Reply to reviews – WES-2026-84

Paul Boumendil, Pierre-Antoine Joulin, Quentin Rodier, and Valéry Masson

Reviewer #2 :

We appreciate your feedback on our manuscript, which allowed us to incorporate valuable additions, and we hope significantly improve its quality and clarity. As several of your general comments overlapped with those raised by reviewer 1, we addressed them simultaneously and try to carefully balance both sets of recommendations where necessary.

The details of these revisions are described below. The comments from the reviewers are in black, while our responses are in blue. Excerpts from the manuscript are enclosed in quotation marks (“”). Additions made to the manuscript following the review are highlighted in **bold**.

<ins>Summary :</ins>

The study investigates the impact of a wind farm located in the south-west of France on the near surface temperature inside and around the wind farm, for a night with frost conditions. The study simulates a case study with the modelling framework Meso-NH/SURFEX/EOL. The main novelty lies in the location of the wind farm in hilly terrain and the associated impact of the wind farm on terrain specific meteorological conditions. Three main findings were identified. First, the temperature increase is the strongest for turbines located in valleys due to stronger temperature stratification. Second, the wind farm can influence the location of a convergence zone related to catabatic flow and thus temperature variations outside of the wind farm area. Third, while the air temperature is mainly increased around the turbines, the ground temperature upstream of each turbine decreases due to reduced wind speeds.

The authors nicely describe the temperature differences caused by the turbines, how the wind farm influences the location of a terrain-specific convergence zone, and how the results are affected by the local terrain characteristics. The discussion includes a well written section that puts the results in the context of spring night frost risk for vineyards and whether turbine-induced temperature changes could reduce this risk. The relevance of the topic is clear. However, I have concerns about multiple points that should be addressed before accepting the paper. The full list of comments, including explanations, are written below. In short, three main aspects should be addressed. First, the paper objectives in the abstract and introduction are not clear and require improvement (major comments 1-2). Second, since the study aims at analysing a realistic stable atmosphere, it should discuss the uncertainty that the vertical profiles and the atmospheric stratification could not be validated with the available measurement data. Also, to better support the conclusions made in the validation, calculating quantifiable validation metrics would be beneficial (major comment 3). Third, several explanations and conclusions of the paper are insufficiently supported by the results and require further analyses. Also, the discussion chapter needs more depth since it currently does not put the results in the context of previous studies, and limitations and uncertainties are barely discussed (major comments 4-5).

Overall, the paper covers an interesting topic and has a good base, but multiple points should be addressed to improve the clarity of the paper, better support the arguments made, and more critically discuss the findings. Therefore, I recommend to accept the paper with major revisions.

Major comments

1. The introduction states too many objectives, of which some are not clear or not specific enough and some are barely addressed in the paper. To improve the clarity of the paper and not raise wrong expectations the objectives should be adapted. Also, instead of mentioning them at different points in the introduction it is clearer to mention them in one place. Similarly, the objectives should be clearly mentioned in the abstract and in the conclusions. Specific improvement suggestions:

a. In the abstract the aim of the paper is not clear to me. It is mentioned that the study investigates turbine-atmosphere interactions during a stable nocturnal radiative cooling event (ll. 4-5). This is a description of what the study does, rather than a clear objective. I am especially missing the clarification that mainly the impact of the wind farm on temperature and terrain-specific flow behaviour is of interest.

We agree that the research objective should be stated more explicitly in the Abstract. We have revised the sentence to clearly emphasize that the primary focus is to evaluate the impact of the wind farm on near-surface/surface temperature and terrain-specific flow dynamics under stable atmospheric conditions:

"In this study, the multi-scale Meso-NH/SURFEX/EOL modelling framework, which includes coupled atmosphere-soil-vegetation and actuator models, is used **to evaluate the impact of the \textit{Ensemble Éolien Catalan} wind farm on ground and near-surface temperatures, as well as terrain-specific flow dynamics,** during a stable nocturnal radiative cooling event associated with frost risk in nearby vineyards."

b. ll. 81-83: "The objective is to investigate the combined effects of multiple turbines within the farm on local meteorological conditions, with particular attention to the near-ground atmospheric response." Suggestion: "The objective is to investigate the effect of a wind farm in hilly terrain on the wind speed, near-surface temperature, ground temperature, and terrain-specific meteorological phenomena." Explanation: with the previous formulation I expected more variables to be analysed than only the temperature, especially since the introduction contains a paragraph on humidity and evapotranspiration. By including explicitly “terrain-specific meteorological phenomena” you could hint to the impact on the convergence zone.

We fully agree with the reviewer's suggestion, the sentence in lines 81–83 has been revised accordingly

c. ll. 83-84: “The study aims to determine how the spatial organization of the turbines and the evolving mesoscale environment jointly influence meteorological change induce by the wind turbines.” Suggestion: remove parts on “spatial organisation” and clarify what you mean with “evolving mesoscale environment”. Explanation: Mentioning the spatial organisation of the turbines sounds like the effect of the wind farm layout is analysed. The results do not include the analysis of the layout, so this objective and ll. 489-492 in the conclusions should be removed. Keeping it in distracts from your main results. Also, I don’t understand what is meant with mesoscale environment, but based on the results I would guess that it refers to the hilly terrain. This should be clarified.

We acknowledge the reviewer's point that we have not study the impact of different layout orientation. We have removed the “spatial organisation” and replace it by “wind farm” l. 83-84. We have also rewritten lines 489–492 in the Conclusion to frame this topic strictly as an open perspective for future work, rather than an outcome of this study:

*"**Evaluating how turbine row orientation and overall wind farm layout influence near-surface meteorological variations represents a natural direction for future research.** A dedicated sensitivity study could therefore investigate the effect of wind farm layout on near-surface meteorological variations under identical atmospheric conditions, for instance by rotating the wind farm layout.”*

By "mesoscale environment," we do not refer only to the complex terrain, but rather to the evolving regional atmospheric conditions over time: heterogeneous temperature distribution, atmospheric boundary layer stratification, and katabatic flow. To clarify this distinction and avoid any confusion, we have rephrased the sentence in the manuscript as follows:

“The study aims to determine how the wind farm and the evolving regional atmospheric conditions **(including nocturnal stability, heterogeneous temperature distribution, and terrain-driven flows)** jointly modulate turbine-induced meteorological variations.”

d. ll. 92-94: “we aim to evaluate how the [...] wind farm interacts with the stable atmosphere shaped by the surrounding complex terrain, and how it may locally increase near-surface temperatures and potentially contribute to the mitigation of nocturnal frost over the surrounding vineyards”. Suggestion: possibly integrate part on stability in other objectives to make it more concise. Also, the second part on the night frost might raise the expectation that you analyse the frost risk damage on vineyards, which is not the case. I would remove this part from the objectives and only mention it as the context of the study.

We completely agree with the reviewer that our formulation could create false expectations regarding a crop- frost risk analysis. However, because the spring frost context serves as the primary environmental driver and physical motivation for assessing the study, we feel it is important to retain this background context. To address the concern while keeping expectations clear, we have rephrased the sentence to frame the frost event strictly as a physical context rather than a functional mitigation study:

"In this context, we aim to evaluate how the \textit{Ensemble Éolien Catalan} wind farm interacts with the stable atmosphere shaped by the surrounding complex terrain, and how it may locally increase near-surface temperatures **in the context** of nocturnal frost over the surrounding vineyards."

2. Based on the abstract and introduction I expected the paper to include analyses on the impact of wind turbines on the vegetation and humidity, since it is explicitly mentioned that the model includes vegetation, frost risk on vineyards is mentioned multiple times and a part of the introduction (ll. 25-33) focuses on the possible impact of wind farms on moisture, vegetation growth, evapotranspiration, ... I think it is good to briefly mention the importance of temperature changes for the vegetation to emphasize the importance of the study, but it should be made clear that this is only context and not part of the study.

Initially, we intended to analyse humidity parameters as well. However, given the complexity of the temperature and flow dynamics identified in this study, we chose to focus strictly on near-surface temperature variations to keep the manuscript relatively short and clear.

