2nd Review: Response to Reviewers - Data assimilation of generic boundary-layer flows for wind-turbine applications - An LES study

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1 General Response

The authors thank the reviewer for their time and their insightful comments. After many discussions in our group and thoughtfully considerations of the arguments put forward in the review, we have attempted to amend the manuscript accordingly. However, the main content with the numerical setup and simulations has not been changed: In our opinion, the present content is an appropriate and profound investigation of the relaxation methods in LESs with respect to the application of the methods for wind energy research.

Although there are clear limitations and restrictions of our numerical setup (e.g. no Coriolis force, small domains, small time scales), we consider the application of the methods for a single wind turbine (or two wind turbines in a row) to be entirely appropriate in the context of the present study. Of course, the limitations must be clearly described, this has now been done in the revised manuscript. We are very grateful to the reviewer for his advice and suggestions in this regard.

In addition, we are of course aware of existing alternatives to our setup and of methods that can be used for data assimilation in LESs (e.g. nudging of the potential temperature (Allaerts et al., 2020) or nudging of the TKE (Spille-Kohoff and Kaltenbach, 2001)). Although such methods are not considered or discussed in our study, the results presented are nevertheless a valuable contribution by its own to the successful implementation and use of data assimilation methods for the generation of inflow fields for wind turbines.

We therefore do not consider a completely new structure of our publication (as requested in comment 3) to be appropriate. The legitimate thoughts, considerations and suggestions of the reviewer would mean that our work, including the numerical simulations, would have to start from scratch.

Unfortunately, this is not feasible in terms of the time for which funding is available for our work.

We appreciate the interest of the reviewer in the presented topic and try to answer in the following the comments and to completely clarify the raised concerns. The comments have led to several changes in the manuscript and we believe that the quality of the work has been improved significantly.

First, the major changes in the revised version of the manuscript are listed below. Second, we respond individually to the comments.

Major changes:

- The introduction and the conclusion are fundamentally altered in order to present a clear image of the purpose and the capability of the developed setup including a 'nudging zone' and the implemented vibration method.
- The mathematical description has been altered in order to present the methods and the numerical setups clearly and to distinguish the applied methods from others in the literature.
- Fig. 6 has been modified and shows now the spatial evolution of the flow downstream of the nudging zone.

In the following we respond in detail to each comment/question:

2 Response to Comment 1

Comment 1:

There are two fundamental and conceptual differences between the methods used by both the authors and Nakayama and Takemi (2020) versus the assimilation techniques developed by Allaerts et al. (2020) or used in other LES codes (e.g. Maronga et al., 2015 or Stipa et al., 2024). Assuming a stationary target flow, like the one used in the paper, the assimilated flow obtained by both the authors and Nakayama and Takemi (2020) is not in equilibrium, while in Allaerts et al. (2020) and others it is. Secondly, the authors use a nudging region at the domain inlet, while in Allaerts et al. (2020) and others the internal forcing is applied in the entire domain, during the precursor phase (the source terms would be then saved and re-applied in a hypothetical successor simulation with wind turbines). Given the large interest that profile assimilation has obtained recently in LES, I think it is important to make some clarity between the implications of different assimilation methods, and this should be explicitly mentioned in the paper. Right now it seems that the method referred with <> is that of Allaerts et al. (2020), when in reality this is not true. Specifically, the momentum equation above the boundary layer (BL) in a conventional LES setup boils down to

$$\frac{\partial u}{\partial t} + f_c(V_G - v) = 0 \tag{1}$$

$$\frac{\partial v}{\partial t} - f_c(U_G - u) = 0 \tag{2}$$

Eq.1 is in essence an undamped linear oscillator (this is mathematically shown for example in Stipa et al., 2024), but the important thing here is that the driving pressure gradient is represented by the terms $f_c V_G$ and $f_c U_G$. These terms are usually modified to control the velocity in the computational domain, for example to maintain a fixed geostrophic wind (Allaerts and Meyers, 2015) or a constant wind at the hub height (e.g as done in Stipa et al., 2024 or in the SOWFA and AMR-Wind codes). In their paper, Allaerts et al. (2020) substituted these terms with more generic terms (F_{u_i} and F_θ) that are derived from assimilation techniques, so that Eq. 1 becomes

$$\frac{\partial u}{\partial t} - f_c v + F_{u_x}(z,t) = 0 \tag{3}$$

$$\frac{\partial v}{\partial t} + f_c u + F_{u_y}(z,t) = 0 \tag{4}$$

while F_θ is added to the potential temperature equation in order to nudge its solution towards the mesoscale observations (if applicable). In summary, the method developed by Allaerts et al. (2020) changes the equilibrium condition of the ABL flow towards a new condition, which is provided from mesoscale simulations or observations. Conversely, in all methods described by the authors, the flow equilibrium condition only changes inside the nudging region, where the equations are essentially

