Responses to reviewer #1

> The work focuses on downscaling simulations for wind energy applications, and presents an implementation of the standard Actuator Disk Model (ADM-std) and the perturbation method (PM) in WRF-LES. In this context, two cases are presented. First, an idealized single turbine case is presented, where some options are tested. These simulations are compared with PALM simulations, serving as a pseudo-validation. Secondly a downscaling of the ERA5 reanalysis data around the Alpha Ventus wind farm is presented.

Thank you for your insightful review! Your suggestions have greatly contributed to the substantial and thorough revision of our manuscript. In response to your recommendations, we have incorporated new analyses, including an assessment of turbulent intensity (TI), and introduced additional idealized experiments to explore the impact of the subgrid-scale TKE factor. Furthermore, we have included a brief comparison with observational data from the FINO1 station and introduced a new discussion section to address the validity of the added subgrid-scale TKE and the limitations associated with the meso-to-micro scale transition resolution. We are confident that these revisions have significantly enhanced the manuscript compared to its initial version and trust that they adequately address your concerns. Below, we provide detailed responses to each of your points.

> 1. The manuscript has two repeated copy-pasted portions, one from line 59-65/66-72 and another from 256-259/260-264.

We greatly appreciate you bringing these typographical errors to our attention, which arose from internal revision oversights. We have removed the inappropriate parts and revising the remaining content.

> 2. Multiples figures have major issues. In Figure 1b the axes units are marked as kilometres when it should be meters. Also, the grid shown in Figure 1a makes no sense. Figure 3,5,9,10 don’t have proper legends on the colormap.

We carefully checked all the figures. For Figure 1a, we've noted in the caption that it's just for illustration (so the vertical grid size stays the same while the WRF grids stretch). We also added a wind turbine drawing for clarity.

> 3. The actuator model implemented is not new in itself and the novelty is only in its implementation in WRF-LES. The text should be very clear with this. This is often not clear e.g. lines 2-3. Also, each time SADLES is used in the text, it should be replaced by WRF-SADLES, e.g. line 7, or eventually WRF-LES-SAD, which seems more appropriate

Thank you for your suggestion; we have revised the text accordingly. For instance, in the abstract, we have changed from "...introduce a new wind turbine model, the Simple Actuator Disc for Large Eddy Simulation (SADLES)," to "...present our implementation of a Simple Actuator Disc model for Large Eddy Simulation (SADLES)". We now use 'WRF-SADLES' instead of 'SADLES', except in a few instances where it's important to differentiate between the WRF and SADLES models. Regarding the abbreviation, We kindly request to maintain the name 'WRF-SADLES' instead of 'WRF-LES-SAD' due to our preference.

> 4. In the community, BEM stand for Blade Element Momentum theory, which is not the same as the Blade Element theory alone which is used in AD+R and ALM, the text should be corrected to avoid any confusion, e.g. line 23,67, etc.

Thank you very much, we have check addressed in the text the avoid any confusion.

> 5. The idea expressed in lines 113-118, 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. Multiple studies have shown that the rotation is not important in LES with Actuator Models. The wake recovery mainly depends on the interaction between the wake with the incoming flow turbulence, including the turbulence generated by the wake shear itself, which means of course on how the simulation capture this accurately, in which the numerics, the sgs model, etc. play a critical role. Adding an additional artificial subgrid-scale turbulence term is fine, but it should be presented as so. Please mention the fTKE used.

Thank you for your valuable suggestion. To tackle this issue, we conducted four new additional experiments to explore the impact of the added TKE. The results indicate that the inclusion of subgrid-scale TKE does not significantly influence the outcomes. This added term can be managed in WRF-SADLES through the 'sadles_tkefact' option, for which we have provided the recommended value (sadles_tkefact=0) in the manuscript. We also discussed the appropriateness of the term (C_t-C_p) in the new Discussion section.

> 6. The strong use of instantaneous flow to analyse the flow is inappropriate. The instantaneous flow can be used for illustration but not for stong flow analyses and conclusions. For example, the statement lines 226-227 make no sense. Also, the statement lines 311-314 has no base/proof. In the same time, turbulence intensity plots are missing. Figures 5,6 and 10 must be reproduced with turbulence intensity.

We appreciate your suggestion! We have implemented changes accordingly. For the idealized cases, we removed the instantaneous plots and included analysis and discussion of turbulent intensity (TI). In the case of realistic data, we conducted a new case study, compared it with observations, and introduced three new figures. Regarding the content on farm-to-farm example, we retained the instantaneous snapshots for visual illustration and combined four

figures from the previous version into two. We aim to keep this section concise since there's no reference simulation for comparison, and it primarily serves as an illustrative example of WRF-SADLES application.

