Responses to referee #1

> The authors implemented substantial changes in the manuscript. Overall, the manuscript has been improved. However, a few critical changes and a few other minor changes should be done before publication. Some of the points were already mentioned in the 1st review but not corrected properly..

Thank you for your constructive review! In this revision, we carefully revised our manuscript to address your concerns. The bellows are out point-to-point response.

Critical comments (general):

> In the AD+R and ALM the momentum theory is not used, because this part is taken care by the CFD simulation itself (corresponding mainly the effect of the turbine induction), and therefore it is wrong to say that they use the BEM theory (where the M stand for the momentum theory), they only use the BE theory. In the text, many wrong references to « BEM » are still present. This was already discussed in the 1st review. The text must be modified accordingly, e.g. lines 25,63,212,255. Also, « BE theory » seems a better formulation than « BMT » as proposed line 25.

Thank you for your comments. We follow your suggestion of using abbrevation "BE theory". Upon careful examination of the methods employed in WRF-GAD and PALM, we have noted that while the PALM model uses the BE theory, the WRF-GAD model uses the Blade Element Momentum (BEM) theory, which combines the BE theory with momentum theory through induction factors. For instance, in WRF-GAD paper (Mirocha et al. 2014), the authors explicitly state that "The GAD model implemented into WRF follows the generalization of the Blade Element Momentum theory of Glauert...". In essence, their model integrates stream tube dynamics (momentum theory) with blade aerodynamics (BE theory) (See their apprendix). Thus, we have revised the entire paragraph and other places on the paper to provide clarity on this matter.

Ref: Mirocha, J. D., Kosovic, B., Aitken, M. L., & Lundquist, J. K. (2014). Implementation of a generalized actuator disk wind turbine model into the weather research and forecasting model for large-eddy simulation applications. *Journal of Renewable and Sustainable Energy*, 6(1).

> The idea that the simple AD (SAD) has weaker resolution requirements than the AD+R is not supported by the literature. In fact, the authors already agree with this, since they write « at least a few grid points across the rotor » for the AD+R (line 64), which is exactly the resolution targeted for the SAD in this paper (tens of meters). This argument can therefore not be used. The argument regarding the availability of the turbine data (line 65-66) is correct and sufficient. The text must be modified accordingly, e.g. lines 72,474.

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Thank you for your comments. We removed the argument as you suggested and revised the relevant paragraphs. We put the notions about the resolution in these two sentences: "While many WRF-GAD applications typically employ fine resolutions of a few meters (e.g. Mirocha et al., 2014; Arthur et al., 2020; Kale et al., 2022), such high resolutions are computationally expensive for realistic downscaling problems involving large domains with multiple wind farms. Therefore, WRF-SADLES is also tested with coarser resolutions (specifically, 30 and 40 meters) to achieve a more practical balance between computational cost and wake resolution".

> Multiple studies have shown that the rotation is not critical in LES with Actuator Models. This was already discussed in the 1st review. The critical difference between the SAD implemented here and the AD+R used in the PALM simulations is the force distribution on the rotor (homogeneous or heterogeneous respectively) and the way these forces are computed (using the thrust coefficient or the BE theory respectively), not the rotation. A SAD like model can have rotation included using the power coefficient, and we could imagine an Actuator Disk model based on the BE theory but without rotation (so similar to the AD+R of PALM but without rotation). The text must be modified accordingly, e.g. lines 114-115,246-247,477-478. Line 242, remove « (e.g. simple actuator disk versus actuator disk with rotation) », not needed and most likely not the most important point.

> The paragraph lines 111-116 is still very confusing. Referring to my 1st review: « The idea expressed in lines 113-118 [lines from V1], that the rotation affects the wake recovery which justify the use of an additional subgrid-scale turbulence term since the rotation is not explicitly included, is not correct. ». In fact, the authors already agree with this, in lines 443-454. In this context, lines 443-448 are well adapted and lines 111-116 should be adapted using lines 443-448.

> I would suggest moving lines 443-448 to lines 111-116, lines 449-454 at line 298, and lines 455-467 at line 397 (with the appropriate modification needed).

We thank you for your clarification and comments. We carefully revised the paper related to your comment as follows:

- We moved the discussion about this from lines 443-448 to lines 111-116 and revised the text (Lines 116-120 in revised version).

- In Line 242, we removed « (e.g. simple actuator disk versus actuator disk with rotation) » as suggested.
- In lines 246-258, we change the text from "...the absence of the rotational effect in WRF-SADLES, which is included in PALM.", to "...how the thrust forces are calculated differently in the BE method (PALM) and the momentum theory (WRF-SADLES)."
- We moved lines 449-454 to line 298 and adapted the text as suggested (Lines 291-294 in revised version)
- We removed "which incorporates rotation in its actuator disc model, providing a more comprehensive representation" in lines 477-478, replaced by "whose actuator disc model uses the blade element theory, providing a more comprehensive representation".
- We moved Lines 455-467 to line 397 with modification to adapted to the text flow (Lines 376-387).