$$\frac{\partial u}{\partial t} + f_c(V_G - v) + F_{u_x}(z,t) = 0 \tag{5}$$

$$\frac{\partial v}{\partial t} - f_c(U_G - u) + F_{u_y}(z,t) = 0 \tag{6}$$

Then, once the flow exits the nudging region, it is again governed by Eq. 1. Hence, if the domain is extended for several kilometers (at least one order of magnitude larger than the size used in the paper), I would expect that the solution returns to the original non-assimilated one that was observed before the nudging region. I think this fundamental difference between let's call them "region nudging methods" and "domain nudging methods" should be explicitly mentioned in the paper. For instance, the problem associated with the flow evolving when the Coriolis force was activated — referring to the previous version of the manuscript — was most likely due to the flow trying to reach equilibrium again once it exited the nudging region. This fundamental difference is important because it essentially reduces the applicability of the method to small domains without Coriolis force. For example, I would not use "region nudging methods" to study wind farm wake evolution, as the balance between the driving pressure gradient and Coriolis force is of crucial importance for these applications (see for example Bastankhah et al., 2024). Conversely, I think "region nudging methods" are appropriate for the single turbine study presented in the paper as the domain size is only 5-6 km. However, the inflow contains Coriolis force in selected case, and the authors should include in Fig. 6 the velocity, TKE and spectra close to the domain exit (the domain length is 5.12 km) instead of only showing what happens 200 m past the nudging zone.

Response:

The Reviewer is correct that the methods presented in the manuscript and the techniques developed by Allaerts et al. (2020) or Maronga et al. (2015) and Stipa et al. (2024) are fundamentally different. In this regard, the mathematical description has been altered in order to explain the methods clearly. We have also altered the introduction to describe the different methods in more detail, offering a clearer reasoning for the methodology applied in the current work. We explain also the desire to use a nudging zone rather than a nudging domain.

There may be some confusion over the state of equilibrium in our simulations. In general, the initial state is in hydrostatic balance. This initial state ($\boldsymbol{v_e} = \boldsymbol{v_g}$, Θ_e, p_e) gets subtracted in Equation 2, resulting in the corresponding term for the pressure perturbation p', Θ' contributing to buoyancy and the velocity deficit, $\boldsymbol{v} - \boldsymbol{v_e}$, Coriolis force is acting on. In order to justify our numerical setup, we detail here the forces acting on the simulated flow fields. Firstly, in the precursor simulation representing the neutral boundary layer, performed with periodic boundary conditions, there is an application of a constant pressure gradient in the horizontal direction that leads to a fully developed turbulent flow. The precursor is run for a relatively long time (about 40 hours) in the presence of this large-scale forcing, with no nudging zones included.

The numerical setup changes for the assimilation simulations, where there is no such pressure gradient but synchronized yz-slices copied from the precursor are used as inflow at the inflow boundary. These simulations are run for only 20 minutes. For these simulations, which are the focus of the paper, in the absence of both a Coriolis force and a pressure gradient force, the equivalent momentum equations are

$$\frac{\partial u}{\partial t} + ... = 0 \tag{7}$$
$$\frac{\partial v}{\partial t} + ... = 0 \tag{8}$$

in the regions where there this is no nudging, and

$$\frac{\partial u}{\partial t} + F_{u_x}(z,t) + ... = 0 \tag{9}$$
$$\frac{\partial v}{\partial t} + F_{u_y}(z,t) + ... = 0 \tag{10}$$

in the nudging region ('...' refers to the neglected terms - e.g. advection, diffusion, pressure gradient - in this short representation of the equations). Once the flow exits the nudging region, it is not in disequilibrium as there is no deviation from a prescribed pressure gradient or there is no Coriolis force acting on the flow.

For more realistic simulation setups and large domain sizes (e.g. large wind farms), we agree with the statement that "the balance between the driving pressure gradient and Coriolis force is of crucial importance for these applications". However, in the absence of both forcings in combination with open streamwise boundary conditions, the flow does not evolve beyond the nudging zones in our assimilation simulations, as there is no forcing which would cause the profile to adapt to the inflow profile within the few km we consider (which is apparent from the vertical profiles shown at various points downstream for the NBL cases in the current version of the paper). In general, we agree with the reviewer, that there is a deceleration of the flow but only because of internal friction, which impacts the vertical profiles downstream. However, the impact within the region considered (the initial 2 km region downstream) is very limited (see Figure 4).

When the Coriolis term is included in our setup, as was the case in the initial submission, some undesired behavior occurred in the assimilation simulations which leads to the flow evolving beyond the nudging zone. In this case, the equations are essentially

$$\frac{\partial u}{\partial t} + f_c(V_e - v) = 0 \tag{11}$$
$$\frac{\partial v}{\partial t} - f_c(U_e - u) = 0 \tag{12}$$

where V_e and U_e are the environment, or background flow. Note that the Coriolis term acts on the perturbations to the environmental flow, and we once again do not employ a large-scale pressure gradient in these simulations. The terms $U_e - u$ and

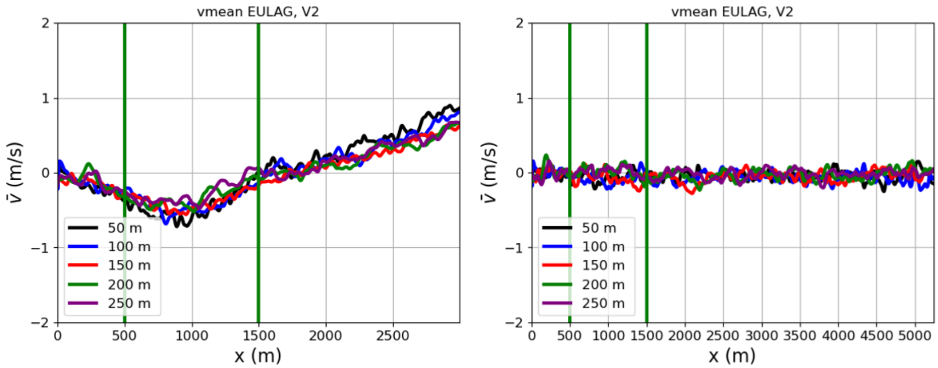

Figure 1. Evolution of the v wind component in the x direction at various heights (see labels) for two simulations in the initial submission, which included the Coriolis force (left panel) and which did not (right panel). The simulations in this figure where computed in both cases with the vibration method with $f_0 = 0.002\ \mathrm{s}^{-1}$.