Responses to reviewer #2

> This manuscript presents a new wind turbine parameterization model called SADLES for WRF model, which strikes a balance between the accuracy of the GAD model and the computaonal efficiency of the WFP models. SADLES only requires power and thrust curves, which are already available in WRF. They validate the effectiveness of SADLES with PALM and also demonstrate a more realistic application by downscaling reanalysis data to investigate turbine-to-turbine and farm-to-farm interactions.

Thank you for your review! We've incorporated your suggestions and substantially improved our manuscript. This includes new experiments, analysis of turbulent intensity (TI), and a brief comparison with observational data. Additionally, we've added a new discussion section addressing the validity of the added subgrid-scale TKE and the meso-to-micro downscaling method. We believe these revisions greatly enhance the manuscript and adequately address your concerns. Below, we provide detailed responses to your points.

> The authors used SADLES and WRF-SADLES interchangeably in the manuscript. They should settle on using only one of the two names.

Thank you for your suggestion. We have editted the manuscript as suggested.

> It is not clear on which option should users chose for the direct / inferred evaluation estimations of axial inducion factor (a). Option 1 is used for the realistic application in the manuscript. Option 2, however, is more appropriate where the resolution is at a few hundred meters. It'll be great to have some recommendations to use each Options.

We have carried out additional experiments and provided the recommendation options in the text. Specificly, The recommended options is: Option 2 for axial induction factor (Line 478), and f_TKE=0 for the added subgrid-scale TKE (Line 483).

> Figure 2 caption: "Power, thrust coefficient …" should read "Thrust coefficient …" There are 2 red stars, one is FINO1, the other is not defined on Figure 8a.

We have clarified the text in Fig.2's caption. Regarding Fig. 8, we have replotted by removing the other star and revised the caption.

Responses to reviewer #3

> The paper titled "Implementation of a Simple Actuator Disc for Large Eddy Simulation (SADLES-V1.0) in the Weather Research and Forecasting Model (V4.3.1) for Wind Turbine Wake Simulation" introduces SADLES as a wind turbine model in WRF. The study aims for realistic downscaling of large eddy simulation and focuses on wind farm assessment. The major concerns revolve around the perceived lack of novelty, absence of radiation considerations, brevity in the discussion, and reliance on instantaneous flow analysis. Minor issues include duplicated text portions and unclear figure captions.

Thank you for your review! We've integrated your suggestions, resulting in significant enhancements to our manuscript. This encompasses the addition of new experiments, analysis of turbulent intensity (TI), and a concise comparison with observational data. Furthermore, we've introduced a new discussion section that delves into the validity of the added subgrid-scale TKE and the meso-to-micro downscaling method. Alongside this, we've included a newly structured Discussion section. We are confident that these revisions elevate the quality of the manuscript and effectively address your concerns. Please find detailed responses to your points below.

> 1. Novelty Clarication. The paper should provide a clearer description of its novelty, particularly in the implementation of the actuator model in WRF-LES. Highlighting the unique aspects of this implementation would strengthen the paper's contribution.

Thank you for your suggestions. While WRF-SADLES is rooted in the traditional actuator disc model, its novelty lies in its integration within the widely utilized WRF model in atmospheric science. To clarify this distinction, we have adjusted the language in the abstract and conclusion, shifting from "...introduce a new wind turbine model, the Simple Actuator Disc for Large Eddy Simulation (SADLES)," to "...present our implementation of a Simple Actuator Disc model for Large Eddy Simulation (SADLES)." This modification aims to prevent any potential confusion.

> 2. Radiation Consideration. The absence of radiation in the study, a crucial component in models like PALM, should be discussed as a limitation. It's recommended to reference relevant radiation-related papers (https://doi.org/10.5194/gmd-15-145-2022 and https://doi.org/10.5194/gmd-14-3095-2021) to support this point.

Thank you for your comments. Radiation is indeed a critical component in weather numerical models, alongside factors such as cumulus and microphysics. In our realistic data, we employed the traditional scheme (RRTMG) as detailed in the text. However, for standard idealized LES simulations, we disabled radiation and other physical processes to simplify the setup and facilitate result interpretation. In fact, the impact of radiation in our idealized experiments is indirectly accounted for through the idealized surface turbulence heat flux. While the role of surface turbulence heat flux on turbulence and wake properties is an important and intriguing topic, it falls beyond the scope of our developmental paper.

> 3. Discussion Depth. The discussion section is noted to be brief and super cial. Expanding this section to delve deeper into the implications and signi cance of the results would enhance the overall quality of the paper.