> In section 3.2, Lines 228-252 and 265-279, make strong statements which are difficult to justify based only on contours, e.g. « slower wake recovery » line 231. Profiles are better suited for these types of statements. To solve this issue, the profiles (Figs 5 and 7) could be presented at the same time as the contours (respectively Fig 4 and 6) and discussed together. Also, the profiles for the induction optimisation should be added (contours could be removed if needed, profiles are more important than contours for a precise discussion). Fig 3 and 4 could be merged and discussed all together.

Thank you for your suggestions! As suggested, we merged Fig. 3 and 4 together and added wind deficit profile to Fig. 5 (Fig. 4 in the revised version). We carefully chose the lines colors to make sure that they are color blind friendly. After that we revised the whole section as suggested (i.e. discuss the contour and profile figures together). We believe the text becomes more concise and comprehensive!

Critical comments (specific):

> Lines 231-232: « indicating potentially stronger turbulence activities », in fact, it is the opposite, slower recovery and smaller rate of wake expansion are in general associated with lower background turbulence intensity. Must be corrected or reformulated

In our original text, it is "indicating potentially stronger turbulence activities at lower resolutions.", which is not incorrect, but easily causes confusion. Nevetherles, this is removed in our revised text.

> Line 27: « rotating circular disk », not appropriate, must be corrected.

We replaced the phrase by "permeable disc"

> Line 145: « within the interval […] chosen boundaries », unclear, must be reformulated.

For clarity, we extend the sentence to "The method introduces a random perturbation of potential temperature within the interval of -0.5K to 0.5K to three cells near the inflow boundaries. Each cell is an 8x8 grid points square in the horizontal plane, and the same perturbation is applied to each cell. Therefore, the total perturbation zone extends 24 grid points from the inflow boundaries."

> Line 233-234. The idea behind the sentence is very strange formulated as such, change « which is used » with « for ».

We changed the text as suggested.

> Lines 275-276: « shear production », not appropriate, Line 283 « shear production of turbulence », not appropriate. Must be reformulated.

We changed the phrases to "increased wind shear at these boundaries" and "turbulence production associated with the wake shear" respectively.

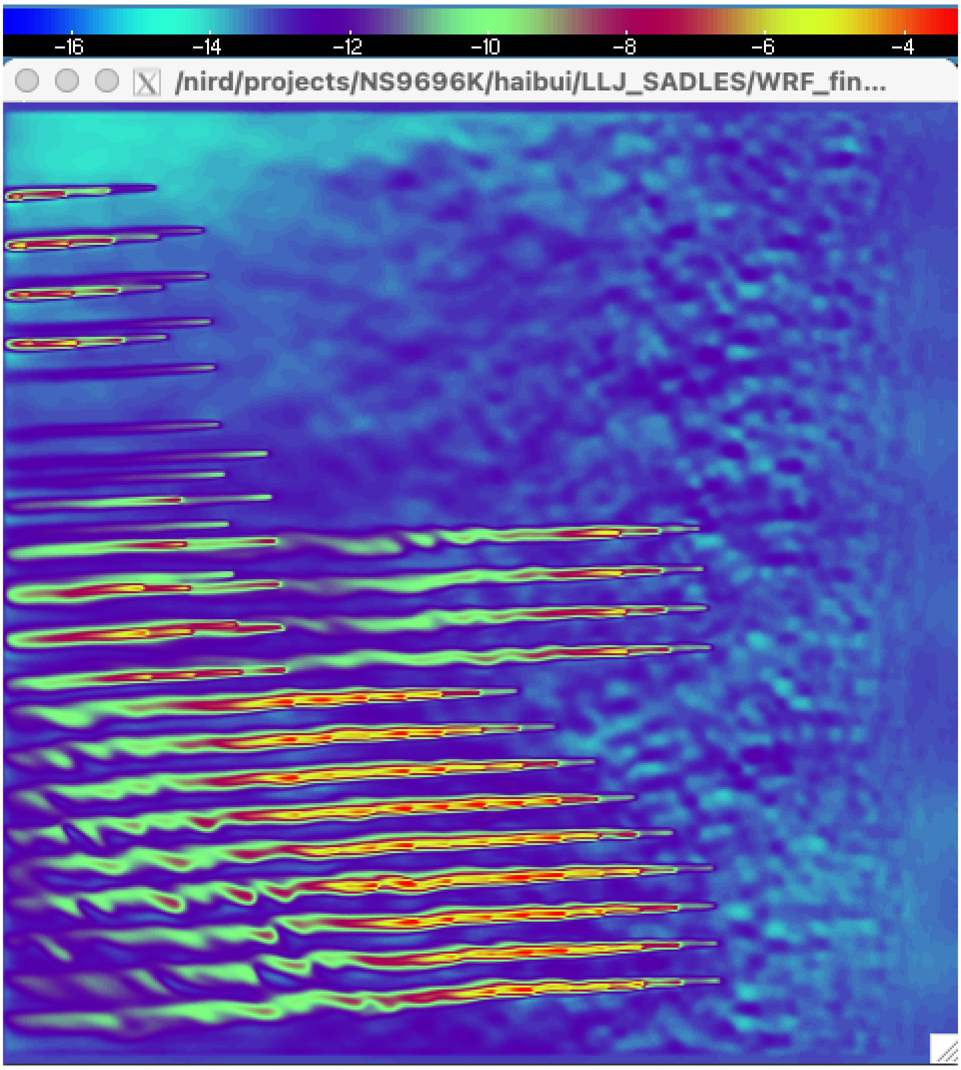
> Lines 301-302: not clear how the spectra are computed, from what I understand, not sure it is appropriate for the targeted goal. Clarify.