$V_e - v$ are in essence zero (in a mean sense) before the nudging zone, as the flow undergoes no forcing before nudging. The issue arises when the mean flow v or u is modified following the application of nudging, which can give rise to unbalanced "perturbations" that are in some cases as large as the mean flow itself. For example, if the mean environmental flow U_e is 10 m s^{-1}, and the target 5 m s^{-1}, then beyond the nudging zone, Coriolis forces would be acting on "perturbations" ($U_e - u$) of 5 m s^{-1}. This leads to a continuous evolution of v downstream of the nudging zone, as shown for one of the initial simulations including Coriolis forcing (Figure 1: left panel). When the Coriolis force is omitted, there is no evolution of the mean flow downstream (right panel).

To this end, and given that our assimilation simulations do not include a large-scale pressure gradient that we can "assimilate" the potential temperature field to, there will always be an imbalance downstream of the nudging zone when Coriolis forces are acting on wind "perturbations". It is true that this setup limits the presented method to small domains and simulation timescales, however, the implementation for a one wind turbine wind park with scales no larger than 5-6 km is, in our view, perfectly reasonable.

The application of a nudging method in the whole domain, instead of a nudging zone, may solve the issue in theory, as then any imbalance of the flow that we encounter would be continuously nudged back towards the target profile. However, in this setup, the flow is never reaching a steady state, requiring nudging to be always active. This setup is unsuitable for our wind turbine simulations, where there would be a nudging of the turbine wake.

To our understanding, our numerical setup (open streamwise boundary conditions) in combination with a nudging zone and no large-scale pressure gradient or active Coriolis force in the assimilation and wind-turbine simulations is appropriate for our scope of interest. The differences in comparison to e.g. Allaerts et al. (2020) or Stipa et al. (2024) are one consequence of our setup.

Another difference is the simulation time, which further justifies the omission of the Coriolis force in the assimilation simulations. We consider in Sec. 6 approximately 1 h of wake simulation time (evaluation of the wake is done for a 20 min average at the end of the simulation) and consequently 1 h of assimilation time is required. Which is much smaller compared to an inertial oscillation period of

$$T = \frac{2\pi}{f} = \frac{2\pi}{1.0 \times 10^{-4}\ s^{-1}} = 17.45\ h. \tag{13}$$

Based on this, we can conclude that the Coriolis force is not significant on such short time scales.

In response to the request of the reviewer to show flow features further downstream we now show profiles beyond 200 m past the nudging zone. In figures 3, 4 we show vertical profiles at 2, 3 and 4 km distance. Regarding Fig. 6 (shown hereafter Fig. 2) in the manuscript the spatial evolution of the flow is now better presented and the positions 3 and 4 km downstream of the nudging zone are included. The authors thank the reviewer for the recommendation. Since the area of interest in this study is the assimilation and evolution of the flow as far as max 10 D (cf. 4.4 D distance between the wind turbines in Krummendeich) downstream of the nudging zone, and the flow does not evolve in this region of interest, we see no further need to investigate the flow fields beyond 4 km distance. As the wake elongation correlates with the turbulent kinetic energy of the inflow, we can assume a wake elongation of up to 22 D like in Fig. 8 approaching the maximum, considering the relatively low TKE in Fig. 6. In neutral and convective cases, the wake is assumed to recover more rapidly.

3 Response to Comment 2

Comment 2:

Another aspect that is not even mentioned by the authors is inertial oscillations above the boundary layer. In the simulations presented by the authors the domain is always fully turbulent, but it would be interesting to see how the method behaves for e.g. a conventionally neutral boundary layer, where the ABL is capped by the stable capping inversion layer instead of developing up to the upper boundary. Inertial oscillations occur because Eq. 1, valid above the BL, has no damping terms (while turbulence acts as damping term inside the BL) and so, if at any time during the simulation the wind is different from the geostrophic values above the capping inversion, these oscillations will be initiated. When controlling using Newtonian methods that provide the entire profile of observed wind, the controller acts as a damping term in the geostrophic layer. However, when trying to attain a given — both constant or variable — velocity at a location inside the boundary layer, these oscillations imply that the geostrophic wind can never reach steady state. It would be very interesting to see how an integral controller performs in this sense and if there are additional constraints on the choice of the frequency w.r.t. the frequency of inertial waves, whose period is $2\pi/f_c$. I believe that these considerations should be made since the authors in the end use a precursor (P3) which contains Coriolis force and — potentially — a geostrophic layer. Something that would be really interesting to me would be showing, for a given height (maybe spanwise averaged) close to the domain top, the time history of velocity magnitude before the nudging (i.e. that of the precursor P3), the target time history (which would be a constant) and the time history at two streamwise locations past the nudging region (ideally 200 m and 5 km). Could the authors provide this plot in the revised version of the paper, or at least in the next reply? Moreover, I think that the transition from cases P1 and P2, where there is no Coriolis and no potential temperature stratification, to a case that comes from a diurnal cycle simulation (Englberger and Dörnbrack, 2018)

