sentences and added some information in order to make this section easier to understand even when not having read the whole paper.

Smaller remarks:

Ln 19: please cite in chronological order, here and please check complete manuscript.

We changed the order of the references accordingly in the revised manuscript.

Ln 45: overperform: do you mean “outperform”?

Yes, thank you for that remark. We changed it in the revised manuscript.

Ln 55: Strange sentence: if the costs are so low, it is an argument to do more rather than less experiments.

We rephrased that sentence and added a remark on the running costs of lidar measurements.

Ln 95: Better to refer to the Hersbach 2020 paper in QJRMS.

We changed the reference in the revised manuscript.

Ln 134-135: Maybe I misunderstand the strategy here, but if you have taken 90 day periods with each 3 days intervals, then you still sample from a complete year (I read it as i you take 1 Jan, 4 Jan, 7 Jan .....). So this is not how a measurement campaign occurs where maybe only one or two months are sampled.

Indeed, we take 1 Jan, 4 Jan, etc. as starting points for the 90-day periods. We added this information in the revised manuscript in order to make that clear. Consequently, we have 122 individual (while overlapping) periods and each period is investigated independently from the others. In this way, it is possible to calculate average statistics (e.g., errors in mean wind speed) for these 122 90-day periods. These form the basis for the question which errors occur (on average), when a measurement campaign is started on a certain date.

Ln 134-135: if you complete the series at the end of the series with the new year, is the winter overrepresented in this analysis?

The winter is not overrepresented, as each 90-day period of a full year is considered exactly once in the analysis (see explanation above). For the case that a 90-day period exceeds the period in which measurement data is available, the data from the beginning of the measurement year is appended.

Example:

Measurement data at site X is available for Aug 2018 - Jul 2019 (i.e., the “real” measurement campaign as part of our data basis took place at that time). For the 90-day period covering the summer (or rather: the 90-day period June 1 - Aug. 29), the data from June and July 2019 and from August 2018 are used.

Of course, this does not correspond to the procedure in practice. It can be used in the analysis, however, as the correlation (MCP) is not restricted to contiguous measurement periods.

Ln 170: please explain more early in the manuscript what are umeas, Umeas, uref and Uref.

As mentioned above, we explain the general meaning at the beginning of the methodology section (lines 129-132 in the revised manuscript).

Ln 202: a one-year time series is generated: but this is inconsistent with was written in line 134-135 where you say you sample 90-days periods.

The measurement data from the 90-day periods is correlated with the reanalysis data and MCP predictions are performed. The result is a one-year time series (i.e., the reanalysis data is corrected for a period of 1 year). In the methodology section, we added a diagram in order to further explain the procedure.

Ln 217: extent

We corrected that, thank you.

Ln 216-219: this paragraph is extremely abstract

We have rephrased it in the revised manuscript.

Ln 257: explain why “true” is between “”.

As the sentence reads “Deviation of "true" mean wind conditions (measured data) in measurement and long-term period”, we feel that the supplement “(measured data)” explains what we mean with “true” mean wind conditions. We use quotation marks as the measured wind data does not exactly reflect the real wind speeds (because of measurement errors), but is expected to be accurate enough in this study to identify errors of the MCP predictions.

Ln 260: representativity: do you mean representativeness??? Please check several places in the manuscript.

We have changed that in the entire manuscript.

Ln 312: Differences occur in the amplitudes.: short and weird sentence. What do you want to say?

We rephrased that to “The amplitudes of the curves (…) differ, indicating clear differences between the reanalysis data sets.” (line 333).

Figure 1: please add in the caption how the normalization was done, so the reader does not need to go through the manuscript again to look it up.

We have done so in the revised manuscript.

Ln 326: might be caused .....: this is speculative. Please prove what you would like to say here.

As we did not get detailed information on the developments done by anemos, this certainly is speculative (despite quite likely, though). In this passage, we find it reasonable to mention the differences in the two data sets nevertheless.

Ln 330: Please explain more what you want the reader to learn from Fig 3.

We added an explaining remark on that at the respective passage (Sect. 5.2).

Figure 5: caption: please reword caption. You do not show seasonal bias, but the bias through the different months. The plot does not show bias for DJF, MAM, JJA, SON...

We changed it to “Temporal variation during the year of the bias in mean wind speed (…).” Additionally, we changed the captions of the figures regarding the other error scores (variance, energy) accordingly.

Ln 391: “or rather because of the erroneous seasonal course of the ERA5 data.”: this is not clear to me since Fig 1 says that ERA5 has a correct seasonal cycle.

We rephrased the sentence using the term “overpronounced annual cycle” in order to make it easier to understand.

Ln 405: However, the authors expect it to be rather small: argue why, prove with data or physical reasoning.

We removed this sentence in the revised manuscript.

Ln 413: these: please indicate to what “these” refer to

We added a remark making clear that we refer to the findings from literature (lines 443-444).

Ln 428: From that it is likely that not all the reference: messy sentence that makes the reader lost.

In the revised manuscript, we rephrased that sentence (lines 458-459).

Ln 446: “The authors” -> we. Now it sounds as if you place it beyond yourself.

We rephrased that in the revised manuscript to “It can be expected that…”.

Figure 9: the variables on the x and y axes should be switched, since the observation is the known and the MERRA is the modelled/predicted.

Since the observation is the target variable and MERRA-2 is the input variable (of the MCP), we feel that the variables should not be switched.

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References

FGW e.V.: Fördergesellschaft Windenergie und andere dezentrale Energien (FGW): Technical Guidelines for Wind Turbines: Determination of Wind Potential an Energy Yield (TR6)., 2020.

MEASNET: Measuring Network of Wind Energy Institutes: Evaluation of Site-Specific Wind Conditions: Version 2 April 2016, 2016.