We shorten these discussions and move them to the beginning of Section 3.2 to briefly compare the turbulence characteristic of PALM and WRF at the two resolutions, showing that the two models are comparable. We added more explaination to the caption of the Figre (Now Fig. 3).

> Lines 405-406, Fig 12: I am not convinced by the « limited resolution » argument (referring to spatial resolution I guess), the flow from the simulation looks like there is no turbulence, any comments on that?

Yes, we mean the spatial resolution. As showin the Fig. 3, coarser solution under resolve the turbulence for small eddies. For clarity, we revised the sentence as "This is likely attributable to the coarse resolution to capture the small-scale turbulence explicitly."

Note that the figure is zoomed in to compare with the LiDAR. Here is the snapshot of zonal wind without clipping.



> Lines 482-483, « independence from model resolution »: does not seem supported by the results and does not seem correct, must be changed/removed.

We removed the argument as suggested.

> Fig 1: the vertical resolution in Fig 1.a. still do not match the legend (dz=30m) and Fig 1.b. This was already discussed in the 1st review.

Thank you for point it out again (we are a little bit confused in the 1s review)! Yes, there is a missmatch, dz should be 20m instead and we corrected in the caption.

> Fig 10: remove « alongside», Fig 11: modify second sentence of caption, with « , data from […] » similar to caption of Fig 10.

We editted the captions as suggested.

Additional comments:

> Line 3: « downscaling of large eddy simulations », not so clear in the context.

We chanced the sentence to "The WRF-SADLES model supports both idealized studies and realistic applications through downscaling from real data, with a focus on resolutions of tens of meters."

> Line 20: « dynamic loading », no so clear in the context, not really needed. Line 36: « such as cyclones an fronts », no so relevant, not really needed. Lines 57-58: « (Fitch et al., 2012) only requires », replace with : « the method used by Fitch et al. ».

We eddited the points above as suggested.

> Line 74: « the LES downscaling approach », formulation could be improved.

We changed "LES downscaling" to "meso-to-micro downscaling" as it's consitent with our previous argument and more relevant.

> Line 75: « necessary for generating turbulence », replace with something like « allows faster development of turbulence ».

We eddited the phrases as suggested.

> Lines 79-80: could be improved, e.g. with something like « an idealized case is used to compare the simulation results from WRF-SADLES with PALM ».

We revised the sentence, keeping the active voice, to "we initially conduct a comparison using an idealized case, evaluating the simulations of a 5-MW wind turbine from WRF-SADLES against those from the PALM model."

> Lines 89-90: does not look appropriate with Section 5. Section 6 is missing.

As we restructred the paper (Remove Section 5 as suggested), we revised this paragraph including the Conclusion section (now Section 5).

> Lines 97-98: « The wind speed at the rotor […] », introduce this at line 108, before actually using the wind speed at the rotor in the equation.

Thank you for your comment, for a more logical flow, we change the euation for V = V0(1-a) to V0=V/(1-a) and move it down to before equation (5) and (6).

> L115: « we adopt », replace with « we test ».

This phrase is removed in our intensive revison of the section.

> L 144: « improve the realism of turbulent representation », not appropriate.

We changed the phrase to "accelerate the development of turbulence within the nested domain"

> L172-173: « models, known for their realistic simulation », not clear (we could think the statement also includes WRF-SADLES), improve formulation, could be simply removed.

