Review of manuscript: wes-2025-42
Title: Impact of atmospheric turbulence on performance and loads of wind turbines: Knowledge gaps and research challenges
Authors: Kosovic et al

Overall comments:

The submitted paper reviews the fundamentals of atmospheric flow from global to microscale, and then zooms in on mesoscale and microscale turbulence and their impact on wind turbines. As turbines continue to grow in size and deployment, this review is timely and important to highlight the need to revisit commonly used assumptions/simplifications. Overall, the paper is well-written and is an especially impressive effort coordinating many authors. There are several areas that can be improved with further integration across the paper, I've noted a few below, where the text repeats or contradicts itself. I hope the authors may consider these comments in a revision.

General comments:

1. The authors have made a choice to focus the review on ABL effects on wind turbines, rather than wind farms. This is a reasonable choice to keep the paper's scope constrained, but wind turbines are nearly always placed in wind farms, where wake and array level effects will both depend on the ABL in interesting/complex ways and also will change many aspects discussed (i.e. in large wind farms, the effect of ABL turbulence on the loads of the leading row described extensively in Sections 7 and 8 could matter less than the interaction between the ABL flow and wakes/farm-scale effects that will dictate the performance of downwind turbines, of which there are many more than there are leading row turbines usually). My specific suggestion would be to confront the scope of the paper in the introduction and conclusions/recommendations to highlight this focus on wind turbines rather than arrays.

2. There is some inconsistency in the degree to which topics are introduced in a simple way through text description versus quantitative measures. For example, first order statistics, shear, and TKE are described completely, whereas Reynolds decomposition, integral length scale, energy spectra, etc. are not as clearly introduced quantitatively. Please consider making the technical descriptions more uniform.

3. There seems to be no substantial discussion of the boundary layer height, which plays a critical role for wind farms, aside from a limited discussion in conjunction with gravity waves in Section 7.3.3. The boundary layer height may also play an increasing role for individual turbines (focus of this review) as well, given the growing size of turbines mentioned many times, and the potential operation in shallow marine/stable boundary layers.

Point comments:

1. Line 14: In paragraph 1, the framing describes that wind resource is typically assessed using 10 min averaged hub height wind speed. Then commentary is made regarding turbulence timescales. I also thought this would be a good place to mention ABL shear (mentioned in abstract).

2. Line 22: "Turbulence affects the efficiency of wind turbine power generation resulting in fluctuating power output."
   This sentence is true, but might be misleading as written. Although not defined explicitly yet, we typically understand turbine 'efficiency' as the coefficient of power of the turbine. The primary way turbulence affects fluctuating power output is by changing the magnitude of the wind speed. The coefficient of power (efficiency) can also be affected by turbulence (e.g. [1, 2]) but this will usually be a much smaller impact than the effect of fluctuating wind speeds.

3. Line 23: "It also shortens their lifespan by inducing dynamic loads"
   References are needed for such a sweeping (and impactful) statement.

4. Line 59: "Rossby waves are a consequence of Earth's rotation (Rossby and Collaborators, 1985; Platzman, 1968), are embedded within global circulations."
   Typographical error

5. Line 100: "The diurnal cycle is more pronounced over land than over water."
   While generally true, diurnal cycles can be significant in coastal environments.

6. Line 122: consider defining barotropic/baroclinic

7. Line 141: The authors could consider first introducing the flux Richardson number, which has a justified derivation from the TKE budget and is therefore a robust measure of stability, before introducing the gradient Richardson number which is its approximate form that is more practically useful. Then, more quantitative statements could be made than this: "It is generally accepted that the boundary layer flow is quasi-laminar when Rig exceeds unity."

8. Figure 5: The roughness sublayer, surface layer, and outer layers appear to not be defined in text or in the caption.

9. Line 230: "There is a lack of measurement of the different ILS components."
   Unclear what this sentence means, consider rephrasing

10. Line 290: "In addition to the characterization of ABL flows motivated by wind energy, we add here a more mathematical discussion to explain the statistical content of the characterization."
    I did not follow what is meant exactly by this sentence (and therefore the motivation of the section). Consider rephrasing. More generally, this subsection contains important content but is

written at a more advanced level than the earlier parts of the paper.

11. Line 335: Typographical error

12. Section 4.1: The authors may add discussion regarding the quantitative identification of LLJs [e.g. 3]

13. Section 4.4 and Section 5 have duplicated content on flow over complex terrain

14. Line 465: I am unclear what the authors mean when they say that computing turbulent fluxes requires Taylor's hypothesis

15. Line 567: "Ideally, assuming a steady laminar flow, the power produced in this region is given theoretically by:" -> "Ideally, assuming a steady laminar **uniform** flow, the power produced in this region is given theoretically by:"

16. Line 592: Of relevance: recent evidence suggests the rotor equivalent wind speed model does not fully capture the effects of the wind profile shape (i.e. wind shear and veer), [4. 5]

17. Section 7.1: Given the focus of this review paper on turbulence, and the discussion of wind tunnel tests, the authors should consider confronting the issue of dynamic similarity, especially Reynolds number, and how that affects the interpretation of wind tunnel tests [6, 7]

18. Section 7.3.1: Motivation is given via Great Plains, but LLJs can also be quite important in coastal regions for offshore wind [8]

19. Line 725: This discussion of the scale of wind turbines and farms could be relocated to the introduction

20. Line 765: Reference formatting

21. Line 923: Typographical error

22. Section 8.1 is comprehensive and well-written.
Related to the discussion in paragraph starting at Line 935: There has been recent discussion as to whether failure rates may be increasing, and high profile failure events in the past several years are gaining more attention. The authors may consider a brief summary of knowledge gaps that could be related to these failures. Growing turbine size and veer are mentioned already. Aeroelasticity and the coupling between SIV/VIV and anisotropic/intermittent ABL turbulence are pertinent.

23. Line 974: Reference formatting

24. Line 981: More recent and relevant publication [9]

25. Line 981: “Furthermore, they state that turbulence length scales are smaller in complex terrain, which is why turbine fatigue loads tend to be higher in complex terrain.”
    I imagine it would be challenging to make such a general statement, especially in light of the discussion earlier on AGW and the lack of a unified standard on what is “complex terrain”

26. Line 992: “Englberger et al. (2020) used LES to study what controls downwind wake deflection and found the blade rotation when combined with directional shear (wind veer or backing) result in a significant wake deflection.”
    This is a good reference but seems out of place in a section about fatigue loads. There are many more studies on how ABL phenomena and turbine operation (shear, stability, Coriolis effects, yaw, …) affect wakes in general, beyond blade rotation+shear. More generally, this review does not describe wakes/farm scale processes, so this reference is somewhat isolated.

27. Figure 15: Unclear what Steps 1, 2, and 3 are

28. Line 1030: This paragraph has high overlap with the previous section on ABL turbulence models

29. Section 8.3: Relevant to the discussion on 1059, recent LES indicates that wake added turbulence depends on ABL stability [10] which is not well addressed by existing empirical models

30. Line 1089: “however, the disadvantage is that the high Reynolds number characteristic for atmospheric flows is not possible to achieve”
    This statement is not strictly correct, as demonstrated in Refs. [6, 7]. Also, shear and stability is possible to achieve [e.g. 11, 12], but veer, LLJs, and AGWs are certainly more challenging.

31. Line 1123: “Turbine incidents and failures are underreported due to legal and other proprietary considerations.”
    Strong statement that may be true, but would require references/proof to include in this paper

32. Line 1151: The sentence is incomplete/cutoff

References

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