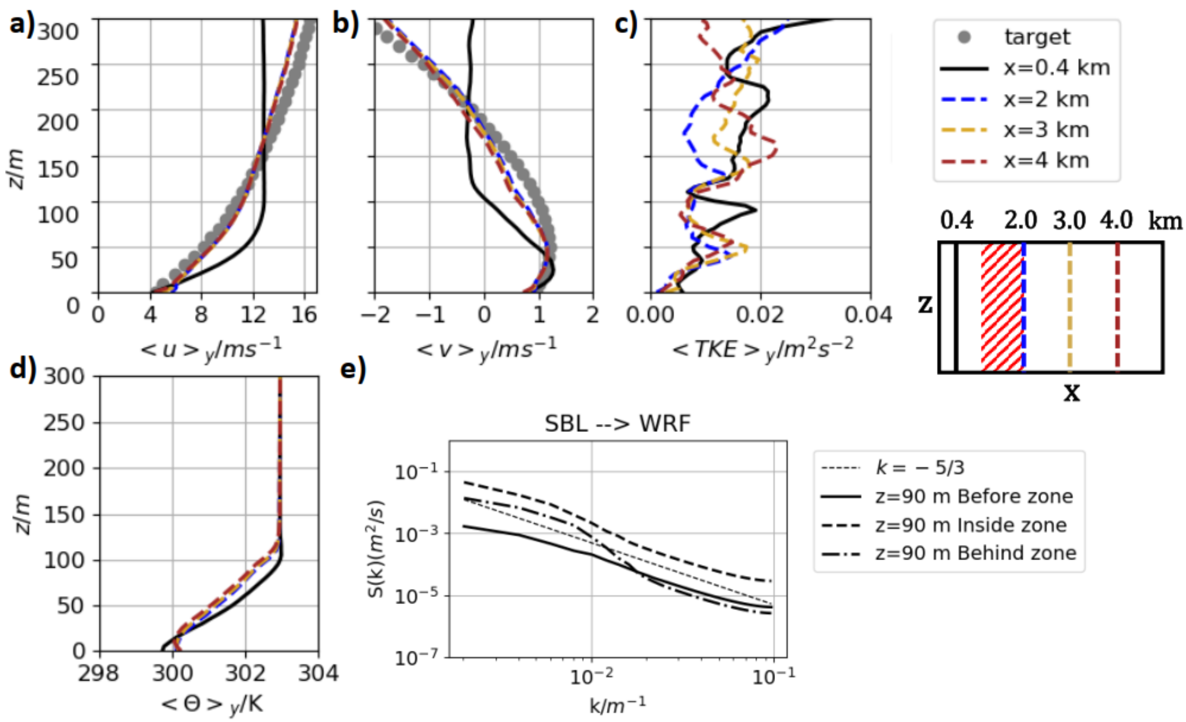

Figure 2. Results for SO of the assimilation towards the representative WRF velocity profile with the precursor simulation P3 and application of the vibration method ($f_0 = 0.002$ s^{-1}). a) zonal velocity $< u >_y$ b) meridional velocity $< v >_y$ c) $< TKE >_y$ d) potential temperature $< \Theta >_y$. The black solid lines refer to the values upstream of the nudging zone. The gray dotted lines in a) and b) indicate the zonal and meridional target velocity profile. The colored lines show the values for the downstream positions according to the scheme on the right. In e) the horizontal energy spectra is shown for the height $z = 90$ m (length 500 m, width 3000 m) for the flow before, inside and behind the nudging zone.

is a good showcase of the proposed method's applicability, but it does not address the above questions entirely. They should be investigated in additional cases (see my next comment) or at least mentioned as future studies or discussion points.

Response:

The main objective of our work is to generate inflow fields for wind turbines in order to simulate the wake behind the wind turbine rotor. For this reason, the atmosphere above the boundary layer is for this work not in the scope of our interest. The spanwise averaged time-history plot from the precursor simulation P3 is shown in Fig. 3. Indeed, small scale disturbances can be seen for the vertical velocity component w. However, they are of small magnitude (<0.3 m s^{-1}). These presumably do not affect the numerical setup using data assimilation and finally a wind turbine in Sec. 6. Additionally, Fig. 4 shows the mean vertical profiles for the three velocity components in the stably-stratified atmosphere from the diurnal cycle simulation from Englberger and Dörnbrack (2018). The vertical component w does almost vanish. In conclusion, it is very unlikely that the synchronized yz-slices are affected by inertial oscillations.

