Anonymous Referee #1

Dear reviewer,

We are grateful for your careful work and appreciate your detailed comments. Below you will find details how we will revise our manuscript based on your review.

With best regards,

Jennie Molinder (née Persson Söderman)

General comments: This article looked at applying two well-known probabilistic techniques from the atmospheric science community to the problem of production loss of wind turbines due to icing. The methods were applied on the NWP model simulations that were used as input to the icing model, which in turn is used as input to the power production model. The paper shows that a traditional model ensemble does not improve results when tested over two wind farms for a two week period, but that using forecasts from neighboring points does improve the forecast.

As discussed in the previous AC, both methods do improve the forecast skill. The referee also recognized this after our previous correspondence.

The article is well structured and the topic is of significant interest to the Wind Energy community. However, the methods are not described in enough detail for reproduction, and then observational data that is used is not described sufficiently nor of sufficient length for it to provide a good validation of the methods being tested.

Methods and observations will be described in more detail including

- HarmonEPS setup and initialisation

- The icing model formulation including the effect of different water components types and ice loss
- Table of observations with approximate location, mast or nacelle, observed parameter
- And more details according to your specific comments below.

We agree that the period is rather short, which is why we stressed the case study character of our work. The limitation resulted from the fact that global EPS data is required on the boundaries. These data are not operationally archived by ECMWF, only selected periods are available. We picked the most relevant period with available production observations. During this period, one of the three stations is containing two icing episodes and that, when combining all days and stations with icing, we have about 17 days of icing for validation. Given the scarcity of production observations and high computational costs of EPS runs, we still think that the results are robust for these meteorological conditions showing the possible benefit of using probabilistic forecasting for icing related production loss forecasts.

The extension of the dataset is not impossible, but not a simple task due to the need of global boundary data, high computational costs in running and storing the high-resolution ensemble and scarcity of icing and production observations. If regarded necessary, we are willing to find a solution.
We welcome guidance to this question by editor and reviewers.

Finally, the paper mentions that the cloud parameters LWC and MVD are key inputs to the icing model, but they are not analyzed in terms of the spread of the probabilistic approaches, which seems like an important metric to analyze.

A discussion about the mean and standard deviation for LWC and MVD will be added to the revised manuscript. Also addressing your specific comment below on the different water components, we will add a discussion on the impact of all water components and of the MVD and their spread to the results on the example of one icing event.

Specific comments:

1. Section 2.1.1: How does the HarmonEPS used in this study differ from the operational usage of SMHI?

1: The HarmonEPS currently used at SMHI are using a new model version cy40h1 and lagged ensemble members, but it should be noted that the operational version also includes physics perturbations. We will add a reference to the operational setup by Andrae et al. (2017, ALADIN-HIRLAM Newsletter No. 8, available from http://www.umr-cnrm.fr/aladin/meshtml/NL8-final.pdf).

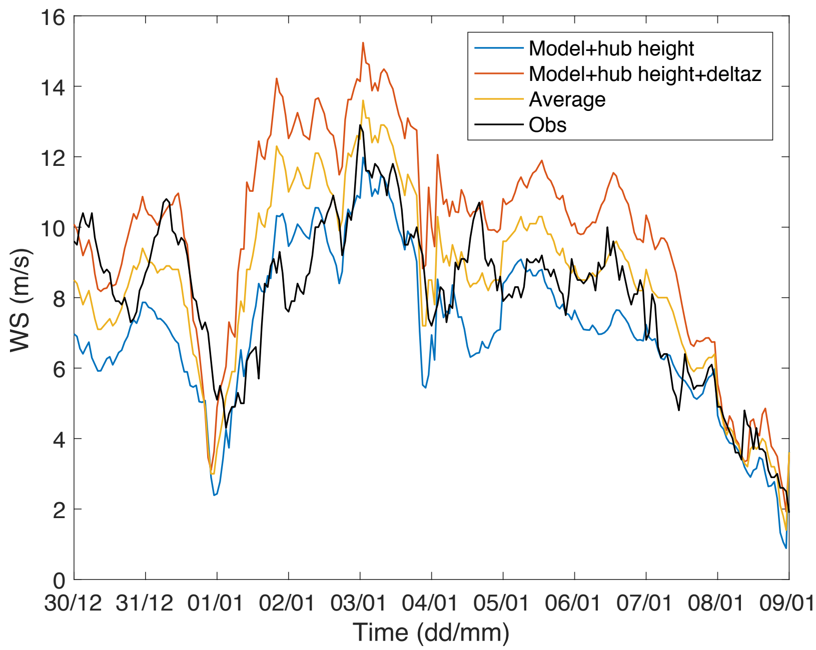
2. P4 L23-24: Do I read correctly that the models are all initialized using the ECMWF control member analysis, and only differ due to boundary conditions?

