Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2020-85-AC1, 2020
© Author(s) 2020. This work is distributed under
the Creative Commons Attribution 4.0 License.

WIND ENERGY SCIENCE DISCUSSIONS

WESD

Interactive comment

Interactive comment on "An Overview of Wind Energy Production Prediction Bias, Losses, and Uncertainties" *by* Joseph C. Y. Lee and M. Jason Fields

Joseph C. Y. Lee and M. Jason Fields

chle6805@colorado.edu

Received and published: 16 October 2020

We thank the reviewer for conducting a deliberate review to improve our manuscript, we greatly appreciate it.

Regarding your comment on the length of the tables, we will discuss with copy editors about the options to shrink them. Moreover, we want to keep the tables in Appendix A because the definitions presented in Appendix A are fundamental in categorizing different losses and uncertainties in the wind resource assessment process. We understand the final standards are in the works, that is why the text and tables refer it as a "proposed framework". We edited the text in Sect. 1 (lines 58 to 63) and Appendix A

Printer-friendly version

Discussion paper

CC BY

(line 593) to make this clear.

Our responses to your specific comments below begin with “Response:”.

l.10: the expression “near-zero” is qualitative (how much has the bias been reduced?), and contrary to the notion of uncertainty quantification–which is a primary subject of this work.

Response: The sentence is changed to “. . . recently the reported average energy prediction bias is reducing.”

l.28 to what average are you referring? Do you mean that your subset of US wind farms gave a bias of 3.5 to 4.5%, or do you mean that there is a distribution of biases over e.g. 2-year rolling periods?

Response: The former interpretation is correct. For clarity, the sentence is changed to “A recent study conducted by the researchers at the National Renewable Energy Laboratory (NREL) found an average of 3.5% to 4.5% P50 overprediction bias based on a subset of wind farms in the United States and accounting for curtailment (Lunacek et al., 2018).”

l.30–34: are you defining P50 in terms of a 2-year basis? If so, you should have done that in l.24–25. Then for the long-term (e.g. 20-year lifetime), you are considering the distribution of overlapping 2-year “P50” values. It is the width of this distribution (e.g. its ’sigma’ if symmetric, or associated P25 or P10) which determines moreso the odds of underproduction. I.e., the “1%” on line 32 is crucial to such.

Response: We refer to the P50 in 20-year time frame for this manuscript. The reference we discuss here uses a specific P50 definition, which is the 1-year P50 within a 2-year rolling period. We are citing this reference here to explain why P50 overprediction has implications. To clarify the P50 definition used in this manuscript, lines 24 to 26 now read, “P50 are often defined with timescales such as 1 year, 10 years, and 20 years. In this study, unless stated otherwise, we primarily discuss the 20-year P50, which is

CC BY

the typical expected lifespan of utility-scale wind turbines."

l.30–35 and Section 1 generally: if using a statistic such as P50 with a particular (e.g. 2-year) definition, would it not make sense to show a distribution of this?

Response: As mentioned above, we focus on the 20-year P50 in this manuscript. By definition, P50 is the predicted median annual energy production, which does not have an underlying distribution.

l.35–36: You mention uncertainty in a vague sense, but it would be helpful to explicitly state what is/can be quantified; this again relates to the comments above.

Response: Per your suggestion, we added a discussion on uncertainty. Lines 50 to 55 now read, "Random errors that deviate observations or model predictions from the truth lead to uncertainty (Clifton et al., 2016), and uncertainty can be expressed in probability (Wilks, 2011). In WRA, the P-values surrounding P50 such as P90 and P95 characterize the uncertainty of the predicted AEP distribution. Such energy-estimate uncertainty depends on the cumulative certainty of the entire WRA process, from wind speed measurements to wind flow modeling (Clifton et al., 2016). Given a Gaussian distribution, the standard deviation around the mean represents the uncertainty of that distribution. Traditionally, the wind energy industry uses standard deviation, or ïĄş, to represent uncertainty."

Fig.2: There is no depiction of the combination of uncertainties; this itself is a nontrivial aspect. Also, "stressor" under Vertical Extrapolation should be "stress" to be consistent with horizontal extrapolation.

Response: The discussion of combined uncertainties (in terms of total uncertainty) is listed in the beginning of Sect. 5. Besides, we used "stressor" under vertical extrapolation in Filippelli et al. (2018). We are following your suggestion and change this and subsequent instances to "stress" instead.

l.55: "financial impact" is also found in the other 2 bullet points (see annotated PDF).

CC BY

Response: Lines 88 to 96 have been edited accordingly, thank you.

l.96–97: What distribution (PDF) are you assuming, to estimate 95l.97–98: Do you mean boot-strap resampling from the entire sample? How much of the sample, and is re-sampling allowed, conditional or otherwise?