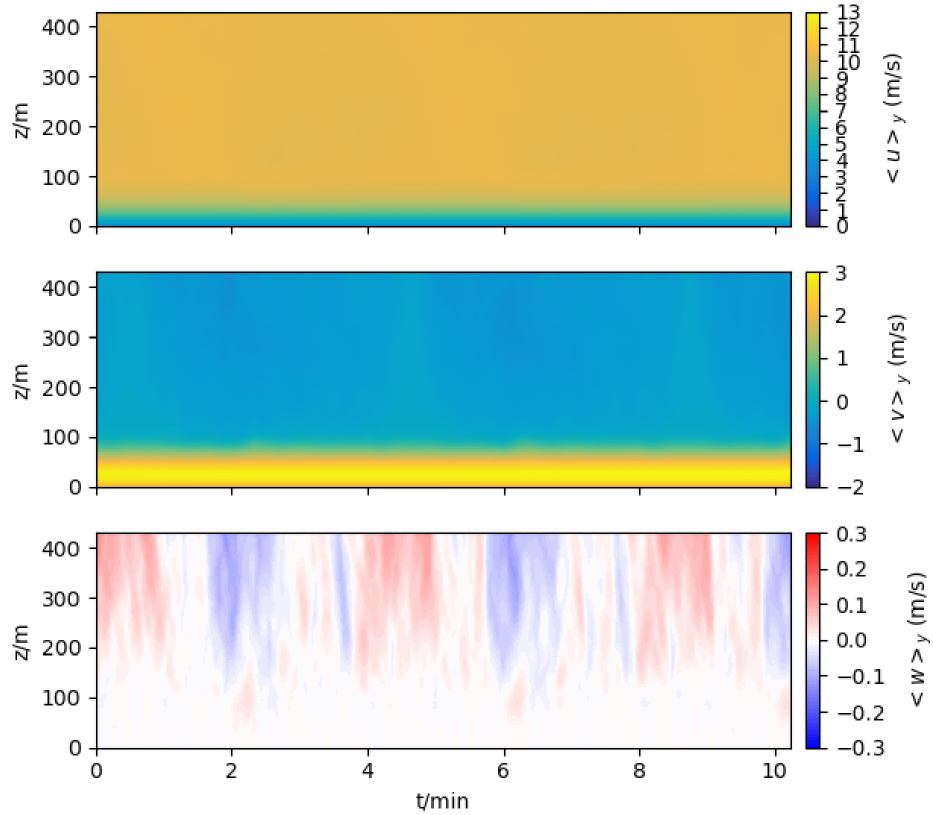

Figure 3. Hovmoeller diagram for the zonal, meridional and vertical velocity component averaged over the y-axis calculated from the synchronized yz-slices from P3 over 10 min.

The proposal of the reviewer to consider the impact of the assimilation methods above the boundary layer is a valid idea which has not been investigated during the current study. As far as we are interested in the inflow field for a wind turbine we try to generate a flow with mean velocities which are as close as possible to target velocity profiles in the area of the wind turbine which has in our simulations a top tip of 153 m. This is the reason why most of the plots shows the quantities up to a height of $z = 300$ m (cf. Fig. 3, 4, 6). The region above the boundary layer has not been included in the current study. The authors thank the Reviewer for this recommendation and will consider it in further investigations related to the topic of data assimilation using a nudging region. When referring to inertial waves in the domain, it may be highlighted that the simulation time of the simulations with data assimilation is less than 1 h. This is a very short time and limits dramatically the possibility of the development of gravity waves as the inertial oscillation period is much higher (cf. Response to comment 1 Eq.13). This short simulation time reduces the probability for significant effects of the interaction between the boundary layer and the free atmosphere above. Considering the precursor simulation P3 no inertial oscillations have been observed which could have an impact on the presented methods (cf. Fig. 3).

Considering the previous arguments and clarifications we are convinced that the test case in Sec. 5 is a good example to test

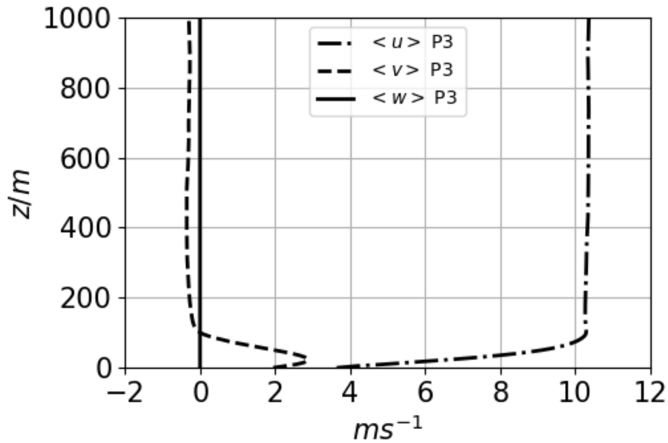

Figure 4. Mean vertical profiles of the stably-stratified atmosphere of the whole diurnal cycle simulation for the zonal $< u >$, meridional $< v >$ and vertical $< w >$ velocity component.

the applicability of the vibration method. Of course, it is a very specific example for a stably-stratified atmosphere. However, the presented test case demonstrates that with the proposed setup it is possible to nudge the zonal and meridional velocity in the nudging zone. The assimilated flow field propagates further downstream and in the end a wind turbine can be included directly in the presented setup. Furthermore, the case in the SBL demonstrates the necessity of a good approximation of the measured wind profiles by the precursor simulation. The precursor simulation P3 is characterized by both wind shear and veer as there is a target profile for both the zonal and the meridional velocity showing as well shear and veer. As already mentioned, the domain size is very small and the simulation time in the simulation SO is very short. Therefore, the impact of the Coriolis-force term may be neglected in SO as no impact is expected. However, an active Coriolis-force term would alter the solution in a non-physically way for the regarded scales of time and space. This has been the major issue in the initial version of the manuscript and led to the significant changes presented in the first revised version of the manuscript.