We acknowledge that mentioning vegetation and broader meteorological impacts without analysing humidity could mislead the reader regarding the scope of our work. Consequently, we have made the following adjustments in the Abstract and Introduction to explicitly frame these elements as contextual motivation rather than study objectives:

L. 1-2 : We refined the opening sentence to specify the focus on temperature: « Various studies have shown that wind turbine wakes can induce **temperature variations.**»

L. 3 : We removed the explicit reference to vegetation when describing the modelling framework: "In this study, the multi-scale Meso-NH/SURFEX/EOL modelling framework, which includes **coupled atmosphere, land-surface,** and actuator models"

We added a clarifying sentence at the end of the paragraph l. 33 on agricultural impacts to explicitly state the scope: "**Although assessing such ecological and agricultural**

outcomes lies beyond the scope of this study, understanding them critically depends on accurately characterizing near-surface temperature modifications."

L. 67: We rephrased the description of SURFEX to emphasize energy exchanges and surface fluxes rather than vegetation: "This framework combines the LES atmospheric model Meso-NH (Lac.2018) with the SURFEX land surface scheme to **represent soil-atmosphere** surface fluxes(Masson.2013)."

3. The validation includes descriptions or conclusions that are not sufficiently supported by the results. First, no validation metrics were calculated. The simulation is only compared to the measurements by plotting for one height the time series of wind direction, relative humidity, temperature and radiation, and for two heights the wind speed. The validation would benefit from calculating e.g. Willmott's index of agreement, systematic error and unsystematic error, especially since the study claims "overall good agreement" (l. 235) and a temperature bias is discussed (l. 247).

We thank the reviewer for this constructive suggestion. To provide a rigorous quantitative evaluation of model performance beyond visual comparisons of Fig. 3, we have computed statistical validation metrics, including the observed and simulated means, Mean Bias Error (BIAS), Root Mean Square Error (RMSE), systematic error (RMSE_s), unsystematic error (RMSE_u), and Willmott's index of agreement (d).

These metrics have been summarized in a new table (**Table 2**) and integrated into Section 4.1 to support our conclusions quantitatively. Section 4.1 has been thoroughly revised to incorporate these metrics and cite specific values throughout the discussion.

"To quantitatively evaluate the performance of the simulation against observations, statistical validation metrics including Mean Bias Error (BIAS), Root Mean Square Error (RMSE), systematic (RMSE_s) and unsystematic (RMSE_u) errors, and Willmott's index of agreement (d) were computed in accordance with the Willmott (1981, 1982) recommendations (Table 2). The simulation demonstrates overall good agreement with experimental observations across most evaluated parameters. **High values of Willmott's index of agreement (d) are obtained for almost all variables, reflecting strong model skill in capturing atmospheric dynamics.** As shown in Fig. 4a-b, the 10 m wind speed and direction closely match the observed time series, successfully reproducing both the daytime convective regime and the post-sunset wind weakening. **This is supported by high Willmott's index values for domain D2 (d = 0.944 for wind speed and d = 0.971 for wind direction). Furthermore, for 10 m wind speed in D2, the unsystematic error exceeds the systematic error (RMSE_u = 0.95 m/s > RMSE_s = 0.66 m/s), indicating that model discrepancies are predominantly driven by unresolvable random fluctuations rather than structural model deficiencies. The model also accurately captures the temporal variations of relative humidity (d = 0.967) and shortwave downward radiation (d = 1.000).**

The main discrepancy between measurements and simulation occurs during nocturnal cooling. A warm bias of approximately 2–3 °C **(RMSE of 2.60 °C)** is observed in both the 2 m air temperature and surface temperature (solid and dotted lines in Fig. 4e). This bias is consistent with the driving AROME forecast (black dots in Fig. 4e). **This nocturnal temperature discrepancy is directly reflected in the statistical metrics for temperature: Willmott's index drops to d = 0.857, the lowest value among all evaluated variables and the systematic error clearly dominates the unsystematic error (RMSE_s = 2.15** °C **> RMSE_u = 1.47** °C**).”**

Second, the validation ends with the conclusion “the atmospheric conditions simulated here are consistent with the primary objective of this study: to reproduce a stable and stratified boundary layer to investigate its interaction with wind turbine and surface properties”. While a stable atmosphere was simulated, the verb “reproduce” indicates that the stable stratification of the case study was reproduced. To me, this is misleading, since the measurements included no stability criterium and consequently it cannot be said with certainty that the simulated strength and vertical extent of the stable stratification were comparable with the actual situation of the case study. Especially since the simulated air and ground temperature deviated from the measurements by up to 3 K (l. 247). Since the estimation of stability criteria from measurements are often difficult or not possible, it is understandable that it is not included in the study, but this point of uncertainty should be discussed.

We agree with the reviewer's comment. The term "reproduce" was indeed misleading, as we have no observational verification of the vertical stability profile.

We have revised the text to clearly acknowledge this limitation. We explicitly state that while near-surface observations confirm nocturnal cooling, the exact strength and vertical extent of the atmospheric stratification remain unconstrained by measurements. Furthermore, we discuss how the observed 2–3 °C surface warm bias introduces uncertainty into the simulated inversion strength.

The concluding paragraph of Section 4.1 has been updated accordingly:

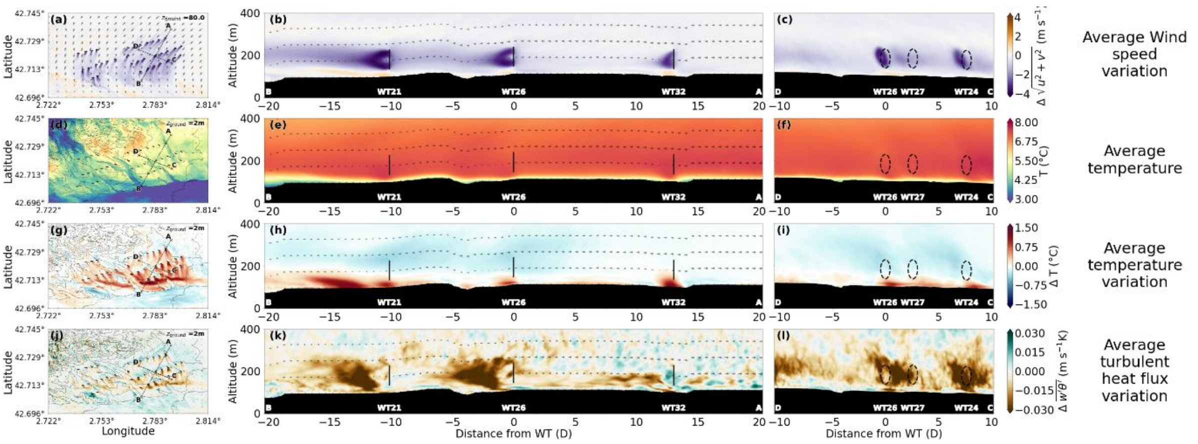
“This validation remains limited due to the availability of only a single near-surface measurement point, and the lack of validation at rotor height **in the absence of vertical profile observations. Consequently, while near-surface measurements confirm the occurrence of nocturnal cooling, the exact vertical extent, inversion strength, and thermal stratification of the real-world boundary layer cannot be quantitatively verified. The 2–3 °C surface warm bias introduces additional uncertainty regarding the exact magnitude of the simulated atmospheric stability compared to actual conditions.** Nevertheless, the atmospheric conditions simulated here are consistent with the primary objective of this study: to simulate **a physically consistent** stable and stratified boundary layer to investigate its interaction with wind turbine and surface properties."