We removed the phrase as suggested.

> L173-174: indicate also the resolution with respect to the rotor diameter », i.e. how many points per rotor diameter.

We added the information (about 12 and 4 grid points per rotor diameter)

> Line 184: « an aspect ratio of 2:1 », add the directions (could be vertical at this moment in the text).

We changed it to "a horizontal aspect ratio..."

> Line 193: « zonal direction », not clear

For clarity, we added explanation "zonal (eastward) direction"

> Line 207: reference in bad format.

We fixed the format.

> Line 222: express time in hours like for the WRF simulation.

We revised the whole section that described the PALM simulation, addressing a number of issues including this point.

> Line 230: « suggesting a dependency of momentum fluxes on model solution », not sure to follow, may need more explanation or reformulation. Line 232: « conversely », not adapted. Line 241: « methodological », maybe « numerical »? Line 245-246: improve, e.g. with something like « PALM simulation exhibits stronger wake expansion than the WRF simulation ». Line 250: « but one possibility is related », not very elegant, could be improved. Line 252: « (2D) », remove parenthesis. Line 253: « agree around 50% », not adapted, remove. Line 280: « vertical structure », modify with « «vertical profile ». Line 290: « brings them closer », not elegant.

As this whole section is revised thoroughly acording to the point above, these phrases are no longer presented in the new version.

> Line 388: « LLJ) », remove parenthesis.

We removed as suggested

> Line 398: »UTC., », remove « . ».

We editted as suggested

> Line 416: « become fully developed after about two kms », justify the statement or make less precise statement.

We make a less precise statement by changing two kilometers by a short distance.

> Line 452: « we imply », not appropriate.

We changed "imply" to "propose"

> Line 465: « model crashes », not so elegant.

We changed it to "model failures"

> In figures 4,6,7, « similar than in fig […] », copy-paste the appropriate description, each figure should be self descriptive.

We revised the figures captions and made them self-descriptive.

> Fig 5, « evaluation point » maybe not so clear (not really a point since it's a profile), « location » could be better.

We changed "point" to "location" as suggested.

> Fig 12, « radial wind speed », not so clear to me in this context.

For clarity, we swap the panel of the figure and change the caption to "(a) Line-of-sight (LOS) velocity from horizontal LiDAR scans at the FINO1 station around 23:00 UTC. (b) Simulated LOS velocity from from WRF-SADLES at 20:00 UTC."

> Fig 14: the layout in panel in (b) could be incorporated in Fig 9, and the y axis of Fig (a) and (b) could be rescaled.

Thank you for your suggestion. We incoporated the turbine layout to Fig. 9 (Now Fig. 8). Regarding the vertical scale of Fig.12. (now Fig. 11), we would like to keep as the current with the minimum on 0 to reflect the variation with their actual magnitude.

> Table 2: « real-data », not very elegant.

We changed "real-data" to "realistic"

Remarks/questions:

> Why only potential temperature in the perturbation method and not the wind speed components?

As explained in Muñoz-Esparza et al. 2014, the idea of cell perturbation of potential temperature is to trigger a formation of microscale three-dimensional motions, not to impose a developed turbulent field (i.e. perturbed wind speed components). Because this is not the main point of the paper, we avoid dicuss it further in the manuscipt.

Ref: Muñoz-Esparza, D., Kosovı̆ c, B., Mirocha, J., and van Beeck, J.: Bridging the transition from mesoscale to microscale turbulence in numerical weather prediction models, Boundary-layer meteorology, 153, 409–440, 2014

> Why using a precursor simulation in PALM and no precursor in WRF simulation ?

We added "To achieve a quasi-equilibrium state of turbulence, a specialized LES model such as PALM typically conducts a precursor run, often using a smaller domain to minimize computational costs. However, this precursor technique is not supported within the WRF model. Instead, we opt for a spin-up period of 20 hours solely in the outer domain." to the paper to clarify our option.

Responses to referee #3

> Overall, authors addressed the comments and improved the manuscript. This article can be directly accepted after they fix the radiation issue.

Thank you for reviewing our paper. We cafefully revised the relevant paragraphs to rationalize our choices. The reason for turning of radition scheme is provided in Lines 199-203. "In idealized LES, the focus is on resolving the turbulent structures within the domain. To achieve this, non-essential physical parameterization schemes, including microphysics, cumulus convection, and radiation, were disabled. The effect of surface radiation on the development of turbulence is represented by a surface turbulence heat flux of $\overline{(\theta' w')_s} = 0.02$ K m s^{-1}, similar to some previous studies (Muñoz-Esparza et al., 2014; Kale et al., 2022). This presents a weak convective boundary layer.