4 Response to Comment 3

Comment 3:

In general, the impression that I am left with after reading the paper is that I don't really know how these "region nudging methods" would behave in cases that are different from those presented by the authors. The authors did not investigate the applicability of the method in what is the current state of the art of wind farm LES (conventionally neutral boundary layers with Coriolis force and potential temperature), but instead essentially conducted channel flow simulations, then applied the method to a very specific case, which doesn't add anything more to the paper but rather contradicts it. Looking at the previous version of the paper, after NBL simulations have been conducted again without the Coriolis force, one could conclude that this method is not applicable when the Coriolis force is active, but then the method is applied to case P3, where the Coriolis force is active (at least in the precursor), and only the flow profile 200 m after the nudging region is shown. A better way, in my opinion, to structure the paper in order to give a more comprehensive overview of the methods would be to provide results of

– NBL with no Coriolis and with no stratification

– NBL with Coriolis and with no stratification

– CNBL with no Coriolis and with stratification

– CNBL with Coriolis and stratification

In the nudged simulations, the domain length should be set such that flow deviation from the target conditions can be observed in order to establish after how much this happens and give clear guidelines on how to set up these methods. Only then it makes sense to switch to more complicated setups such as an SBL originating from a diurnal cycle, where the flow — and especially turbulence — depends on the history of the ABL. This is in essence a case that is very difficult to reproduce, hence of little use — as is — for future users of the method who will be interested in verifying their implementation against results presented in this paper.

Response:

The reason why the Coriolis-force term is excluded in the simulations with data assimilation has been explained in the responses to Comment 1 and Comment 2. This choice does indeed limit the applicability of the method. The active Coriolis-force term in the precursor P3 is required to drive (in addition with a negative heat flux) a veering flow associated with an SBL.

We are thankful for the recommendation of a different structure of the manuscript. We hope that our arguments and clarifications in the previous responses to comment 1 and 2 as well as the modifications in the manuscript present now a clear image of the purpose of our setup.

As there is no forcing of the solution downstream of the nudging zone towards the non-assimilated inflow velocity a significant deviation of the assimilated velocity profile is not observed downstream of the nudging zone. The effect of the Coriolis-force, leading to the mentioned deviation, can be seen in the initial version of the manuscript. However, as the reviewers in the first review round emphasized the inconsistency in this simulation setup and in the results, we altered the setup in order to show how the methods can be used without this numerical issue. The domain of interest for the analysis of the assimilated flow is also far (>1 km) from the outflow boundary. In addition with the implemented absorbers (cf. Eq. 2 in the manuscript) numerical artifacts coming from the boundaries can be excluded.

Moreover, the SBL cases presented by the authors are lacking several points of discussion, very important in my opinion, among which

– Potential temperature is evolving in the SBL case, is that solved in the nudged runs? If yes, why it is not nudged?
  Response: While it is true that the potential temperature profile evolves in the precursor SBL case, which is a simulation of a diurnal cycle, the assimilation simulations are run for a period of < 1 h in which there is no applied heating or cooling at the surface. We consider this time to be too short to see a significant influence of additional surface cooling

for a heat flux of -10 W m^{-2}. Fig. 2 includes a plot of the evolution of the potential temperature (cf. Fig. 2d).

The horizontal assimilation of the zonal and meridional wind components in the nudging zone induces convergence or divergence of the upstream flow, depending on whether the nudging to the target profile leads to an acceleration or deceleration of the flow. This, in turn, leads to vertical motion (both within and slightly upstream of the nudging region) as required by mass continuity. In this specific case, the nudging induces downward motion close the surface and upward motion aloft 15 m, associated with the deceleration and acceleration of the zonal (cf. Fig. 2a) and meridional (cf. Fig. 2b) wind components, respectively. Given the stratification of potential temperature in the stable boundary layer (SBL), this vertical motion results in a contribution from the vertical advection term in the potential temperature equation (Eqs. 3 in the manuscript), manifesting as local warming below 15 m and cooling between approximately 20 m and 110 m.

The evolution of potential temperature in this simulation here is confined to the aforementioned effects in the nudging zone. After leaving this zone, there is no further evolution downstream (cf. Fig 2d). This issue does not manifest in the NBL simulations, as there is no stratification of potential temperature, thus there is no contribution from the potential temperature advection term.

As to why we didn't nudge potential temperature here, it was assumed that, as balancing issues were not a concern, that the temperature evolution would not play a major role on the velocity fields downstream of the nudging zone. To test this assumption, we have conducted a numerical experiment where we repeat this SO simulation and set gravity to zero. This would have the consequence of removing the contributions of buoyancy from the momentum equation, and thus, removing any ill-intended consequences of localized temperature evolution on the velocity fields. Fig. 5 shows the evolution of the potential temperature and the zonal and meridional velocity for the simulation SO without gravity. The assimilated velocities downstream of the nudging zone (Fig. 5b and c) are in principal identical to the velocities in the simulation with gravity (Fig. 2b and c). Furthermore, a local warming below 15 m and a local cooling between approximately 20 m and 110 m can be seen in Fig. 5a similar to Fig. 2d. It can be concluded that the deviation of the potential temperature field has only a very small impact on the assimilated zonal and meridional velocity component in the simulation SO with gravity. An assimilation of the potential temperature field could be included but presumably would not have a great impact in the presented test case applying the vibration method according to our setup in an SBL. Despite this result showing minimum impact, we would recommend that future studies implement a potential temperature nudging for profiles with stratification.

- Potential temperature influences turbulence (it suppresses vertical motions when stable). Hence, if temperature was solved in the non assimilated case and not in the assimilated case, I am not surprised that wake recovery is faster in the latter. Perhaps this is also due to the fact that the adopted assimilation method increases turbulence intensity.

