

Review of the article “A new base of wind turbine noise measurement data and its application for a systematic validation of sound propagation models”

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1 Specific comments

1.1 Source modeling

You mention page 4 that you follow the approach of Barlas et al. (2017b) to represent the wind turbine as an acoustic source. You indeed consider 3 source heights located at $h_s = h - 0.85l$, $h_s = 0$ and $h_s = h + 0.85l$, with h the hub height and l the rotor length, as in the steady case considered by Barlas et al. (2017b). You then write line 125 that “simulation results are logarithmically summed (Barlas et al., 2017b)”. When I read the article of Barlas et al. (2017b) I don’t understand that they summed the simulation results for the 3 source heights. Indeed Figure 9 of this paper shows the influence of the source height on this predicted sound pressure levels, but no mention is made of a logarithmic sum except over frequency bands in Equation (2). For me this way of calculating the sound pressure level L_p and then the propagation loss ΔL_p has not been done before, and deserves a to be discussed in details.

Following Equation (1), the sound pressure level is calculated as:

$$L_p(f) = L_W(f) - 10 \log(4\pi R^2) - \alpha_L(f)R + \Delta L(f). \quad (1)$$

Although not clearly stated in the article, this model is valid for one point source (monopole). If the wind turbine is modeled with one point source, it is straightforward to calculate the propagation loss following Equation (12):

$$\Delta L_p = L_{p,1} - L_{p,m} = -10 \log\left(\frac{R_1}{R_m}\right) - \alpha_L(R_1 - R_m) + \Delta L_1 - \Delta L_m. \quad (2)$$

Note that it is not needed to assume that the sound power level L_W is equal to zero as you do on page 10. The term L_W is canceled when calculating the propagation loss.

Now if you model the wind turbine with 3 source heights (or more), Equation (1) becomes:

$$L_p^n(f) = L_W^n(f) - 10 \log(4\pi(R^n)^2) - \alpha_L(f)R^n + \Delta L^n(f), \quad n = 1..3, \quad (3)$$

where the subscript n refers to the point source number. Then if you sum logarithmically the contributions from the 3 point sources, we obtain:

$$L_p(f) = 10 \log_{10} \left(10^{L_p^1/10} + 10^{L_p^2/10} + 10^{L_p^3/10} \right). \quad (4)$$

The propagation loss is now given by:

$$\begin{aligned} \Delta L_p &= 10 \log_{10} \left(10^{L_{p,1}^1/10} + 10^{L_{p,1}^2/10} + 10^{L_{p,1}^3/10} \right) - 10 \log_{10} \left(10^{L_{p,m}^1/10} + 10^{L_{p,m}^2/10} + 10^{L_{p,m}^3/10} \right) \\ &= 10 \log_{10} \left(\frac{10^{L_{p,1}^1/10} + 10^{L_{p,1}^2/10} + 10^{L_{p,1}^3/10}}{10^{L_{p,m}^1/10} + 10^{L_{p,m}^2/10} + 10^{L_{p,m}^3/10}} \right) \end{aligned}$$

If the values of the sound power level L_W^n for the 3 point sources is unknown, it does not seem possible to simplify this expression. Your solution is to set $L_W^n = 0$, but this is not physically valid. I think your approximation consists in reality in distributing equally the sound power level among the 3 point sources: $L_W^n = L_W/3$. In this case the sound power level terms cancel when the propagation loss is calculated.

Now is this approximation valid? It is difficult to answer this question without comparing the simplified model to a reference solution involving a real blade in rotation. I nevertheless feel that giving an equal power to each point source may yield too much strength to the top and bottom point sources, as one blade spends twice as much time close to the hub than at each extremity. Furthermore, other effects may change the source distribution over height such as the effects of directivity and convective amplification [1], or the effect of inhomogeneous inflow due for instance to wind shear [2].

1.2 Description of measurement setup and data processing

The description of the experimental setup in Section 2.2 raises several issues:

- Figure 1 shows that the wind farm where the noise is measured is composed of 6 wind turbines. You are interested in the noise of a specific wind turbine located on the southern part of the farm, but Microphone 2 is close to another wind turbine, and microphone 3 seems relatively close to yet another wind turbine. Does it mean that you have selected periods where only one wind turbine in the park is on?
- In Figure 2, you write that the hub height is 93 m and the rotor diameter is 114 m. However in line 254, you write that the hub height is 119 m and the rotor diameter is 114 m. Finally in the readme.pdf that is provided along with the data on the project homepage, the hub height is 114 m and the rotor diameter is 93 m! Please correct these differences...
- In order to measure temperature, wind speed and wind direction at different heights, you use the data of a meteorological mast located a few kilometers away from the wind turbine.
 - Since you have access to SCADA recordings, have you compared the wind speed and direction on the wind turbine hub with the ones measured by the anemometer on the meteo mast. It would allow you to check the spatial homogeneity of atmospheric quantities in this specific wind farm.
 - What is the resolution of the temperature sensors you use? If this is 1 Hz as for the wind speed (line 170) it is likely to be insufficient to characterize turbulent fluctuations (see subsection below on atmospheric turbulence).
- Page 15, you mention that background noise can change in the course of the measurement campaign depending on the vegetation. What is the duration of the measurement campaign? Could you add the date and hour of each case in Table 3? This would also be a valuable information regarding the change in ground impedance (see subsection below).