4. In the results and discussion some points could be better supported by additional plots:

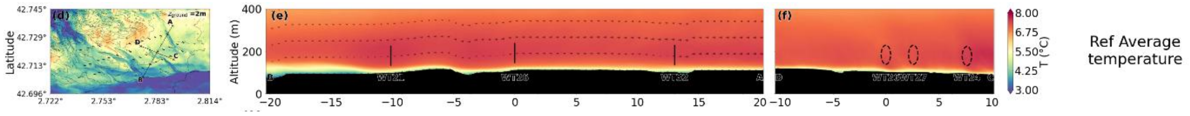
a. In ll. 364-365, when describing that turbines in valleys show stronger temperature anomalies, the authors give the explanation that the reason for the stronger anomalies are stronger temperature inversions in the valley. However, no plots are given that actually show these stronger temperature inversions. Here, it would be useful to include vertical cross sections with temperature contours that show once a wind turbine in a valley and once a wind turbine at higher altitudes to support the explanation.

This is indeed the purpose of Figure B1, which was not sufficiently highlighted in the original manuscript. To better illustrate this effect, we have modified Figure B1 by reducing the cross-section height from 800m to 400m. This allows us to better focus on the turbine wakes while also providing a clearer view of the terrain variations. However, as Figure B1 is too large to be included as such in the Results section, we have created a new figure extracted from Figure B1 (see below), showing only the wind-speed deficit, temperature, temperature variation, and heat flux. This new figure has been added to the manuscript to address Major Comment 4.

The cross-section passes through turbines 21, 26, and 32. Turbine 32 is located in a small valley, only a few tens of meters deep, which is nevertheless sufficient to accumulate relatively colder air, with temperatures approximately 1-1.5°C lower (see Fig. 11 at x=0 and y =0). In contrast, turbines 26 and 21 are located over relatively flat terrain. The new figure clearly shows how turbine 32 interacts with a stronger atmospheric stratification, resulting in larger temperature variations.



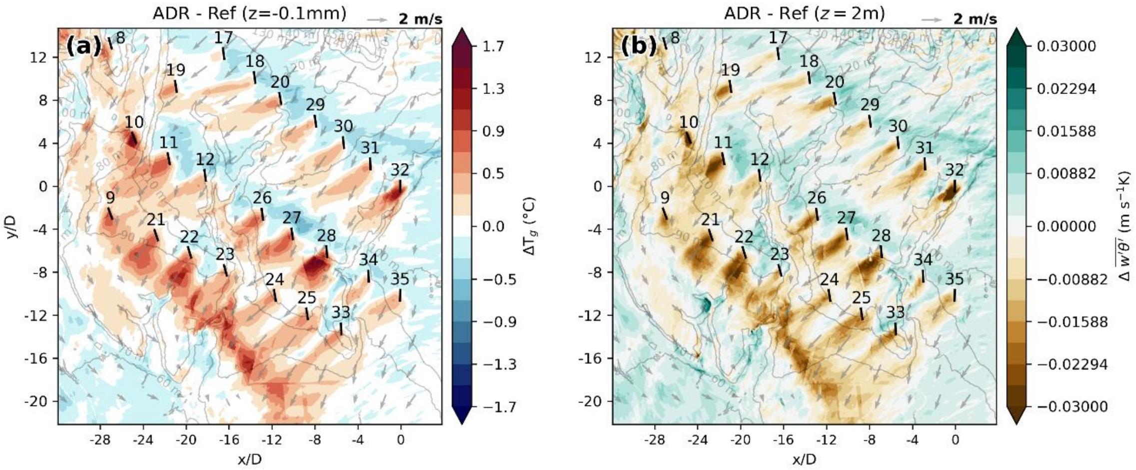
Comparison with the simulation without wind turbines (not shown in the article) further highlights the stronger stratification within the small valley around turbine 32.



New sentences has been added to the paragraph on lines 364–365 to explain this phenomenon: "**By comparing the terrain contours with the temperature variations induced by the wind turbines,** we can conclude that the strongest warming beneath the rotors occurs for turbines located within valleys, where cold air tends to accumulate (e.g., turbines 9, 10, and 32). **Figure 14 helps illustrate this phenomenon by showing average vertical cross-sections of domain D4 passing through turbines 32, 26, and 21. Turbine 32 is located within a small valley a few tens of meters deep, whereas turbines 26 and 21 are located over relatively flat terrain**. Within the valley, turbine-induced mixing interacts with stronger temperature inversions, bringing warmer air from aloft down towards the surface and thereby generating stronger temperature anomalies. For example, a temperature increase of up to 1.6 °C is observed downstream of turbine 32."

b. In ll. 380-383 it is explained that "ground temperature variations result from a combination of changes in wind speed and near-surface air temperature" which is "consistent with the formulation of the sensible heat flux from the surface model, which depends on both the surface-atmosphere temperature difference and the turbulent exchange driven by wind speed near the surface". Here I would suggest two points. First, to write down the equation for the sensible heat flux. This could also be done in the methods. Second, why not calculate and plot the sensible heat flux? This would help support the hypothesis that wind speed and air temperature are the cause of the ground temperature differences. Also, I think it could be helpful to the reader to explain more explicitly why the upstream temperature differs between 2m air temperature and ground temperature.

We thank the reviewer for this suggestion. Evaluating the variation of turbulent heat flux near the surface (at 2 m) clearly reveals spatial variations that directly match the patterns observed in both ground temperature and near-surface wind speed variations. Figure 13 has therefore been revised and replaced with the version shown below. See major comment 4b for revise description.



We have now explicitly detailed the physical mechanism driving the upstream ground cooling in Section 5.3. The wind deceleration induced by the upstream blockage effect weakens near-surface turbulent exchange and reduces the downward turbulent heat flux. This drop in turbulent mixing causes a thermal decoupling between the land surface and the near-surface air, allowing nocturnal radiative cooling of the top soil layer to dominate and induce localized ground cooling (see new sentence in major comment 4c).

c. In the discussion in ll. 398-400 turbine-induced turbulent motions are mentioned as the cause of temperature and momentum anomalies near the surface. However, turbulent fluxes were not analysed. Since I think that is a really interesting part it might be worth including an analysis of the turbulent temperature transport, including a figure. I saw that the appendix (ll. 528-537) already includes interesting explanations that address some of my points, but I think they should be expanded and in the main part.

To better illustrate and describe this mechanism, we have incorporated the Turbulent Heat Flux (THF) into the new figures 14 (see response to Major Comment 4a) and 15 (see response to Major Comment 4b).

We have added this description to Section 5.2 of the Results:

“At night, surface radiative cooling makes the ground colder than the air above, establishing a stable air-temperature inversion. Under these conditions, heat is transported downward by background turbulence from the warmer air aloft toward the colder surface, resulting in a negative turbulent heat flux (Fig. 7f). At and below rotor height, a negative flux anomaly develops downstream of each turbine, making the turbulent heat flux even more negative (Fig. 14j, k and l). This indicates an enhanced downward transport of warm air or an upward transport of cold air caused by wake-generated turbulence. This increased vertical mixing within the stable stratification drives the positive temperature anomalies below the rotor and the negative anomalies at and above rotor level.”

We have added this description to Section 5.3 of the Results:

“Compared to the 2 m air temperature response, ground temperature exhibits a similar spatial pattern downstream of the turbines, where downward turbulent mixing of warmer air creates distinct surface warming plumes. However, a key difference lies in the higher prevalence of cooling anomalies (30% of the subdomain for ground temperature versus 20% for air temperature) and a lower overall mean impact (+0.043 °C vs. +0.173 °C). Comparing ground temperature variations with the near-surface turbulent heat flux (resolved + subgrid) reveals an identical spatial pattern (Fig. 15b), featuring positive turbulent heat flux anomalies where cooling occurs and negative anomalies where warming is observed. These cooling anomalies are primarily located immediately upstream of the turbine rows, in regions affected by the upstream blockage effect (Fig. 10). In these deceleration zones, reduced near-surface wind speeds weaken turbulent exchange, thereby reducing the downward (negative) turbulent heat flux toward the ground. This drop in turbulent exchange leads to a local decoupling between the ground and the near-surface air. Consequently, nocturnal radiative cooling of the top soil layer prevails, enhancing ground cooling upstream of the turbines. Conversely, because the upstream near-surface air is continuously replaced by the background flow, its temperature remains unaffected by this localized surface cooling. Ground temperature variations reflect a coupled response to both air temperature changes and wind-speed change through modifications of turbulent transfer.”