Response: The construction of the bootstrapped confidence intervals is based on a Gaussian distribution, according to the central limit theorem; we do not presume any specific parametric distribution for the data. The bootstrapped sample size is the same as the original data (e.g. 63 in Fig. 3). By definition, resampling is allowed in bootstrapping, and the resampling with replacement is random. These details are included in Waskom et al. (2020), which is cited here. We also edited the text to make this clearer, and lines 151 to 170 now read, "We also derive the trend of P50 prediction errors using polynomial regression and investigate the reasons behind such trend. We use the second-degree polynomial regression (i.e. quadratic regression) to analyze the trend of the P50 prediction errors over time, and polynomials of higher degrees only marginally improve the fitting. We choose the polynomial regression over the simple linear regression because the P50 prediction errors are reducing towards zero with a diminishing rate and we use quadratic polynomial over higher order polynomials to avoid overfitting. Additionally, in the regressions presented in this article (Figs. 3, 8, and C1), we present an estimated 95% confidence interval, generated via bootstrapping with replacement using the same sample size of the data, which is performed through the regplot function in the seaborn Python library (Waskom et al., 2020). The confidence interval describes the bounds of the regression coefficients with 95% confidence. Furthermore, we present the 95% prediction interval in Fig. 3, which depicts the range of the predicted values, i.e. the P50 prediction bias, with 95% confidence, given the existing data and regression model. The prediction interval is calculated using standard deviation, assuming an underlying Gaussian distribution."

l.101: is the prediction interval assuming a Gaussian distribution, or what?

CC BY

Response: That is correct. Please see our response above for the edits in the text.

l.101–3: Why do you evaluate the R2 of the linear fit? What does this tell you? More importantly: why would you use a linear fit for a quantity that is unlikely to continue to rise linearly? The bias is decreasing, towards zero, and will likely not increase beyond that at the same rate.

Response: The reviewer raised an excellent point. We now switched to the second-degree polynomial regression for Fig. 3. We also expanded the methodology description from lines 151 to 173. The R2 is a commonly used metric to evaluate statistical fitting, and it describes the variance of the predictand explained by the regression.

l.104: do you "need to interpret a small subset", or are your forced to to so?

Response: We only have limited data for a specific category or subcategory of loss and uncertainty. Lines 174 to 175 now read "For loss and uncertainty, we have limited data samples for certain categories because these data are only sparsely available."

l.156–7: you argued in the previous paragraph that the low R2 of the linear fit means most of the variability in bias is not described by the regression. Thus how can you say the bias is approaching zero? Statistically, you can say that its magnitude is decreasing; again, perhaps a linear trend is not appropriate (though this is difficult also to prove statistically, given the limited data).

Response: Thank you. Because we changed from linear regression to quadratic regression, the R2 has improved in Fig. 3b, where the studies based on fewer than 10 wind farms are removed.

l.151: why is 'typically' included? Isn't it just one standard deviation?

Response: We checked that the uncertainty values presented in Fig. 3 and Table B1 all represent one standard deviation from the mean. The relevant sentences here and throughout the manuscript are modified.

Printer-friendly version

Discussion paper

Fig.5: In the caption, indicate how many observations/cases/references were used.

Response: This is a great suggestion, and we added the sample size as part of this plot and all the similar plots in the manuscript (Figs. 5, 6, 7, 9, 10, 11, and 12). We edited the captions accordingly as well.

Fig.6: there appears to be no “observed max” in any subcategories, except degradation. Perhaps explain why there are relatively few yellow dots.

Response: We display the references we can gather in this study, and the sample size of observed numbers usually trails that of the estimated values. The lack of observations is discussed in the last paragraph of Sect. 7. Regarding your comment that only degradation has “Observed max” values, this is what the data show.

Fig.12 (also 9): the intermonthly variability appears to be much too large; is this taken out of context? E.g., is this a just a higher percentage of a smaller number than the other losses?

Response: For Fig. 9, we only have 1 data point for the observed uncertainty of energy production loss from month to month. That study explicitly cited that the intermonthly variability contributes to 10% to 14% of energy production loss, depending on the location. The study did not specify which types of categorical energy production loss it refers to, so we cannot answer on your last question with evidence. We understand that the value is nontrivial in magnitude while this is what was reported in that study.

Regarding Fig. 12, the green dot of intermonthly variability you refer to is also from the same study above. They observed 19% to 24% of wind speed variability from month to month, depending on the region.

l.305–306: This sentence is confusing. It appears that you are trying to say that the uncertainty in WRA is larger than the industry-wide mean bias; i.e. the ensemble mean error is smaller than the variability.

