Title: Influence of simple terrain on the spatial variability of a low-level jet and wind farm performance in the AWAKEN field campaign
Author(s): William Radünz et al.
MS No.: wes-2024-166
MS type: Research article

This paper focuses on a strong low-level jet (LLJ) event under stable conditions during the AWAKEN campaign, investigating how simple terrain modulates flow and wake fields. Specifically, it examines how terrain-induced effects contribute to spatial variability in the flow field. For the simulation, the authors employ a multiscale modeling framework (WRF-LES-GAD) alongside available SCADA data. The study presents interesting findings with a valuable contribution to the field. Publication is recommended after addressing the comments provided for further clarity and accuracy.

General comments

I appreciate the authors' effort in integrating several interesting concepts into the manuscript. Given the space constraints, some of my comments stem from the manuscript's structure and the concepts addressed. For instance, while the study provides insights into the specific LLJ event analyzed, it does not sufficiently elaborate on its generation mechanism.

The paper presents several interesting findings and interpretations, particularly regarding the cross-sectional plot of KHI in Fig. 7, which effectively illustrates the flow field. However, I was expecting further discussion or analysis on how the observed instability may or may not influence the flow field or more specifically wind streamlines both above and within the wind park. Given that such instabilities can locally impact turbulence, wake dynamics, and momentum transfer, it would be important to explore whether KHI explicitly or implicitly plays a role in shaping the flow field. Including this discussion could strengthen the interpretation of the results and enhance their relevance to wind farm performance.

There are several interesting aspects in the discussion section. However, I think some parts of the Discussion section (e.g., Sections 4.2) tend to summarize previous studies in a way that resembles a literature review to me. While providing context is valuable, the main focus should be on interpreting the study’s findings in a more quantitative way if it fits and highlighting their contribution to new knowledge. For example, in Sec. 4.2, the paras related to control, TI, and TKE, the authors could provide a more in-depth analysis of their own results, discussing their implications and significance in more detail and quantitative way. Strengthening this aspect would improve the clarity of the study's contributions in order to remain centered on the novel aspects of the research.

Given the quality of the work and its good coverage of important factors, like including operational aspects such as control and aerodynamic load (e.g., in terms of Ct), I suggest that the authors explicitly discuss the effect of positive and negative shear on aerodynamic load. A brief elaboration in the main text or a relevant citation would enhance the clarity and completeness of the discussion. Additionally, I have come across a couple of reports and publications in recent years that explore multiscale interactions from mesoscale to microscale, down to structural responses, that can be cited for this purpose.

Specific comments

- In line 198-200, authors mention using the Eckert number (Ec) in the calculation of potential temperature perturbation amplitude, based on Muñoz-Esparza and Kosovic (2018), with a value of 0.2. It would be helpful if the authors could include and explain why the Eckert number is important in this context and how it influences the simulation, particularly in relation to the vertical confinement of perturbations and the diagnosed PBL height in this paper. Whether the 0.2 limit for the Eckert number remains the same across different applications, particularly in scenarios involving terrain effects. Does the presence of terrain influence the chosen value for the Eckert number, or is it considered a constant in all cases?
- How does your model avoid feedback between the turbulent signal in the buffer zone and the inflow boundary for the terrain simulation? More specifically, in your simulations with realistic land surface distributions, as well as simple terrain, does the method rely on statistically homogeneous turbulence within the buffer zone? If so, how can it be ensured that statistically homogeneous turbulence is achievable when large buffer zones are added?
- Aligned with above, for the innermost domain, to shorten the fetch required for turbulent spin-up (lines 196-198), cell perturbation has been used. However, the effects of this method are not limited to the potential temperature or velocity fields; it may also induce unrealistic thermodynamic conditions. This highlights the importance of having sufficient buffer zones at the inflow boundaries, where turbulence can develop spatially. As noted by Mirocha et al. (2014), who showed that without perturbations, a fetch length of several tens of kilometers is needed to achieve fully developed turbulence, meaning that a significant portion of computational resources is spent on these buffer zones. You mention in lines 202-203 that spinning up occurs between 1.5 and 2 km from the southerly boundary of D3. However, I believe more detail is needed on the

turbulence recycling process, particularly how it contributes to computational efficiency and results in faster spin-up.

