Review of the manuscript wes-2025-42, "Impact of atmospheric turbulence on performance and loads of wind turbines: Knowledge gaps and research challenges", by B. Kosovic, S. Basu, J. Berg, L.K. Berg, S.E. Haupt, X.G. Larsen, J. Peinke, R.J.A.M. Stevens, P. Veers, S. Watson.

This manuscript provides an extensive review of the role of the atmosphere in power capture and loads of wind turbines. This manuscript includes essential information for a general reader to be informed about the main phenomena associated with this wind energy topic, and it can be a valuable resource for our research community as well. These are my main comments on this manuscript:

1. Each section tackles, in more or less depth, a specific related topic by providing a summary of the associated recent literature. Finally, a very brief outlook on the related research is provided in the final section 8.6. I would propose to restructure each section including a summary of the recent research achievements (rather than listing the executed works), then illustrating the current research gaps, and the research projects/tasks needed to address those gaps. This writing approach is sometimes outlined in a few sections, but generally not implemented in most of the sections.
2. Some effort should be made to homogenize this extensive manuscript. Currently, it reads as a collection of various drafts written by different authors with different writing styles connected by their content. As I understand this was a necessary strategy to work on such an extensive manuscript, at the same time, I believe an extra effort should be made to homogenize the writing, avoid potential overlaps, and cross-reference different sections when possible.
3. The manuscript is very extensive and, sometimes, some discussions are rather shallow and could be omitted (see details below). I would suggest revising critically the manuscript to identify those sections/parts that can be removed, merged, or shortened without omitting important information for the reader.

Some detailed comments are reported in the following.

Comments:

1. L57 The turbulent motions….To my knowledge (e.g. PERRY, A.E. & MARUSIC, I. 1995 A wall-wake model for the turbulence structure of boundary layers. Part 1. Extension of the attached eddy hypothesis. J. Fluid Mech. 298, 361–388.; HÖGSTRÖM,U., HUNT, J.C.R. & SMEDMAN, A.S. 2002 Theory and measurements for turbulence spectra and variances in the atmospheric neutral surface layer. Boundary-Layer Meteorol. 103 (1), 101–124; Van der Hoven (1957) for the spectral gap) turbulent motions have a specific spectral footprint and are restricted to scales smaller than those belonging to the mesoscale range. I think you should replace the adjective turbulent with atmospheric.
2. L79 – You can merge it with the previous paragraph.
3. L108 – Check for typos.
4. L192-194 – Specify the criterion used in Kelley et al. (2006) to identify neutral conditions.
5. Sect. 3.9 is very disconnected from the rest of the discussion. Maybe it can be removed.
6. L464 – Please add that sonic anemometers typically measure virtual temperature as well. This physical parameter is leveraged for the estimation of the friction velocity and Obukhov length through the eddy-covariance method.

7. L559 – Add here the reference to the IEC standards, which is provided at L 585, instead.
8. L655 – Provide details on the Langevin equation.
9. L656 – Fix references.
10. L721 – Wind Energy, no need for capital letters.
11. Sect. 8.2.2 does not provide a clear explanation of the phenomenon described. I would suggest removing it.
12. Similarly for Sect. 8.3. The discussion is very generic and no critical information is provided.