

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

**Mark Kelly (Referee)**

mkel@dtu.dk

Received and published: 17 August 2020

### **General comments**

This paper reviews the framework of the emerging IEC-61400-15(-2) standard on uncertainty in wind farm assessment.

It also identifies a long-term reduction of bias in energy predictions (as an average across the industry), summarizing improvements which appear to explain such.

It aggregates an impressive collection of reports made by the industry for various (sub-)component uncertainties, though it is mostly based on projects in the United States.

C1

The work is somewhat qualitative, with a few quantitative issues; clear (explicit) definitions, and clear distinction between ‘uncertainty,’ ‘errors,’ and ‘losses’ are a bit lacking. There is a bit of ‘sloppiness’ in the quantitative sense. In at least one case, an imprecise conclusion (actually *incorrect* within the given phrasing) involving one of these aspects is given (l.342). This and similar issues are addressed in more detail within the **specific comments** section below.

It would also be useful to more clearly delineate between industry-wide (*ensemble*) bias, and uncertainty.

The tables are quite lengthy, and better formatting could likely make them half as long. Further, for the sake of brevity, it might be better to omit Appendix A (since the first two figures show the components), and leave these details to the 61400-15 reference—especially because some of the details are still pending potential ‘clean-up’ or modification/checking before going to IEC-draft.

Numerous corrections, suggestions, and comments are also included as annotations, in the attached PDF file. There are a number of English-usage issues; I have fixed some to be helpful, and leave the rest for the authors (to improve upon proof-reading the revised version).

The work is a potentially useful aggregation of industry practices and results, though it needs to be “cleaned up” a bit in order to be worthy of publication (in this reviewer’s professional opinion).

### **Specific comments**

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.

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

C2

e.g. 2-year rolling periods?

I.30–34: are you defining P50 in terms of a 2-year basis? If so, you should have done that in I.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.

I.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?

I.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.

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.

I.55: “financial impact” is also found in the other 2 bullet points (see annotated PDF).

I.96–97: What distribution (PDF) are you assuming, to estimate 95I.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?

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

I.101–3: Why do you evaluate the  $R^2$  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.*

I.104: do you “need to interpret a small subset”, or are your forced to to so?

I.156–7: you argued in the previous paragraph that the low  $R^2$  of the linear fit means most of the variability in bias is not described by the regression. Thus how can you

C3

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).

I.151: why is ‘typically’ included? Isn’t it just one standard deviation?

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

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

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?

I.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.

I.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.

I.355: I’d suggest “being reduced” over time, not “approaching zero”, because the uncertainty will not disappear—but rather decrease, as practices and reporting improve.

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

Table B1–headers: the values for ‘Wind Farm’ and ‘Wind Farm Year’ are not defined

C4

here in Appendix B.

### **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.

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

l.87–8

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’.

l.159: “of” should be “for”

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

l.305: “immersed” should be “inherent”.

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?

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

l.608: reference incomplete

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

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

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

C5

Please also note the supplement to this comment:

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

---

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

C6