- Given that the Kelvin-Helmholtz Instabilities (KHIs) are observed near the third row and below the LLJ nose, and are not caused by the turbines (as they also occur in the simulation without turbines), could the authors elaborate on the specific factors contributing to the formation of KHIs in this region (e.g. small Richardson number, …)? Are these instabilities primarily driven by shear in the wind profiles? You may slightly elaborate here with citation of any related reference on this process, for the region.
- In Figure 7 and Section 3.4, I am curious about the presence of Kelvin-Helmholtz instability (KHI) in Figures 7b and 7d and whether such instability (dynamical instability) could potentially influence streamline vertical displacement or have any potential impact on the overall flow field.
- In lines 327–336, a clearer and more quantitative explanation would be beneficial. The discussion relies on Figures C1a and C1b to explain thrust coefficient behavior, but it is somewhat unclear whether these figures present direct empirical data, simulation results, or theoretical curves. Could you clarify this? Additionally, including specific Ct values or a comparative table would enhance clarity.

  Since you are using an ADM, I assume you may have access to along-the-blade thrust force distributions or at least the total thrust force from the model. I suggest more quantitative details on this and refine the explanation accordingly.

- In Figure 9, analyzing turbulence intensity, alongside the given shear and veer studies by authors, can provide a better understanding of how terrain influences wake behavior in the wind park, as well as the relationship between TI and wake characteristics and recovery. I recommend authors could comment and elaborate on this.

Technical corrections and minor comments

- In the caption of Fig. 2, it appears that the domains are related to WRF, but clarifying whether they are mesoscale or microscale would be helpful. For example, the inner domain, as mentioned in lines 152 and 162, corresponds to the LES domain.
- In lines 172-173, the authors use the term 'simulations forced...'. While it is somewhat clear that the forcing files are from ERA5, the word 'forced' may give the impression of data assimilation, at least for me. While this is not necessarily incorrect, it would be helpful if the authors could slightly modify here to avoid potential confusion.
- Minor comment: The paper discusses the effect of terrain on wind speed but does not specify properly the extent of lateral inhomogeneity required to observe significant changes. A quantitative measure of inhomogeneity (e.g., spatial correlation metrics) in

the wind field during the study events would help clarify this aspect. This is particularly important given the discussion on terrain-induced accelerations, “even with simple topographic features, potentially causing substantial changes in wind speed and wind farm performance” (see lines 74–75). Additionally, it directly relates to the first study objective outlined in lines 86–87.

- Minor comment: While selecting a stationary window—where wind speed and large-scale forcing remain relatively steady—might be reasonable, could you clarify which dynamic events are relevant to this region? For example, are you referring to large-scale weather systems such as cold fronts, synoptic-scale cyclones, or other mesoscale influences? Lines 127-128.
- I may have overlooked something in Section 3.3—could you clarify what resolution is required to realistically simulate KH waves in WRF? (whether In all domains, WRF is able to resolve it ?, I assume no)? Additionally, since potential temperature serves as a useful metric for identifying wave overturning, I’m curious about how KHI responds to the selection of microphysics schemes. While this might extend beyond the immediate scope of your study, it would still be valuable if you could reference any relevant studies on KH waves in this context and area.
- Minor comment: Furthermore, is there a way to improve the representation of Figure C1? Perhaps an alternative visualization or additional annotations?.
- Minor comment: In lines 345-346 for the sake of more clarity, do you mean something like? : The initial terrain-induced up- and downward motions trigger disturbances, and the buoyancy restoring forces sustain the resulting undulations in the flow.