Thank you for your valuable suggestion. We've carefully reviewed the paper and added a new Discussion section to explore aspects of WRF-SADLES, including the use of subgrid-scale added TKE and the transition resolution of the downscaling method.

> 4. Instantaneous Flow Analysis. Authors are advised not to rely solely on instan- taneous flow-related analysis, emphasizing that in LES, such instantaneous flows may lack meaningful interpretation. Discussing the limitations and considerations regarding the choice of analyses would strengthen the paper.

We appreciate your suggestion. We have removed the plot and discussion related to the idealized instantaneous flow. Instead, we added four new experiments focusing on the effect of the added subgrid-scale TKE and included analysis and discussion on turbulence intensity. For the realistic case, we retained one figure for illustrative purposes but revised the text to be more concise.

> 5. Choice of Comparative Model. The paper compares results with PALM, a numerical model. Authors should discuss the rationale for this choice and why they did not consider comparing results to experimental data or field measurements to enhance scientic validity.

Thank you for your suggestion. In addition to incorporating the discussion related to the reasons for using PALM (Lines 79-82), we introduced a new section (Section 4.2) comparing WRF-SADLES with observations, including cup anemometer and LiDAR data. We believe that these additions enhance the scientific validity of our manuscript substantially.

> 6. Text Repetition. Duplicate copy-pasted portions in the manuscript (lines 59-65/66-72 and 256-259/260-264) should be addressed to ensure the clarity and ow of the manuscript. Figure Captions. The clarity of figure captions should be improved to enhance reader understanding. Clear, concise, and informative captions are essential for effective communication.

Thank you very much! We have extensively revised the text, figures, and captions to create a more concise and clear manuscript.

Responses to reviewer #4

> The authors introduce a Simple Actuator Disc Low-order wind turbine model (SADLES) for Large Eddy Simulation and implement its parameterization in the WRF model. This primarily addresses the downscaling simulation issue of wind farms within weather systems, striking a balance between the required accuracy for wind farm simulations and computational performance. Through validation in idealized scenarios and comparison cases, as well as application in the real world Alpha Ventus offshore wind farm in Germany, the authors demonstrate the model's advantages in downscaling.

Thank you for your review! We have provided detailed responses to your points below. Additionally, we have thoroughly revised the manuscript, incorporating four new idealized experiments and one realistic case study with a comparison to observational data. Furthermore, we have conducted new analyses and added new plots. We believe these changes have significantly improved the manuscript.

> 1. There are two highly similar content sections in the manuscript that need careful inspection. One from lines 59 65/66 72, and another from lines 256 259/260 264..

Thank you for highlighting the typos identified during our internal revision. We have carefully reviewed the paper and addressed these issues.

> 2. The authors assume the use of the inferred evaluation method when the direct evaluation result exceeds 0.5, but this assumption lacks specific clarification. It's better if the authors consider including an analysis explaining why the direct evaluation method calculates the axial inductin factor 'a' greater than 0.5.

In the 1-D momentum method, the axial induction factor 'a' cannot exceed 0.5, as it suggests wind in the wake blowing against the mean wind. However, the model using Option can lead to this scenario, potentially causing the model to crash. We observed this issue during some of our test experiments. To address this, we have revised the relevant paragraph (Lines 134-136) to clarify: "By applying this formula, 'a' can exceed 0.5, indicating wind behind the turbine opposing the ambient wind. This is nonphysical and may result in model instability."

> 3. From the perspective of the paper, there doesn't seem to be any difference between SADLES and WRF-SADLES. The authors should decide to use one term to refer to the model consistently.

Thank you for your suggestion. We have revised the paper to clarify the issues. It's important to note that SADLES is the name of the module implemented in WRF, while WRF-SADLES refers to the entire package, which also includes the cell perturbation model. Throughout the paper, we have replaced most instances of 'SADLES' with 'WRF-SADLES', except in a few places where it is necessary to distinguish between the two terms

> 4. In Figure 8a, there are two red stars, one indicating FINO1, and the other is undefined.

We have replotted the figure and addressed the issue.

> 5. The authors should provide a more detailed explanation of the code implementation section.

Thank you for your suggestion, we added some more detailed about the code implmentation (Lines 155-167).

> 6. In Figure 9 and Figure 10, the second subplot should be labeled as (b).

We combined the two figures into one (which is now Figure 13) and addressed the issue.

> 7. Appendix A: "Additonal WRF namelist options," the word "Additonal" should be adjusted to "Additional"

Thank you a lot, we fixed the typo.