Review of article titled ”Numerical model for noise reduction of small vertical-axis wind turbines (WES-2023-54)”

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The topic addressed by the authors is relevant to the journal in terms of wind turbines and noise reduction strategies, but the work and the methodology has several strong short comings and lack of information which prevent me from accepting this manuscript in its current form and requires several major revisions. Please find attached the main general comments of concern, followed by specific comments.

Main general comments

1. The grid independence study has not been properly conducted, by increasing / decreasing the grid size over the entire domain, but parts of the domain have been selectively modified to justify grid independence (Table 1). The size of the mesh near wall region plays a key role in the predictions and this has not been changed (kept constant at 0.003 m) between Mesh 3 and Mesh 4, and the changes made in the mesh sizes is moreover negligible. Also importantly, a convergence in time (the number of revolutions, the time taken for the initial transient and convergence in forces) and the convergence with choice of time-step has not been shown. The mesh sizes could be reported based on number of points across the blade and the maximum surface y^+ over the revolution of the blade. Hence the effects of the mesh has not been addressed in a consistent manner.

2. The methodology (Section 2) is incomplete, with several variables and constants undefined, which makes it difficult to understand the model and choice. The constants used for the different turbulence models is missing. Several figures and numbers in the text have no units defined, and a consistent comparison has not been made eg. in some places the noise change is reported in dB, while others only a change in acoustic power (in W) is reported. And the methodology to compute the torque, angle of attack etc are not described.

3. No validation of either torque and/or noise measurements has been performed with any experimental/field measurement or other numerical data, which makes it difficult to have confidence in the simulation model. This along with the lack of a proper grid independence study makes the comparison for using the model for a study on different noise reduction methods (mask, deflectors, wall roughness) not reliable.

4. The CFD flow solution has been predicted using an in-compressible code (no variation of density and propagation of sound waves in the domain), relevant for only prediction of forces over the blade and a tonal noise prediction. The aeroacoustic formulation used for the study does not provide sound sources in the frequency spectrum and consider the Doppler shift that takes place over the revolution of the turbine. In this context, the entire discussion of noise and the acoustic power using turbulent kinetic energy does not seem appropriate as the acoustics are not captured by the simulated CFD model. Ideally an FW-H approach could to be performed using a converged blade loading statistics to estimate the tonal components, or switching to a compressible flow solver/methodology to capture the entire acoustic spectrum.

And an entire acoustic spectrum would be required to identify the different noise sources and their differences for then simulating different noise reduction methods.

5. The Introduction (Section 1) does not appear to be thorough and complete about the most recent literature related to noise radiated by vertical axis wind turbines, and contains too many references to work not directly related to the present study (vertical axis wind turbine noise and noise reduction strategies). Moreover the literature presented does not highlight the methodology and important simulation details to draw a contrast and highlight the novelty of the present work.

6. The conclusion about the $k - \omega$ SST turbulence model showing a better prediction for the flow field is not novel (Line 359), as it has been used significantly in published literature for vertical axis turbines. Also the noise reduction has been reported in percentages but not in dB in conclusions. Moreover, the deflector suitable noise reduction has not been been shown for all operating conditions of the turbine (only a single velocity ratio has been simulated), hence it is not a conclusive (Line 365) justification to use it as a noise reduction approach.

Specific comments

1. Line 32: 95 dB sound pressure level (SPL). Which kind of turbine (horizontal axis or vertical axis) does it correspond to ?

2. Line 35: Rephrase, "noise by air"

3. Line 39: "acoustic analogy theory" : This statement does not seem completely correct. There is the direct noise prediction (by performing a direct numerical simulation or large eddy simulation) or indirect noise approach by separating the noise source calculation, and noise propagation. And the acoustic analogy is one of the ways for noise propagation (eg. other approaches such as finite element, boundary element, or low order approaches).