5. The entire discussion should be expanded. Currently the three subsections summarise the results (6.1), discuss implications for vineyards (6.2) and discuss implications for experimental studies (6.3). I am still missing a critical comparison of the findings with previous studies and a discussion of the studies limitations or uncertainties. The former part on previous literature could be incorporated in 6.1, which is currently only a summary. The latter part could e.g. include the uncertainty that the simulated stability could not be validated and expand on the limitation that the results are specific to certain conditions.

Thank for this comment, which strongly aligns with major comment 5 from reviewer 1. We have thoroughly rewritten Section 6.1 to systematically benchmark each of our physical findings against literature, thereby transforming this section into we hope a real scientific discussion. A new subsection 6.4 **"Limitations and uncertainties"** has been added at the end of the Discussion section. This new subsection addresses the fact that the results are specific to certain atmospheric conditions and detail the uncertainties from observational constraints, the lack of experimental vertical profiles, and the simulated nocturnal warm bias.

Section 6.1 : \subsection{Physical processes controlling turbine-atmosphere interactions}

The results highlight the importance of flow heterogeneities when investigating wind farm-atmosphere interactions in complex environments. In the present case, both the complex terrain and the stable, strongly stratified atmospheric conditions play a key role in shaping the flow. **These findings align with recent literature emphasizing the necessity of multiscale modeling chains (Wise.2022) to capture the full spectrum of interactions across spatial scales.**

The simulations reveal a strong heterogeneity of meteorological conditions across the domain, with significant spatial variability in both wind speed and wind direction horizontally as well as vertically. This heterogeneity becomes particularly pronounced near the surface, where local katabatic flows induce the convergence and interaction of air masses with distinct thermal and momentum properties. **As recently highlighted by Lundquist (2026) in complex terrain setups, terrain-induced atmospheric structures can strongly interact with or even dominate near-surface microclimatic signals, making realistic multiscale forcing essential to isolate turbine-induced perturbations from natural background variability.**

Within this already complex background flow, wind turbines introduce additional perturbations that further modify local atmospheric circulation. Under stable and strongly stratified conditions, turbines facilitate vertical transport through two main physical mechanisms: **(i) organized vertical motion driven by blade rotation and wake divergence, which generates coherent vertical velocity pairs (Mirocha.2014, Boumendil.2024), and (ii) vertical convergence and divergence of turbulent heat and momentum fluxes downwind of the rotor (Archer.2019, Wu.2021, Wu.2023). In stably stratified boundary layers, both mechanisms act to transport warmer, higher-momentum air from aloft down toward the surface.**

At the single-turbine scale, the wind turbine act as localized obstacles to the flow, manifesting as upstream induction zones and near-surface Venturi acceleration beneath the rotor. **Local sub-rotor wind acceleration under stable stratification is consistent with fine-scale LES studies of isolated and aligned turbines (Mirocha.2014, Xie.2017, GarciaSantiago.2024), where buoyancy forces in stable stratification reduces vertical displacement over the obstacle and enhance localized flow acceleration near the ground.** At the wind farm scale, cumulative blockage causes broader flow redistribution, including lateral edge acceleration and slight displacement of surrounding air masses, **echoing findings from mesoscale and microscale wind farm simulations (Fitch.2012, Quint.2025).**

The amplitude of the meteorological variations induced by each turbine appears to be strongly influenced by local topography. In particular, turbines located within valleys generate the largest perturbations, as they interact with stronger near-surface temperature inversions. The strong relationship between near-surface thermal stratification and the magnitude of turbine-induced temperature variations is consistent with the experimental findings of (Wu.2021) and the numerical results of (Boumendil.2024). In contrast, the amplitude of these variations does not seem to be primarily controlled by the turbine power production (Fig. 9) nor by its spatial position within the wind farm.

The atmospheric perturbations generated by the turbines extend to near-surface air temperatures before propagating to the land surface through coupled energy exchanges. At 2 m height, the simulations indicate that wind turbines induce a mean near-surface warming of approximately 0.4 °C within the wind farm immediate area, with local maxima reaching up to 1.6 °C. This magnitude aligns well with field observations and numerical modeling under stably stratified nocturnal conditions, which typically report air temperature increases ranging between 0.1 and 2.2 °C depending on inversion strength, turbine hub height, and farm layout (BaidyaRoy.2010, Rajewski.2013, Smith.2013, Henschen.2011, Wu.2021).

Driven by these near-surface air anomalies, local modifications of wind and turbulent heat flux, coupled land-atmosphere energy exchanges lead to top-soil ground temperature variations ranging from -0.7 °C. This range and its spatial heterogeneity are consistent with satellite-derived land surface temperature observations over operating wind farms (Zhou.2012, Slawsky.2015) as well as surface energy balance modeling (Lu.2015, Xia.2019). Upstream blockage and velocity deficits weaken surface turbulent exchanges, leading to localized ground cooling, whereas downwind advection of turbine-mixed air enhances downward heat transfer, resulting in localized ground warming (Xia.2019, Quint.2025).

Section 6.4 : **\subsection{Limitations and uncertainties}**

While the present simulations provide high-resolution insights into turbine-atmosphere-surface interactions in complex terrain, several limitations and uncertainties must be emphasized.

First, model validation is inherently constrained by the availability of observational data. Quantitative evaluation was restricted to a single operational surface station at 2 m or 10 m height. In the absence of vertical profile observations at rotor height, the exact vertical extent, inversion strength, and thermal stratification of the actual boundary layer could not be quantitatively verified against field measurements.

Second, the simulation exhibits a nocturnal warm bias in air temperature of approximately 2-3 °C, inherited from the driving AROME forecast and subsurface soil temperature initialization. Although sensitivity experiments were performed on

mixing length formulations, soil parameters, and atmosphere-surface coupling coefficients, this systematic bias could not be fully removed. Consequently, while the simulated boundary layer is physically consistent and stably stratified, this thermal bias introduces quantitative uncertainty regarding the exact magnitude of the simulated atmospheric stability relative to the study period.

Finally, the results presented here are specific to a single nocturnal event featuring a complex synoptic transition with weak rotor wind speed, local katabatic flows, and a late-night mesoscale vortex. While the fundamental physical processes identified in this study are generalizable to stably stratified boundary layers over complex topography, the exact quantitative impacts on near-surface warming and soil temperature variations remain inherently linked to the specific thermal stratification, wind farm geometry, and soil properties of this case study.

<u>Minor comments</u>

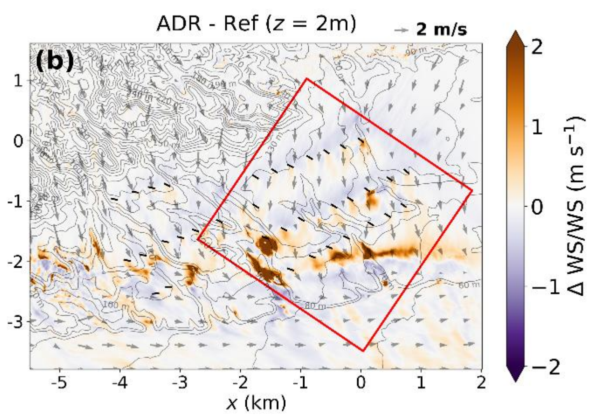
1. Generally:

a) Always specify which wind speed you refer to. It is not always clear whether it is the horizontal wind speed or total wind speed (e.g. fig. 5,7,9). You might do so e.g. by using Uh or Utot instead of WS and adding it to the figure captions.

We always refer to the horizontal wind speed. We have therefore replaced "WS" with U_h in all the figures and added in the first sentence where it is defined.

b) Instead of using the absolute wind speed, the normalised might be more meaningful. E.g. in fig. 9 with respect to l. 339.

We initially considered plotting the absolute wind speed at 2 m (figure below). However, the near-surface variations in absolute wind speed are very large in some areas, which completely masks the rest of the signal and makes it more difficult to clearly identify the regions of flow acceleration and deceleration.



c) In most figures that include arrows to visualise the wind speed and direction, the arrows are not well visible. A darker/different colour for the arrows, or a different colourbar might help.