Furthermore, there are some lack of details on the data processing that you perform:

- You calculate the effective sound speed in Equation (8) using $c_{\text{eff}} = c_0 - |u| \cos \gamma$. If γ is the angle between the wind direction and the sound propagation direction, with $\gamma = 0$ corresponding to downwind conditions, the expression should be $c_{\text{eff}} = c_0 + |u| \cos \gamma$.

- You explain line 234 that a sampling rate of 5 Hz is used within the 1/3 octave bands. Is it true only for the measured data? Do you calculate the numerical SPL only at the center frequencies of the 1/3 octave band or you consider several frequencies per 1/3 octave band?
- In Figure 4(a) the values of wind speed cannot be read. Also, why don't you plot values at heights closer to the hub (such as 100 m for wind speed and 95 m for wind direction)? They would be more representative of what the average inflow impinging on the wind turbine.
- Figure 5 does not provide any additional information compared to Figure 4(b) and can be removed. The values of RMSE can simply be given in the text.

1.3 Characterization of atmospheric turbulence

To characterize the turbulent fields of temperature and wind speed, you use a Gaussian correlation function with a correlation length of 1 m and a variance of index of refraction fluctuations μ^2 . This variance is related to the variance of the temperature and wind speed turbulent fluctuations, as given by Equation (6) of your paper.

To study the scattering of acoustic waves by turbulent structures, it is however necessary to measure temperature and wind speed fluctuations with a sampling frequency of at least 20 Hz (with an ultrasonic anemometer for instance). Indeed, the structures of interest are generally located in the inertial subrange (see Figures 1 and 3 of Wilson et al. [3]). If you use a sampling frequency of 1 Hz, you will only capture turbulent structures in the energy subrange, as can be seen for instance in Figure 8 of Daigle et al. [4].

Figure 8 of Daigle et al. [4] also clearly shows that the Gaussian model is not adapted to capture the spectral slope in the inertial subrange. At best, it can fit the spectrum over a small range of turbulence wavenumbers, as explained in Wilson et al. [3], but the von Kármán model is generally recommended.

Thus, I disagree with your sentence lines 518-519, where you conclude that “the available meteorological data, i.e. the wind speed and temperature fluctuations, are sufficient for the determination of atmospheric turbulence”. I think that the experimental estimates of μ^2 cannot be used to calculate the turbulence spectrum. The good agreement that you obtain in Figure 11 at 845 m with turbulence above 1000 Hz may not be meaningful. Indeed the SPL values are very low in the shadow zone, and corresponding signal-to-noise ratio is very low, as can be seen in Figure 9. Thus the difference between the model predictions without turbulence and the measurements above 1000 Hz is likely not be due (at least partly) to background noise.

1.4 Ground effect

You show clearly in Figure 15 that the flow resistivity used to calculate the ground impedance in the Delany-Bazley-Miki model has a significant impact on the propagation loss. From Figure 10 and Figure A.1, it appears that the modeled and measured third octave band spectra are in good agreement at 535 m, but not at 845 m, with differences greater than 5 dB between 250 Hz and 500 Hz approximately. The main reason for the discrepancies between model predictions and measurements is likely to be the ground effect, as refraction effects cannot explain the differences (small vertical sound speed gradient for cases 3/4 and cases 5/6).

You mention that ground properties may have changed between the various cases lines 389-394, due for instance to the soil humidity. It has indeed been shown in the literature that the flow resistivity can vary drastically if the soil is dry or wet. In Figure 15 you have

tested various flow resistivities at 535 m. Could you perform the same tests at 845 m where the agreement is less good, to test if a change in flow resistivity could explain the differences between model predictions and measurements?

Also, you show in Figure 14 that model predictions with a single point source yield strong interference minima and maxima, that are smoothed out when you consider your wind turbine model with 3 point sources, as shown by various researchers [5,6]. However at 845 m the interference minima and maxima associated with ground effect seem too strong in the model predictions compared to the measurements. This could mean that 3 point sources is not enough to calculate the propagation loss. Could you test with a greater number of point sources distributed over the rotor height?

