Review for the study "Multi-scale simulations of wind farm impacts on nighttime near-surface temperature in complex terrain"

Summary

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.

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.*

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.*

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*.

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.
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). 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.

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.
   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.
   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.
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.

<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 U_h or U_{tot} instead of WS and adding it to the figure captions.
   b) Instead of using the absolute wind speed, the normalised might be more meaningful. E.g. in fig. 9 with respect to l. 339.
   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.
   d) In most figures I would add more terrain contours, especially in fig. 12.
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?

b) ll. 39 “To investigate this phenomena and its mechanisms”: Which phenomenon and mechanisms?
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?
d) l. 46: What do you mean by “natural atmospheric stratification”? The stratification present in the background flow?
e) l. 271: Which circulation do you mean? The large-scale wind direction change?
f) For the whole conclusions specify when you refer to air temperature and when to ground temperature.
g) In l. 463, what do “these atmospheric perturbations” refer to?
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”

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.
   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.
   c) The context of frost protection methods in ll. 90-91 would fit better earlier in the introduction, e.g. around ll. 25-33.
4. ll. 25-26 “those […] temperatures and increased vertical mixing”: Vertical mixing was so far not mentioned.
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.
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.
7. In the methods, the governing equations of the model should be included.
8. l. 105: How many turbines each?
9. l. 121-122 “Minimum temperatures close to the ground (50 cm) reached values down to -8°C”: What is the source?
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.
11. What do you mean with “metric scales” (l. 141)? Do you mean “scales of a few metres”?
12. In section 3.1/table 1 I would specify for each domain the exact simulated times of the case study.
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.
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.
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?

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.
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.
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.
    b) I would not mix title and legend, i.e. where needed include the linetype in a separate label of each subplot.
    c) For the cloud fraction (3d)) specify whether measurement/D2/D3. Probably D2, right?
    d) The axis label in 3h) states “heat flux”, but short wave up and down refers to radiation, right? Should be changed.
    e) Is it correct that no measurements for short wave up exist?
    f) The figure caption states that the short-wave radiation is plotted for D2 and D4, while the legend says D2 and D3.
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.
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.
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?
22. l. 280/Fig. 6: How did you decide on the segment AB?
23. For section 5.1 I would choose a different header, since not only veer is described. E.g. “Wake deficit and power production”
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)
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.
26. Fig. 8: How is the power ratio calculated?
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.
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.
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”.
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?

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.
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.
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.
35. In l. 465 only include conclusion on turbulent heat flux if you actually analysed it (see major comment 4)
36. I would remove in l. 463 the wind direction change, since it is not that relevant for your findings and rather distracts.
37. Move sentence in l. 483 to discussion. The conclusions should not include new references.

Technical remarks

1. At several locations the in-text reference includes brackets around the authors, which should not be the case, e.g. l. 162
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”
4. l. 30 remove “other”
5. l. 31 “species diversity” → “diversity in species”
6. l. 39 “in order to” → “to” (also change at other locations)
7. l. 39 “this phenomena” → “these phenomena” or “this phenomenon”
8. l. 43 “micro scale” → “micro-scale”
9. l. 44 remove “new”
10. l. 51/52 only “LES”/ “TKE”; abbreviation was already defined in l. 43/42
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.
12. l. 62 “direction” → “wind direction”
13. l. 63 remove “through the atmospheric and surface properties”
14. l. 68/72 define abbreviation EOL/WRF
15. l. 74 “it simulate scales” → “multiple scales can be simulated”
16. l. 82 “multiple turbines within the farm” → “a wind farm”
17. l. 101 “withing” → “within”
18. l. 101 remove “agricultural landscape”
19. l. 101 move the sentence “We selected…” to the start of section 2.1
20. l. 106 “arranged” → “irregularly arranged”
21. l. 107 “consistent with the prevailing regional winds” → “perpendicular to the prevailing regional winds”
22. l. 111 “which are predominantly […]” is doubled with l. 113-114
23. l. 112 “orographic effects” → “orography”
24. l. 114 “scrubland” → “shrubland”
25. l. 126 define abbreviation AOC
26. l. 239 When “later”? I suggest to mention the actual times.

27. l. 246 The solid line does not show the nocturnal times → remove “solid line” and in l. 348 “dotted line”
28. ll. 262-263 remove the sentence “Wu …”
29. Subsection headers 4.2 & 4.3: “condition” → “conditions”
30. l. 288 “lower bottom”: better to use axis coordinates to describe the location
31. l. 316 “twice less power” → “half of the”
32. l. 317 “downstream of the wind farm” → “at southern wind turbine locations”?
33. l. 334 “80” → “80 m”
34. l. 344 “modification” → “wind farm-induced modification”
35. l. 345 remove or rewrite sentence “Therefore …” since the term “therefore” does not fit to the previous sentence
36. l. 376 remove “differentiate”
37. l. 386 “flow heterogeneities” instead of “local atmospheric flows”?
38. l. 454 “the numerical simulations highlight” → “the results highlight”
39. l. 462 “These near-ground warming anomalies…” does not fit, since the previous sentence was on the wind speed.
40. l. 468 “atmospheric circulation” → “atmospheric flow”

41. Most plots that include longitude/latitude on the x-/y-axis miss the axis label (e.g. Fig. 8) → add
42. Table 1: put header above the table and add to the header the definition of the variables mentioned in the table.
43. Fig. 5 caption: “vertical” → “horizontal”, “upper/lower line” → “upper/lower row”, “son domain” → “child domain”
44. Fig. 5 add the time zone to the times
45. Use the same colorbar orientation for the wind speed of Fig. 5 and Fig. 6(a,b)
46. Fig. 6: put the text in the plot in a box with a light background, so it is readable. Colorbar label “HR” → “RH”
47. Fig. 7: use thicker and/or longer lines for WT locations
48. Fig. 10 & l. 346 “temperature variation” → “temperature difference”
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”
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)
51. Fig. 11 caption & l. 357: “dotted” → “dashed”
52. Fig. 12 caption: “x-axis” → “y-axis”
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.

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