Figure 5: The colormap has been inverted and extended up to 12 m/s. The arrows have also been changed to black and white to improve overall visibility.

We tested other colormaps, but they did not provide better visibility with respect to the arrows and terrain contour lines.

d) In most figures I would add more terrain contours, especially in fig. 12.

Thank you for this suggestion which was also from the reviewer 1. Contour lines have been added to Figure 9b, 5. The contour interval has also been reduced from 20 m to 10 m in Figures 8, 9, 10, 11, 12, and 13.

We acknowledge that it can sometimes be difficult to distinguish between the turbine locations, arrows, terrain contours, and the variable represented by the colormap. In each case, we have tried to find a compromise that ensures all elements remain visible without compromising the visibility of the others.

2. I would suggest to read through the manuscript one more time and check for phrasings that are not specific, since it is sometimes either not clear what you refer to or multiple interpretations are possible. Here are some of the points that need in my opinion clarification:

a) What is meant by “immediate vicinity of the wind farm” (ll. 10-11, …)? Is it the wind farm area, wake area, wind farm + wake area, a different definition?

Replaced by “In the wind farm”

b) ll. 39 “To investigate this phenomena and its mechanisms”: Which phenomenon and mechanisms?

Replaced by “To investigate **wind-turbine-induced meteorological variations and their underlying mechanisms**, numerical studies have been conducted […]”

c) ll. 41 “By the large-scale nature of the first studies on this subject, such effects”: What is meant with large-scale nature? What was the method/model and resolution? Again, which effects?

Replaced by “**Given the coarse spatial resolutions and low-fidelity wind-farm parameterizations used in** the first studies on this subject, such effects were primarily attributed to the turbulent kinetic energy (TKE) added by wind-turbine wakes, which enhances vertical mixing.”

d) l. 46: What do you mean by “natural atmospheric stratification”? The stratification present in the background flow?

Yes, exactly. To make it clearer, I have replaced the term “natural” with “background.”

e) l. 271: Which circulation do you mean? The large-scale wind direction change?

Yes, to make the meaning clearer, I have added “**atmospheric** circulation.”

f) For the whole conclusions specify when you refer to air temperature and when to ground temperature.

Added as suggested

g) In l. 463, what do “these atmospheric perturbations” refer to?

“These temperature and wind speed changes”

h) In l. 468, what is meant by “local horizontal distribution of air masses”? Overall, the clarity of this sentence could be improved. I understand it as “the wind farm behaves as a collective obstacle and results in the vertical and lateral flow deflection around the wind farm”

Indeed, the initial sentence was not clear. Following the suggestion, we propose: “Second, the wind farm behaves as a collective obstacle to the atmospheric circulation, inducing vertical and lateral flow deflection around the wind farm.”

3. The structure of the introduction needs improvements to make it more concise and better frame the context of the study.

a) The explanations of the model in ll. 65-80 are in my opinion too elaborate for the introduction. I would move them to the methods chapter. Briefly mentioning the model in one to two sentences should be sufficient for the introduction.

We agree with the reviewer that lines 65–80 in the introduction contained too many technical details regarding the modeling components. As suggested, we have reduced this part:

ll. 65-80 replaced by : **“To account for these processes, we employ a multi-scale online coupling approach, referred to as Meso-NH/SURFEX/EOL. This framework combines the LES atmospheric model Meso-NH (Lac.2018) with the SURFEX land surface scheme (Masson.2013) and the actuator disk model with rotation (ADR) implemented in the EOL module (Joulin.2020, Boumendil.2024). This setup dynamically downscales atmospheric conditions from the mesoscale down to the microscale over complex terrain, resolving both turbine wake rotational dynamics and fine-scale surface-atmosphere exchanges. In our previous work (Boumendil.2025), this coupling framework was developed and validated against the VERTEX measurement campaign (Archer.2019) in a single-turbine configuration, demonstrating its capability to reproduce realistic atmospheric conditions and capture wake interactions within transitioning boundary layers.”**

b) The introduction is missing a section on wind farms in hilly terrain. Since the study analyses the impact of a wind farm on terrain-specific flow features, like katabatic flow, this should be added.

We agree with the reviewer that framing the challenges of modeling wind farms in complex and hilly terrain is important for context.

To address this without overloading the Introduction or distracting from the core focus of our article we have added a concise sentence in the Introduction highlighting key terrain-driven challenges and features:

« In the present paper, we extend this approach to a real wind farm located near Perpignan (France) call \textit{Ensemble Éolien Catalan} place **within the complex terrain of** the Languedoc vineyards. **Hilly and complex terrains represent strategic sites for wind energy deployment due to local flow acceleration, yet accurately modeling turbine wakes in such environments remains challenging due to complex terrain-driven flows, canopy interactions, and nocturnal thermal circulations (Fernando.2019, Kale.2023, AlOqaily.2025).** The objective **of this study** is to investigate the effect of a wind farm in hilly terrain on the wind speed, near-surface temperature, ground temperature, and terrain-specific meteorological phenomena. The study aims to determine how the wind farm and the evolving mesoscale environment—including nocturnal stability, heterogeneous temperature distribution, and terrain-driven flows—jointly influence meteorological change induce by the wind turbines. »

c) The context of frost protection methods in ll. 90-91 would fit better earlier in the introduction, e.g. around ll. 25-33.

We tested integrating the context of frost protection into lines 25–33. However, because that early part of the Introduction is dedicated to setting the context of wind farm–atmosphere interaction, introducing specific agricultural frost mitigation techniques at that stage felt disrupted the narrative flow. We have therefore chosen for now to retain this context around line 90, where it serves as a natural transition toward the specific objectives of our case study.

4. ll. 25-26 “those […] temperatures and increased vertical mixing”: Vertical mixing was so far not mentioned.

Rephrased lines 25-26 " Those enhanced nighttime temperatures **and associated microclimatic changes** may reduce dew duration and modify soil moisture availability, potentially impacting crop respiration, photosynthesis, and soil decomposition rates (Armstrong.2014,Adkins.2017)."

5. In Fig. 1 I would not show the simulation domains yet, since it is not explained yet that four-way nested simulations are performed. Here I would suggest to include a figure of the wind farm (subfigures of D4) and a map of France which shows where the wind farm

is located. Fig. 1 can then be included in the methods chapter. Also, add to the legend a symbol for the RADOME station and that the red boxes are the child domains.

A new figure has been added presenting the wind farm layout and its national and regional orographic context with satellite imagery. The former Fig. 1 has been moved to the methods section, and the RADOME station and child domains have been added to the figure legend.

6. In section 2.2 I am missing information on the hub height wind speeds and wind direction, and the approximate time of sun set/sun rise.

Added as suggested: "According to the AROME model, on 8 April at 05:00 UTC, the hub-height wind speed at the center of the wind farm was approximately 4.8 m/s from the NNE (24°), with sunrise occurring at 05:20 UTC.

7. In the methods, the governing equations of the model should be included."

We agree that the description of the Meso-NH and SURFEX models and their associated parameterizations in the Methods section was not sufficiently detailed. We have revised this section to provide a more comprehensive description of the equations and the model assumptions. Additional details on the Meso-NH and SURFEX parameterizations have also been included, and references to publications providing more detailed descriptions of the models and their formulations have been added.

"Meso-NH model solves the non-hydrostatic system of conservation equations for momentum, mass, humidity, and other scalar variables, as well as a thermodynamic equation derived from entropy conservation. A detailed description of these governing equations and the model's formulation is available in Lafore (1998). In this work, the choice is made to solve the pseudo-incompressible system of equations as defined by Durran (1989) to filter out acoustic modes and allow for an explicit time-stepping integration.

The prognostic variables include the three velocity components, the dry potential temperature, and the mixing ratios of the water phases (Lac.2018). The governing equations are formulated in flux form using the dry air density of a hydrostatic reference state at rest. Enforcing the corresponding continuity constraint requires solving an elliptic equation for the pressure perturbation.