Response: Temperature is solved both in the simulations with and without data assimilation (Fig. 8 in the manuscript). The differences in the wake can be traced back to the different inflow field and the resulting lower rotational speed of the wind turbine.

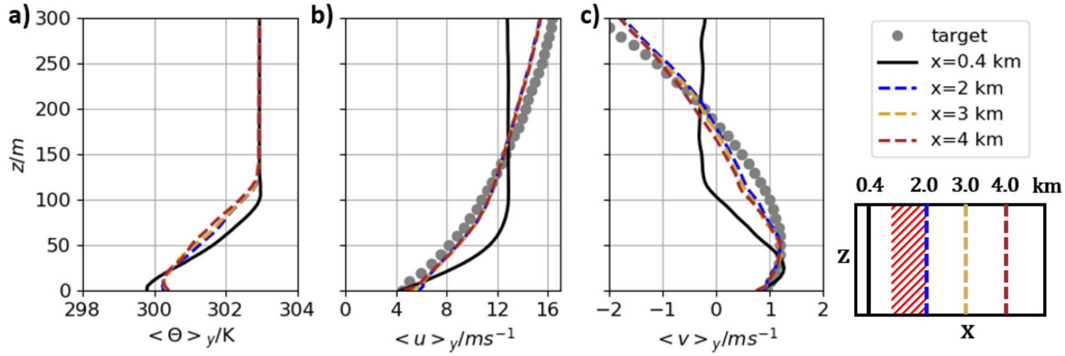

Figure 5. Results for SO without gravity ($g = 0$ in Eq. 2 in the manuscript) of the assimilation towards the representative WRF velocity profile with the precursor simulation P3 and application of the vibration method ($f_0 = 0.002$ s^{-1}). a) potential temperature $<\Theta>_y$ b) zonal velocity $<u>_y$ c) meridional velocity $<v>_y$. The black solid lines refer to the values upstream of the nudging zone. The gray dotted lines in a) and b) indicate the zonal and meridional target velocity profile. The colored lines show the values for the downstream positions according to the scheme on the right.

– Velocity profile, turbulent fluctuations and potential temperature are all interlinked. The methods discussed adjust the velocity profile to a completely different velocity profile, but turbulence is left unchanged (that is the objective, but sometimes it is even increased. In principle, it is possible to nudge the profile of a convective ABL to that of a stable ABL, but this results in a SBL with the turbulence of a CBL, which does not make much sense in my opinion if the objective is to increase the realism of the simulation. In fact, the only thing that has been added is time variability of the mean, but the realism might be even lower. This should be discussed, i.e. the authors should mention that turbulence from the precursor should be consistent with that observed in the target profile, and methods to verify this should be discussed.

Response: Thank you for this comment. In fact, the precursor simulation has to be set up in order to approximate as good as possible the principal characteristics (vertical gradient of horizontal velocity, TKE, stratification) of the atmospheric situation measured by on site instruments (from the investigated location) which represents the atmospheric inflow situation we want to generate in our LESs. Measurements techniques for the wind-farm site WiValdi are presented by Wildmann et al. (2020, 2022). This data set shall be used in future in order to distinguish characteristic atmospheric situations (wind speed, TKE, shear, veer) for the wind-farm site WiValdi. The general idea is to produce a limited amount of precursor simulations for each stratification, enabling us to distinguish between different categories of neutral, stable and convective situations e.g. by wind speed and TKE and for stratified situations also by lapse rate. In this work we only consider purely idealized neutral simulations and one stable category for the wind-farm site WiValdi, as we present the first step towards the generation of site-specific inflow fields for WiValdi. We rewrote the corresponding part: "In order to account for the broad range of possible atmospheric situations, multiple precursor simulations are required. The proposed method here is only meaningful if the key values of wind speed and TKE (and also lapse rate in case of stratified situations) are approximated by the precursor simulation."

– Connected to the previous point there is the validation of the method. Without it, how can it be stated that the assimilated flow (not the sole velocity profile) makes sense?

  Response: This is the aim of future studies, where the inflow field of the wind turbine is assimilated and the interaction of the wake with the boundary layer is computed. Measurements of the wake coming from a Doppler wind lidar mounted on top of the nacelle of the wind turbine are then used for the comparison of the computed wake (with the assimilated inflow) with the wake measurements. Hence, the performance of the assimilation method can be investigated when it comes to the simulation of wind-turbine wakes. The described case studies in this work show however, how the method can be applied in principal. We demonstrate an assimilation technique which is capable of adapting velocities in an neutral idealized case (Sec. 3 & 4) and tested the method for a stably-stratified atmospheric situation at Krummendeich. We describe in the text that "there is no measurement data available for wakes behind the wind turbines at the wind-farm site WiValdi for the considered situation. Therefore, a validation of the wake against observations could not be done at this point. However, the results of this section present the first step towards an efficient method to generate inflow fields for a single wind turbine in a near-stably stratified ABL." As soon as there are measurements of the wake after the wind turbine, we are able to validate the wake simulations. However, we demonstrated the functionality of the assimilation method and our setup with a nudging region in this manuscript.