1.5 Section 3.2.2 on total SPL

In section 3.2.2, you discuss in length the agreement between measured and modeled propagation losses averaged over all frequencies. It seems to me that this is a bad indicator to check the validity of the model. For instance for case 4 at 845 m, you obtain a small difference of 0.15 dB, while the spectra in Figure 10 differ significantly. What happens is that the overestimation at low frequencies is compensated by the underestimation at high frequencies. This is even more problematic when you write lines 441-442: “Being 0.92 dB, the averaged absolute difference of the losses is also below 1 dB, meaning that the model generally predicts the propagation losses well also at longer distances.” This averaging makes no sense to me! If you want to quantify the validity of your model predictions using a single number, my suggestion would be to calculate the mean over frequencies of the absolute value of the differences (or something similar):

$$\text{Mean Difference} = \frac{1}{N} \sum_i^N |\Delta L_{p,i}(\text{model}) - \Delta L_{p,i}(\text{measurement})|. \quad (5)$$

1.6 Section 3.3.1 on model assumptions and limitations

In the discussion section 3.3.1, you insist on two assumptions that have a minor influence on the model predictions in my opinion:

1. 2D approach: based on the article of Cheng et al. (2019), you explain the differences between model predictions and measurements crosswind by the fact that azimuthal refraction is neglected. In the study of Cheng et al., some differences can indeed be seen in their figures 10 and 11 in the cross-wind direction between 2D and 3D approaches (5 dB in the worst case for a source close to the ground). However this is for an unrealistic wind speed profile; the wind speed is greater than 40 m/s above a height of 70 m! For more realistic wind speed profiles, Salomons (2001) has shown in Figures 4.14 and 4.15 that the 2D approximation is valid with a good accuracy.
2. Vertical components of wind speed: you write lines 470-471 that “According to the literature, the consideration of vertical wind components is recommended when examining high sources such as wind turbines.” It would be good to cite relevant articles to show that this effect is important, as neglecting the vertical components of wind speed is generally considered as a good approximation for outdoor noise propagation.
3. Wake effect: it has been shown that sound propagation through a wind turbine wake can have a significant effect on the SPL, however this is limited to downwind configurations. As you don’t consider angles between -30° and 30° , wake effect is unlikely to be important in your study.

In my opinion, the discussion section should focus on the main issues that are:

1. the source model (see above subsection);
2. the ground characteristics that are not measured and can only be found by comparing model predictions and measurements (see above subsection);
3. the characterization of atmospheric turbulence (see above subsection);
4. the possible changes of temperature and wind speed profiles with distance (the meteorological mast being relatively far from the wind turbine under study).

The conclusion should be changed accordingly, for instance in lines 543-544: “Discrepancies in the 1/3 octave spectrum might be eliminated by a distance-dependent implementation of ground parameters. Also the consideration of wake effects is reasonable.”

2 Technical corrections

Here is a list of suggested corrections:

1. abstract line 10: “1/3 octave spectra” \Rightarrow “third octave band spectra”
2. Introduction line 20: the article of Bérengier et al. is about outdoor noise propagation models in general, there is no application to wind turbine noise.
3. Introduction line 32: the conference paper by Shen et al. was published in 2020, not in 2005.
4. Introduction lines 38-40: when the difference between measurements and model predictions are between -3.4 and 2.5 dB, I don’t think it can be concluded that the general agreement is good. Furthermore, from what I understand from Shen et al. (2020) the comparison is not done exactly crosswind, as they write in Section 2.3 that the wind directions are 269 to 279 degrees while the microphone line direction is 225 degrees (44 to 54 degrees respective to downwind).
5. Introduction lines 46-47: “Moreover, with a hub height of 60 m, the investigated wind turbine does not correspond to the current scales of up to 165 m”. It is uncommon to encounter onshore wind turbines with a hub height of 165 m, It is typically between 80 m and 120 m.
6. Introduction lines 51: “measurement mast” \Rightarrow “meteorological mast”
7. Introduction lines 53-59: when you discuss the results of Sonbdergaard and Plovsing (2009), I think it is not sufficient to provide the average deviation between model predictions and measurements. For the validation cases with a single wind turbine, the differences are between -3.8 dB and 1.3 dB, which corresponds to an average deviation of -1.0 dB and a standard deviation of 2.3 dB. I suggest you add the standard deviation or that you give the extreme values.
8. Equation (1) page 3: the term associated to atmospheric absorption is usually written $-\alpha_L R$ and not $-\alpha_L R/1000$. See Equation (3.8) of Salomons (2001).
9. Line 116 page 4: I think the coefficients A_0 , A_2 and B in Equation (3) have been obtained by an optimization procedure based on an analytical solution. Thus they are not obtained from empirical studies.

10. line 169 page 6: you write "having a resolution of ten minutes". I think you mean that the data is averaged over ten minutes, but the data resolution is lower.
11. line 179 page 7: I would replace "characterizing the turbulences" by "characterizing the turbulent fluctuations".
12. Lines 308-309 and Figure 7(b): it would be easier to read values of the absorption coefficient in dB/100m or dB/km.
13. Line 319 page 15: I think "However" should be removed.

References

- [1] S. Oerlemans, P. Sijtsma and B. Mndez Lpez, Effect of wind shear on amplitude modulation of wind turbine noise , *Journal of Sound and Vibration*, vol. 299, pp. 869–883, 2007.
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