These equations are formulated on a conformal projection plane to account for the Earth's sphericity and discretized horizontally and vertically on an Arakawa C-grid using a height-based terrain-following coordinate system. Momentum transport is integrated in time with an explicit Runge-Kutta centered 4 th-order scheme paired with a centered 4th-order spatial discretization. **Advection of meteorological scalars and water substances is handled via the Piecewise Parabolic Method.**

Subgrid-scale turbulence is parameterized using a three-dimensional, 1.5-order closure scheme based on a prognostic equation for the subgrid TKE (Cuxart.2000), closed with a mixing length as defined by Deardorff (1980) (Honnert.2021). Convective scale motions are explicitly resolved across the finest nested grids; however, for the domain D1, shallow convection is parameterized using the Eddy-Diffusivity-Kain-Fritsch parameterization (Pergaud.2009). **Radiation forcing terms are represented** by the ecRad modular radiation scheme (Hogan.2016) **and the** ICE3 mixed-phase single-moment microphysics scheme, which predicts mass mixing ratios for the six water categories (Caniaux.1994, Pinty.1998).**”**

“An online coupling with the SURFEX v9.0 surface transfer model (Masson.2013) is employed to finely model surface exchange with the atmosphere. Soil and vegetation effects are captured through the explicit multilayer model ISBA-DF (Boone.2000,Decharme.2011) which solves the heat and mass transfer equations in the soil with 14 vertical soil layers. **Heat transfer is computed using Fourier's law, with soil thermal properties depending on soil texture, moisture, and ice content. Water transfer is described using the mass-conservative mixed form of the Richards equation, accounting for both liquid water transport and water vapor diffusion. The surface heat flux into the soil-vegetation composite is determined by the surface energy balance, which partitions net radiation into sensible and latent heat fluxes (Masson.2013).** Urban effects are modeled using the TEB model (Masson.2000).**The detailed formulations of these physical processes are provided in the SURFEX scientific documentation ({P.LeMoigne.2018).”**

8. l. 105: How many turbines each?

Added as suggested: “The Catalan wind farm consists of 35 turbines, **includin**g 9 Vestas V80 (2 MW) **and 26** Vestas V90 (3 MW), resulting in a total installed capacity of 96 MW.”

9. l. 121-122 “Minimum temperatures close to the ground (50 cm) reached values down to -8°C”: What is the source?

We thank the reviewer for pointing this. Despite extensive searches, we were unable to retrieve the exact source confirming the -8 °C temperature measurement at 50 cm. However, exceptionally low temperatures were indeed recorded at 2 m between 6 and 8 April across several meteorological stations (notably -7.4 °C at Saint-Étienne-Bouthéon on 8 April), as reported in Météo-France's April 2021 climate bulletin. Since this information is already detailed in the second paragraph of Section 2.2, we have simply removed the sentence from lines 121–122 to avoid any inaccuracy.

10. In section 3.1 mention again that the model is a LES. So far, this aspect is only mentioned in the introduction (l. 66) and can be easily missed.

“In this work, Meso-NH v5.7.1 is used to simulate the atmosphere from mesoscale to micro scales**, with the finest domains relying on a LES method.”**

11. What do you mean with “metric scales” (l. 141)? Do you mean “scales of a few metres”?

Yes, to maintain the parallelism with “mesoscale” at the beginning of the sentence, I have replaced “metric” with “microscale.”

12. In section 3.1/table 1 I would specify for each domain the exact simulated times of the case study.

A column has been added indicating the simulated time for each domain. The table title has also been changed from “Domain dimensions of the numerical setup” to “Characteristics of the numerical setup.”

13. Is the ADR (l. 197) based on Wu & Porté-Agel (2011)? In the given reference of Boumendil et al. (2024) I cannot find a detailed description of the ADR.

No, the ADR implemented in Meso-NH is not directly based on the formulation of Wu and Porté-Agel (2011). In particular, our approach does not rely on a separate BEM calculation to determine the induction, as the aerodynamic forces are directly computed from the flow resolved by the Meso-NH.

The description of the implementation of the ADR model in Meso-NH is available in the PhD thesis of Pierre-Antoine Joulin (2019), while its validation was presented in Boumendil et al. (2024). Line 197 has been modified and an additional sentence has been added:

“To overcome these limitations and provide a compromise between accuracy and computational cost, the Actuator Disk model with Rotation (ADR) has been implemented in Meso-NH since version 5.7.**0 (Boumendil.2024), offering a more affordable alternative while still capturing the rotational effects (Wu.2011). A detailed description of its implementation is provided by Joulin (2019), while its validation is presented in (Boumendil.2024).”**

14. In section 3.4 include turbine specifications like the cut-in wind speed. Since the wind speeds during the analysed period are quite low, this is an important information.

“The operating wind speed range of the wind turbine used is 3--24 m/s, with a rated wind speed of 12.5 m/s.”

15. In l. 219 the “reference incoming wind speed U_inf [...] over a disk” is mentioned. Is that the rotor equivalent wind speed (REWS) or is it defined differently?

We do not use the Rotor-Equivalent Wind Speed (REWS). Among the options available in Meso-NH, the controller wind speed calculation method selected here consists of computing the magnitude of the wind velocity spatially averaged over an upstream disk located ahead of the wind turbine, combined with a 1-minute temporal moving average.

To clarify that both spatial and temporal averaging are applied in this calculation, we have revised the text in l. 219 as follows: “The reference incoming wind speed U_inf for each turbine is calculated as a **spatial** average over a disk located one rotor diameter upstream of the turbine, **averaged** over a 1-minute time **interval**.”

16. I would suggest to change the chapter header 4 “Validation and analysis of the simulation” to “Validation and meteorological conditions at the test site”, since with “analysis of the simulation” I expected to read all the results already.

Change as suggest

17. The structure of chapter 4 on the validation and meteorological conditions could be improved to clearly separate both parts. E.g. currently in the validation section 4.1, ll. 236-245 are a pure description of the meteorological conditions and should therefore be in section 4.2/4.3. 1

We agree that Section 4.1 contained descriptive elements of the meteorological evolution that were better suited for the subsequent subsections. We have restructured Section 4 to strictly separate model validation from the meteorological condition analysis.

The vertical profiles of Figure 4 have been moved to the Appendix. The figure A1 has been moved to Section 4.3, and a paragraph has been added to describe the vertical profiles.

18. Fig. 3:

a) Adding to minor comment 17, the figure is a mixture of validation and variables only used to describe the meteorological conditions. It might be clearer to separate those, so to put the variables without measurements (cloud fraction, TKE, lapse rate) in a separate plot.

The separation of the plots with and without experimental measurements would clarify which results are related to the validation and which describe the atmospheric conditions. However, we believe that using two separate plots would make the comparison between variables more difficult compared to the present arrangement.

To address this comment, we propose to apply the separation between the validation and the description of the atmospheric conditions suggested in Comment 17, and to add the following sentence: “**Evaluation is performed for the set of meteorological variables available from the RADOME observation network at this site.**”

b) I would not mix title and legend, i.e. where needed include the linetype in a separate label of each subplot.

We have simplified the titles and added a legend for the relevant subplots.

c) For the cloud fraction (3d)) specify whether measurement/D2/D3. Probably D2, right?

Indeed, the cloud fraction corresponds to domain D2, and this has now been specified in the title.

d) The axis label in 3h) states "heat flux", but short wave up and down refers to radiation, right? Should be changed.

Corrected as suggested

e) Is it correct that no measurements for short wave up exist?

Yes, no measurements of short wave up are available at the Perpignan RADOME station

f) The figure caption states that the short-wave radiation is plotted for D2 and D4, while the legend says D2 and D3.

The caption has been corrected, as domain D4 does not include the Perpignan RADOME station.

19. Fig. 4 can be removed, since the measurements only contain one height and are therefore not useful to be compared in vertical profiles. Generally, I think it's good to include vertical profiles as a description of the meteorological conditions, e.g. fig. A1 added to section 4.3. Then the sentence in l. 305 can be removed.