4. Line 40: Inconsistent use of sound pressure / sound pressure level (SPL), I suggest to define and use the abbreviation

5. Line 43: ANSYS CFX version ? Which method (RANS or LES) ?

6. Line 44: Relevance of NREL Phase VI blade ? Is it a VAWT ?

7. Line 49: Tip noise is the dominant source ? Is it true for VAWTs ? Justification/citation to relevant literature required

8. Line 51: Maizi.et al (2018) used a 2D or 3D approach ?

9. Line 57: The term "velocity ratio' is not defined

10. Line 61: What is meant by spacing ? Does it refer to blade solidity ? Any justification given in the paper for "excessively small or large spacing increasing noise emissions ?

11. Line 62: The review of Botha (2017) is vague regarding the analytical model falling short ? Which noise generation mechanism was modelled ?

12. Line 69: Naccache et. al (2018) , appears to be irrelevant to the present study, since it is a D-VAWT and not a standard VAWT that has been investigated in the present study.

13. Line 77: Ideally always method the methodology used in a flow solver (RANS/LES/DNS etc) and if the simulation is 2D or 3D.

14. Line 79: Nyborg et. al (2018) the use of higher -fidelity sound propagation model is vague. Is it a empirical model ? Is it relevant to the present study on VAWTs ?

15. Line 92: Not true, there have been several studies which are missing from the literature review such as for example: https://www.mdpi.com/1996-1073/13/16/4148,
https://journals.sagepub.com/doi/10.1260/1475-472X.14.5-6.883,
https://arc.aiaa.org/doi/10.2514/6.2022-3058

16. Line 102: ANSYS FLUENT has to be mentioned unsteady RANS (uRANS) approach is being used, incompressible flow can capture only tonal components (unsteady blade loading). For turbulence interaction noise, no propagation of sound waves since the density is constant. , the numerical schemes and methodology is too dissipative.

17. Line 116: The relevant research related to the 2 turbulence models that have been extensively used for VAWTs has not been cited.

18. Line 120: The constants have not been defined. Please define all the constants and the values for the constants in a Table for both the turbulence models.

19. Line 138: ϵ has not been defined

20. Line 145: Relevance of the used methodology/formulation for rotating machines ? Has it been used in published literature for rotating machines and also specifically for VAWTs ? The variation of noise sources over the revolution of the blade is not accounted for.

21. Line 151: What is the correlation area ? How is it defined ? A diagram could be useful.

22. Line 157: No end plates or supporting structures have been included in the model. They could alter in the predicted blade loading and noise characteristics, in case a practical noise reduction methodologies are required. https://doi.org/10.1108/HFF-09-2022-0562

23. Line 183: How accurate are first-order schemes ? The turbulent kinetic energy could be too dissipative to have any meaningful prediction for the computed noise (the methodology which itself is questionable for use for VAWT noise prediction).

24. Line 184: Influence of time step size has not been reported ? How is this time step choice justified ? Is it based on a CFL number / any relevant flow physics ?

25. Line 210: Refer main general comment 1, also the number of points across the blade and the maximum wall y^+ could be reported in the table. The mesh sizes have not been changed in a consistent way.

26. Line 221: The methodology used to compute the torque is not reported. Are the forces on the blades summed up, and multiplied with radius and the rotational speed ?

27. Line 224: Mesh independence study has not been performed in consistent way, by changing the mesh size over the entire domain , refer main general comment 1.

28. Line 230: Time-averaged torque of 227.7 , unit undefined.

29. Line 240: Figure 8 : unit undefined for Torque , unit for acoustic power shown in W, but the differences reported in dB in text.

30. Line 245: Figure 3, how many revolutions of the turbine have been simulated ? Has convergence been achieved in time. How long does it take for the initial transient ?

31. Line 256: How were the angles of attack calculated ? Was the flow velocity sampled at a point ?

32. Line 296: Deflectors could be similar to trailing edge serrations used for noise reductions. Agree that they help energize vortices close to the trailing edge and reduce extent of separation.

33. Line 320: No units on legends for Pressure, also the azimuthal angle (positions of the blades over the revolution) needs to be added.

34. Line 336: $u*$ is not defined

35. Line 351: The higher wall roughness could increase the wall pressure fluctuations which increase the noise. But here the increase/change has not been reported in dB.

36. Line 355: "visualization effect of the distribution" - unclear, please rephrase.

37. Line 358: Refer main general comment 6