Response: The sentence now reads “Although the industry is reducing the mean P50

CC BY

overprediction bias, the remarkable uncertainties inherent in the WRA process overshadows such achievement."

l.341–2: "the resultant compound effect can become larger than the total value from a linear approach" is not mathematically correct. Here you are conflating two things: significant higher-order moments involving correlated values, and simple 2nd-order quantities that have significant correlation. Explicitly, the former causes extra terms which appear to give a 'sum' greater than the linear combination of two correlated component uncertainties; I remind that the latter is equal to the result for two perfectly correlated quantities.

Response: Thank you. Lines 528 to 531 now read "Furthermore, different types of energy-production losses or uncertainties interact and correlate with each other, and dependent data sources can emerge in the WRA process. The resultant compound effect from two correlating sources of uncertainty can change the total uncertainty derived using a linear (Brower, 2011) or root-sum-square approach (Istchenko, 2015)."

l.355: I'd suggest "being reduced" over time, not "approaching zero", because the uncertainty will not disappearâĂŤbut rather decrease, as practices and reporting improve.

Response: Thank you. The instances of "is approaching zero" in the manuscript are changed to "is reducing".

Table B1: The caption denotes "usually illustrates one standard deviation"âĂŤyou should note where it does not, e.g. with an asterisk (not just text, but in the table).

Response: Please see the comment above regarding line 151.

Table B1–headers: the values for 'Wind Farm' and 'Wind Farm Year' are not defined here in Appendix B.

Response: Thank you. The definition has been added, and lines 611 to 613 now read "The "Wind Farm" column denotes the number of wind farms reported in the reference, and the "Wind Farm Year" column indicates the total number of operation years among the wind farms in that study."

Technical corrections

There are a number of English usage errors; in the first pages I make a number of corrections and suggestions via the attached annotated PDF, to help get the authors started with this aspect of revision.

Response: Thank you. The copy-editing team of Wind Energy Science will also review the manuscript too.

l.73–4: disallow line-break between "Sect." and "5".

Response: We do not think the line-break here is against the rules of Wind Energy Science. Because the final form of the manuscript will have a different format, we are not editing the line-break here in this version.

Table 1/p.8: under 'improve modeling techniques', it should be "flow over complex terrain"; and "effects of changes in" needs to be prepended to 'surface roughness'.

Response: Thank you.

l.159: "of" should be "for"

Response: Thank you.

Fig.5 caption: English corrected to "losses are expressed as percentage of AEP"

Response: Thank you. Subsequent captions are also edited accordingly.

l.305: "immersed" should be "inherent".

Response: Thank you, this is a better word here.

Table B2: in Group 16, shouldn't the first Lunacek et al (2018) line be for projects before (not after) 2011? Also, should similar distinctions be included for the Lunacek [et al 2018] data shown on the first two lines of group 7?

cc BY

Response: You raised a great point. The three entries of group 7 are now labeled as “2010” with “Projects before 2011” in the notes. We also updated Fig. 4 accordingly.

references: many are to presentations at workshops/conferences, but lack any link or specific designations (e.g. session/talk numbers, etc.) within proceedings.

Response: Many of the presentations at AWEA and WindEurope conferences are only available for attendees or their members, and they often lack specific session details. We cannot provide the links to the presentations on our end because we do not possess the copyright. We are doing our best to document the references in this manuscript.

l.608: reference incomplete

Response: The technical report does not indicate any report number. We added the location of Ecofys, the company that published the report, in the citation.

l.632: update to 2019 report; also reference is incomplete (e.g. DTU report ...).

Response: We edited the citation and we included the 2019 DTU report in the analysis.

l.657: “M.J” should come after “Fields”, without ’Jason’; otherwise should be listed as e.g. “Fields, M. Jason”

Response: Thank you, the citation is fixed now.

l.675: reference is garbled (Denmark, in Ireland?)

Response: The conference location was Dublin, Ireland.

Please also note the supplement to this comment:

https://wes.copernicus.org/preprints/wes-2020-85/wes-2020-85-RC1-supplement.pdf

Response: We accepted a lot of your proposed changes. For those of your suggestions that require further discussions, please see our comments below.

CC BY

Line 156-157: “but you argued above that the "uncertainty between validation studies" is large enough that this is not necessarily true”

Response: With the improved quadratic regression, the reducing trend of the P50 bias is more reliable. We also edited the sentence, it now reads, “Even though the industry-wide mean P50 prediction bias is converging towards zero, the industry appears to overestimate or underpredict the AEP for many individual wind projects.”

Table 1: “remove windiness”

Response: We are keeping the term “windiness”, which is useful here because it is a commonly used term in the industry.

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2020-85, 2020.