As suggested sentence l.305 is removed and fig. A1 is added to section 4.3. and a paragraph has been added to describe the vertical profiles.

20. l. 250-251: How would a higher resolution decrease the temperature bias? Since it is a systematic error related to the input data, I do not see that as given.

We agree that the systematic bias is primarily related to the input data. However, we hypothesize that increasing the model resolution could improve the representation of near-surface processes, particularly nocturnal radiative cooling, and potentially reduce the temperature bias. This effect would, however, require further investigation.

To avoid overinterpreting this potential effect, we have therefore decided to remove the sentence in question.

21. l. 271: If the wind speeds are close to the cut-in wind speed, are they running or not? And could that result in the turbines in the wake of other turbines to be shut-off because the wind speeds might decrease to below cut-in?

The turbine cut-in wind speed is 3 m/s, while the wind speed within the wind farm in the simulation without wind turbines ranges between 4.5 and 6 m/s. Therefore, the turbines are operating, as shown in Fig.8 with the mean power output of each turbine. However, significant wake effects can be observed, with downstream turbines producing very little power, particularly turbines 2, 3, 9, 10, 21, and 22.

22. l. 280/Fig. 6: How did you decide on the segment AB?

A sentence has been added to justify the choice of segment AB: **"This cross-section is aligned with the wind direction at hub height and focuses on the eastern and southern parts of the wind farm, which are subsequently analysed in Section 5 (see segment AB in the horizontal cross-sections)."**

It also provides an opportunity to introduce the presence of the katabatic flow, which plays an important role in the observed temperature variations.

23. For section 5.1 I would choose a different header, since not only veer is described. E.g. "Wake deficit and power production"

The suggested change has been made.

24. l. 324 "acceleration [...] along its lateral edges": I cannot see a speed-up at the sides of the wind farm. Where is it? (also in discussion ll. 403-404)

There is indeed only a weak acceleration (Fig. 7a), and only on the right side at approximately x =1.5 km et y = -3 km.

The sentence on line 324 has been corrected as follows: "At the wind farm scale, flow acceleration can also be observed above the farm (Appendix Fig. B1e) **and, to a lesser extent, along its eastern lateral edges** (Fig. 8a). »

25. l. 326: When stating that the wind farm behaves like an obstacle it would be good to mention the (global) blockage effect and add references on that.

Same comment that reviewer 1 : The wind farm blockage effect has now been explicitly introduced, and a relevant reference on this topic has been added.

"These wind speed anomalies arise from the same mechanism as the sub-rotor acceleration but acting at the scale of the farm. ***This phenomenon, commonly referred to as wind farm blockage (Bleeg.2018)****, occurs because the wind farm behaves as an obstacle that locally modifies the atmospheric circulation,* ***forcing the flow to decelerate in front of the wind farm*** *and accelerate around and above the farm in order to satisfy mass conservation."*

26. Fig. 8: How is the power ratio calculated?

Same comment that reviewer 1: What we refer to as the power ratio is the ratio between the power produced by the wind turbine and its maximum theoretical power output.

New sentence added before line 315 : « **Figure 9 shows the ratio between the mean power produced by the ADR and the maximum theoretical power output of the wind turbine. Power ratio** is maximal for turbines located along the outer edges of the farm, unaffected by upstream wakes, whereas turbines within the interior produce around half of the power."

27. Fig. 11(a,c) and Fig. 12(a) show kind of the same information. I would remove 11(a,c) or move them to the appendix and integrate Fig. 11(b) in Fig. 12.

We agree that, since Fig. 12(a) = Fig. 11(c) − Fig. 11(a), Fig. 12(a) alone could be sufficient to show the temperature perturbation induced by the wind farm. However, we believe that the disposition of subplots of Fig. 11 provides useful additional information by clearly illustrating the heterogeneity between the 2 m air temperature in the background atmosphere (a) and that induced by the wind turbines (b). Furthermore, Fig. 11 allows the displacement of the katabatic flow to be identified, which cannot be observed from Fig. 12. For these reasons, we have chosen to retain for now both figures.

28. In section 5.2 I would appreciate more analyses of the histogram of Fig. 12, e.g. the number of occurrences. Currently only the magnitude of the temperature difference is addressed. Also, the histogram of Fig. 13 is not discussed at all in section 5.3. Since it is interesting to see that the distribution differs between ground and air temperature, that could be added.

We agree that the analysis of the temperature variations was incomplete, particularly regarding the distributions of subplots (b) and (c) of Figures 12 and 13. The paragraph on lines 371–375 has therefore been rewritten to provide a more detailed description and analysis of the vertical profile and histogram shown in Figure 12.

Furthermore, the histogram in Figure 13 has been removed and replaced with a spatial map of the turbulent heat flux at 2m to demonstrate the strong similarity between its spatial footprint and that of the surface temperature. Finally, a dedicated paragraph comparing near-surface air temperature and ground temperature has been added to explain the similarity and differences.

29. l. 372 "This suggests that [...] turbine-induced mixing tends to increase near-surface temperatures downstream of the wind farm, while modifications of the flow field around the farm tend to slightly cool the surrounding regions." I don't get how the spatial differences follow from the description that warming anomalies are stronger than cooling anomalies. They rather summarise the entire section 5.2. Maybe change "this suggests that".

We agree the spatial localization is not inferred from the histogram, but rather from the spatial map analysis. We have removed "This suggests that" and reworded the transition

"**The spatial distribution suggests that under stable atmospheric conditions** turbine-induced mixing tends to increase near-surface temperatures downstream of the wind farm, while modifications of the flow field around the farm tend to slightly cool the surrounding regions."

30. l. 374 "These statistics should [...] be interpreted with caution": The statistics in general or only the exact numbers? If "in general", is it even meaningful to include them?

Our intention was to warm the reader that the statistics such as the mean or the proportion of affected pixel are sensitive to the arbitrary boundaries of the subdomain. Expanding the subdomain further out would increase the proportion of unaffected pixels and lower the domain-averaged mean towards zero. However, these statistics remain meaningful as a relative quantitative summary of the spatial response within the wind farm's area of influence. We have reworded the concluding sentence:

"These spatial statistics should, however, **be considered indicative rather than absolute due to their dependence on the subdomain boundaries. Nevertheless, they provide a meaningful quantitative basis confirming the dominance of 2 m air temperature warming across the study area.**"

31. In l. 401 change “the alignment of turbines in rows” to “the wind farm” since the alignment of the turbines was not analysed.

32. In l. 405 I would phrase it more carefully and say that these factors likely amplified the effects, since you did not analyse the sensitivity to different atmospheric conditions. This is a good point to use literature to support your claim.

“In the present case, the complex meteorological conditions and the strong stratification **are likely to further amplify** these effects.”

33. In l. 408, I would clarify that Wu and Archer (2021) did not look at hilly terrain, but that you are referring to the lapse rate.

Reversal of the sentence to avoid any ambiguity without making it more heavy by specifying “not at hilly terrain.”

“The strong relationship between near-surface thermal stratification and the magnitude of turbine-induced temperature variations is consistent with the experimental findings of (Wu.2021) and the numerical results of (Boumendil.2024).”

34. In ll. 412-414 and Fig. 14 no new results should be brought up in the discussion. This part fits in the results section. However instead of Fig. 14 I would include vertical cross-section. In the current depiction I find it difficult to distinguish the different heights.

The figure displaying the 3D isosurfaces, along with its descriptive text, has been removed from the Discussion section and relocated to the Appendix

A new figure showing vertical cross-sections of the atmospheric boundary layer has been added to the Results section to provide a clearer and more direct visualization of the vertical structure and flow features.

35. In l. 465 only include conclusion on turbulent heat flux if you actually analysed it (see major comment 4)

Changed as suggested

36. I would remove in l. 463 the wind direction change, since it is not that relevant for your findings and rather distracts.

We prefer to keep the mention of the wind direction change, as it represents a key physical feature of stable boundary-layer dynamics with critical implications for experimental setups.