2: We understand that our description on the initial conditions has to be clarified. In fact, we use the so-called PERTANA option, where the initial fields are combining the HarmonEPS control analysis with the fields from the ECMWF EPS perturbed members. We will revise our description.

3. P5 L12: It would be interesting to see the impact of this averaging on the results. You state they are not here, but perhaps you could at least mention the improvement in error for the different variables in the text.

3: The vertical interpolation was introduced for an earlier study and not validated again for this study. The earlier vertical interpolation used adiabatic lifting, except for inversion where a linear interpolation was used. This caused jumps in wind speed etc. during regime changes. Thus, this more general vertical interpolation was introduced. Although undoubtedly interesting, we think that a more detailed discussion is beyond the scope of this article and plan to shorten this passage in order to keep the focus on the probabilistic forecasting.

For your interest, we add a plot with the wind on model level plus nacelle height, model level plus topographic correction plus nacelle height and our vertical correction. The RMSE is smallest for our vertical interpolation scheme.



4. P5 L12: What are all parameters, and which parameters was the lifting validated against?

4: All model parameters used in the icing model are corrected. We will add a clarification. No validation for this method is included here. We think that it could be done in a separate study.

5. P6 L31-32: You note that different forms of water are fed separately through the model, how exactly is this carried out? Are you just running the model 4 times and summing the results? It is unclear to me how that would impact the accretion efficiency, which is a heat balance that depends on the mass flux of water impacting the structure.

5: Yes, we are running the model several times and summing the results. For simplicity, we have treated all water components in the same way for alpha3. This assumption can be investigated. However, we think that it is not crucial for the main focus of the paper, namely the application of probablistic forecasting for wind power in cold climate. We will add a clarification on the calculation of alpha3 for the different water components.

6. P7 L2: Is the MVD only calculated for cloud water?

6: The MVD is calculated for each of the water components since this is a necessary input to the icing model. In a revised version of the paper we will describe this more clearly.

7. P7 L4: What is the empirical ice shedding model?

7: The ice shedding is basically a constant multiplied with the melting equation; this will be described better in a revised version of the paper.

8. P7 L15: In not familiar with the term effect curve, is this just the power curve?

8: Yes, we are sorry for using the direct Swedish translation. This will be corrected.

9. P11 L26: You mention that there could be an issue with warm turbines, but you mentioned that you have a mixture of mast and nacelle data. Did you investigate if the bias was different between the two sources?

9: The bias was not yet investigated in detail. We will examine the bias for mast and nacelle data separately and add a remark to the manuscript.

10. P12 L10: could you list the % improvement of the wind speed and RH?

10: Yes, the improvements for wind speed and RH is 7 and 12% respectively using ENSngb. This will be added in the article.

11. P13 L9: The shift from 10 sites to 3 was confusing when reading this paper, perhaps remind the reader that you only had production data at 4 sites, and had to discard one. I am not sure why you only looked at two of the three sites here though.

11: Thank you for the comment; we will describe this more clearly in a revised version of the paper.

12. Section 3.2: You discuss quite often the LWC values and how they impact the ice growth. It is unclear to me what this parameter is, since your model includes two types of liquid water hydrometeors and two solid hydrometeors. Could you describe what it is, and if it doesn't already make sure it includes all of the relevant hydrometeors.

12: The LWC includes in this section refers to the liquid water content and not the solid parts. In our icing model, liquid water is a necessary condition for ice formation.

As you point out, it would be valuable to include all hydrometeors in the discussion. In a revised version of the manuscript we will, also according to your general comment above, add some discussion about the water components and related spread.

13. Section 3.2: In the experiment period and available data section, you mention that you have production data from each turbine at each site. It is unclear how you aggregated the production data to get a singular value. Can you state approximately how many turbines were at each of the two sites you used?

13. Yes, we will describe the aggregation of the production data and give an approximate number of wind turbines together with more detailed description of the sites in a revised manuscript.

14. While it is understandable that you cannot list the wind farms themselves, can you at least describe how far apart they are, and if they are exposed to similar weather patterns.

14. We will add a characterisation and approximate location for each wind farm.

15. Fig 6. It is hard to see the observations clearly, perhaps you could use a color like red that would stand out more.

15: Thank you, we will make the figures clearer.

16. Figure 9: Why was there no ice during the beginning of the period, even though there was still a fairly large amount of LWC?

16: Thank you for this observation! A quick analysis of the relevant meteorological parameters suggest this is because of very low wind speeds around the 29/12. We will examine this episode more carefully and add a comment about this in the revised manuscript.

17. Table 1: What is the mean RMSE the mean of?

17: It is the mean of the three sites. We will clarify this.