As discussed in Section 6.3 (*Implications for experimental studies*), wind direction shear and directional shifts significantly impact observational strategies in field campaigns, particularly for aligning scanning LiDARs, orienting meteorological masts, and tracking wind turbine wake deflection.

37. Move sentence in l. 483 to discussion. The conclusions should not include new references.

Removed as suggested from the conclusion and move to the discussion.

<u>Technical remarks</u>

1. At several locations the in-text reference includes brackets around the authors, which should not be the case, e.g. l. 162

Corrected as suggested

2. I would recommend to check the whole manuscript again for spelling and grammatical errors. In the following I will only mention a selection.

3. l. 13 “thermal variation” → “temperature differences/changes”

Replaced by “temperature changes”

4. l. 30 remove “other”

Corrected as suggested

5. l. 31 “species diversity” → “diversity in species”

Corrected as suggested

6. l. 39 “in order to” → “to” (also change at other locations)

Corrected everywhere as suggested

7. l. 39 “this phenomena” → “these phenomena” or “this phenomenon”

Replaced by “this phenomenon”

8. l. 43 “micro scale” → “micro-scale”

Corrected as suggested

9. l. 44 remove “new”

Corrected as suggested

10. l. 51/52 only “LES”/ “TKE”; abbreviation was already defined in l. 43/42

Removed as suggested

11. l. 58 “large-scale simulations”: do you mean low fidelity in this context? Large-scale can still be a high resolution and explicit calculation of TKE.

Yes, we initially avoided using this term because we were concerned that it might have a negative connotation. However, this is indeed what we intended to say. We change it as suggested.

12. l. 62 “direction” → “wind direction”

Corrected as suggested

13. l. 63 remove “through the atmospheric and surface properties”

Removed as suggested

14. l. 68/72 define abbreviation EOL/WRF

EOL is not an acronym but the name of the actuator module in Meso-NH. The definition of the WRF acronym has been added: “with the WRF (Weather Research and Forecasting) model.”

15. l. 74 “it simulate scales” → “multiple scales can be simulated”

Changed as suggested

16. l. 82 “multiple turbines within the farm” → “a wind farm”

Changed as suggested

17. l. 101 “withing” → “within”

Corrected as suggested

18. l. 101 remove “agricultural landscape”

Removed as suggested

19. l. 101 move the sentence “We selected…” to the start of section 2.1

Moved as suggested

20. l. 106 “arranged” → “irregularly arranged”

Changed for “irregularly arranged in approximately”

21. l. 107 “consistent with the prevailing regional winds” → “perpendicular to the prevailing regional winds”

Changed as suggested

22. l. 111 “which are predominantly […]” is doubled with l. 113-114

Replaced by “mainly” to avoid repetition

23. l. 112 “orographic effects” → “orography”

Changed as suggested

24. l. 114 “scrubland” → “shrubland”

Changed as suggested

25. l. 126 define abbreviation AOC

The French acronym AOC has been defined in both French and English: “(Appellation d’Origine Contrôlée; French protected designation of origin)”. In addition, “of vineyard areas” has been added after “70%” to clarify precisely what this number refers to.

26. l. 239 When “later”? I suggest to mention the actual times.

The sentence revealed an imprecision: the arrival of the maritime fronts occurred later on 8 April. The sentence has therefore been modified as follows:

“During the late afternoon of 7 April, changes in wind speed and direction, together with sunset, marked an increase in atmospheric moisture.”

27. l. 246 The solid line does not show the nocturnal times → remove “solid line” and in l. 348 “dotted line”

Indeed, the position of the parenthesis associated with the solid line was incorrect. The solid line corresponds to air temperature, whereas the dotted line corresponds to surface temperature. The parenthesis has therefore been moved to the next line.

28. ll. 262-263 remove the sentence “Wu …”

Sentence removed

29. Subsection headers 4.2 & 4.3: “condition” → “conditions”

Corrected as suggested

30. l. 288 “lower bottom”: better to use axis coordinates to describe the location

Changed as: “at low altitude on the left side of vertical cross”

31. l. 316 “twice less power” → “half of the”

We propose to further improve clarity: “whereas turbines located within the interior produce approximately half as much power.”

32. l. 317 “downstream of the wind farm” → “at southern wind turbine locations”?

Indeed, since we are referring to near-surface wind, which differs from the wind at hub height, the term “downstream” was not appropriate. We have corrected as suggested.

33. l. 334 “80” → “80 m”

Corrected as suggested

34. l. 344 “modification” → “wind farm-induced modification”

Corrected as suggested

35. l. 345 remove or rewrite sentence “Therefore …” since the term “therefore” does not fit to the previous sentence

Changed as: "**The net** contribution of the wind farm has to be evaluated by comparing the two curves.”

36. l. 376 remove “differentiate”

Changed as suggested

37. l. 386 “flow heterogeneities” instead of “local atmospheric flows”?

Changed as suggested

38. l. 454 “the numerical simulations highlight” → “the results highlight”

Changed as suggested

39. l. 462 “These near-ground warming anomalies…” does not fit, since the previous sentence was on the wind speed.

Inversion of the two sentences: “These near-ground warming anomalies are advected by the local wind which, in the present case, differs by nearly 30–50° from the wind direction at hub height. Variation of near ground wind speed is also noted with zones of acceleration and deceleration associated with blockage effects of wind turbine.”

40. l. 468 “atmospheric circulation” → “atmospheric flow”

Changed as suggested

41. Most plots that include longitude/latitude on the x-/y-axis miss the axis label (e.g. Fig. 8) → add

The latitude and longitude were added to the axes where they were missing.

42. Table 1: put header above the table and add to the header the definition of the variables mentioned in the table.

Changed as suggested

43. Fig. 5 caption: “vertical” → “horizontal”, “upper/lower line” → “upper/lower row”, “son domain” → “child domain”

Thank you very much. I must have written the caption late in the evening…

44. Fig. 5 add the time zone to the times

UTC has been added after each time indication.

45. Use the same colorbar orientation for the wind speed of Fig. 5 and Fig. 6(a,b)

Changed as suggested

46. Fig. 6: put the text in the plot in a box with a light background, so it is readable. Colorbar label “HR” → “RH”

Changed and corrected as suggested

47. Fig. 7: use thicker and/or longer lines for WT locations

Thicker line for WT

48. Fig. 10 & l. 346 “temperature variation” → “temperature difference”

Changed as suggested

49. Fig. 11: be consistent with the y axis tick labels. Either include tick labels both for (b) and (c), or for none of them. For (c) include tick marks. Also, add a legend with “--- convergence line”

The y-axis tick labels have been removed when necessary to be consistent across all plots. Tick marks have also been added. In addition, legends indicating the convergence line have been added to Figures 12 and 13.

50. Fig. 11(b) header “Mean value along x” → “T along y”, otherwise I would expect one axis to be the x-coordinate; same for Fig. 12 (b) & Fig. 13 (b)

Changed as suggested

51. Fig. 11 caption & l. 357: “dotted” → “dashed”

Changed as suggested

52. Fig. 12 caption: “x-axis” → “y-axis”

Changed as suggested

53. In Fig. A1 preferably display rotor area by a shaded area. If it is included in chapter 4, an average over all times would also be sufficient, so only one line per subplot.

As suggested a shaded area has been added to indicate the rotor-swept area.

We agree that a time-averaged profile would be easier to read. However, we believe that, given the dynamical nature of the atmosphere, showing several profiles at different times provides a more representative picture of the actual atmospheric conditions experienced by the wind turbines throughout the simulation. A time-averaged profile would smooth out the temporal variability and could mask important variations in atmospheric stability and wind conditions. We therefore chose, for now, to retain several profiles to better illustrate the changes in atmospheric conditions while the turbines are operating.

References

Wu, Y.-T., & Porté-Agel, F. (2011). Large-Eddy Simulation of Wind-Turbine Wakes: Evaluation of Turbine Parametrisations. Boundary-Layer Meteorology, 138, 345–366. https://doi.org/10.1007/s10546-010-9569-x

This reference was added, following a comment from reviewer 1.