To be eligible for publication, the manuscript should be extensively enhanced in my opinion. I think both cases with and without Coriolis force should be included in the paper, as their difference points out to the heart of the discussion regarding data assimilation. This would maybe help the authors to address my first comment. If the authors would like to retain Sections 5 and 6, the aspects outlined in Comments 2 and 3 should be addressed. If these sections are removed, then the authors should explicitly mention that the region nudging method — as presented — is applicable to neutral channel flows with no Coriolis force. This does not mean that the presented method cannot be applied to wind energy problems, but rather that these should be highly simplified, and this should clearly come out when reading the manuscript.

5 Specific Comments

line 7-8: a 5 m resolution is not necessary in wind energy applications unless a stable stratification is simulated. There are many studies that use a higher resolution than that (see for example Cheung et al., 2023; Lanzilao and Meyers, 2023; Maas, 2023; Wu and Porté-Agel, 2015).

Response: This is correct and maybe the formulation in the manuscript is misleading. However, the smallest eddies impacting the performance of the wind turbine are in the order of meters (cf. Kosović et al. (2025)). And also the resolution of turbulence in the wake increases when the grid size is increased. This is why the method is tested in regards to the grid sensitivity. The formulation is altered in the 2nd revised version:"The method based on the vibration equation is suited for fine grids (dx = dy = dz = 5 m), which is a common grid resolution for wind-energy studies in LESs."

line 12-13: only the local Newtonian method damps turbulence. The problem of assimilation methods commonly adopted in wind energy is that they usually increase turbulence (see for example Allaerts et al., 2020, 2023)

Response: Correct, the formulation is changed into a more properly description:"The assimilation method based on the vibration equation is able to adapt the zonal and meridional velocity components of an atmospheric flow while negative effects on the atmospheric turbulence could be reduced."

line 24: I would say mainly determined instead of mainly controlled.

Response: We agree, we use "mainly determined" in the manuscript.

line 67-78: what is described here: "In general, those methods apply a damped harmonic oscillator as an additional forcing in the governing equations of motion. Commonly, this forcing term can consist of a damping (proportional) and an oscillating (integral) part (e.g., Spille-Kohoff and Kaltenbach, 2001). In the case of Newtonian relaxation, only the damping part is considered. Here, the numerically calculated profiles of wind, temperature, humidity etc. are adjusted to given target profiles (which can either come from measurements or are extracted from the output of mesoscale model simulations) using a specific relaxation time scale, which is a free parameter of this method." is exactly what Allaerts et al. (2020) do, so the follow-up sentence "To circumvent this limitation", referred to their method, is not clear to me. Moreover, Newtonian relaxation increases turbulence, rather then reducing it. This whole paragraph should be made more clear. Perhaps, the authors should focus on the word "additional forcing" at line 67 in relation to my discussion in Comment 1 of Sec. 1.

Response: Thank you for the advice, the paragraph is changed and the methods are now consequently distinguished: " The investigations by Allaerts et al. (2020) and Allaerts et al. (2023) present an 'indirect profile assimilation' method. It is described as an internal forcing technique deduced from mesoscale variables (wind speed and temperature), including the time and height history of these variable in the LES. Their grid assimilation method acts in the precursor simulation at every grid point in the numerical domain and achieves an equilibrium state with the desired atmospheric mean characteristics and turbulent statistics. They tested the approach with the damping part, and a combination of both damping and oscillating, with quite similar results. Further, Stipa et al. (2024) developed another domain relaxation approach by applying the proportional and integral part, which is additionally able to prevent inertial oscillations, making it well suited for wind-park approaches"

line 116: more than preserving turbulence I would ask myself whether it is correct or not to arbitrarily change the wind profile while preserving the turbulence profile. By doing this, the flow is not in equilibrium anymore, i.e. the equations will tend to the original state as soon as the nudging stops.

Response: The detailed responses to comment 1 and 2 might answer the comment to this first research question. We deliberately added the second part of the sentence 'within the scope of the defined conditions' to highlight the limitation of the approach. Turbulence is not preserved by all means but within the presented setups and predefined conditions, turbulence should not be altered significantly. The objective is to generate an atmospheric inflow field with characteristic turbulence. The discussed assimilation methods shall nudge the horizontal velocity components while TKE is not damped or enhanced inadequately. Due to the concern of the reviewer to this first research question and the next specific comment, we altered the introduction and merged the research question into the text (cf. response to next specific comment).

line 116-127: I would try to merge these research questions into a coherent text. Otherwise the impression is that these are the only questions regarding data assimilation techniques, while in reality there is much more to it. I would explicitly state that these are the questions that the authors want to answer in this specific study (which are not fully answered in my opinion), but there is much more to be addressed and this only represent a part of it. Finally, Q3 seems a bit trivial to me, maybe not really a research question. The wake will change based on the provided inflow conditions, whatever they are, either assimilated or coming from a different precursor. There is not much to research on this. It seems to me that this is only here to justify Sec. 5 and 6 of the paper, but it is not clear what physical change the authors expect in the physics when using an assimilated inflow rather then a non-assimilated one.

Response: The authors follow the advice of the reviewer and have changed the introduction. Research questions are omitted in the revised version. We highlight the limited amount of research questions we are dealing with in this work, making clear that there are much more, some still open and some answered in other studies: "In general, data assimilation techniques applied in wind-energy research pose a lot of open questions (Allaerts et al., 2020, 2023; Stipa et al., 2024). With this study, we want to make a first step towards the generation of site-specific inflow fields for wind turbines using data assimilation where the additional forcing due to the assimilation is applied in a region of the computational domain upstream of the wind turbine." In the outline (Introduction) of the paper we now write: "A special focus is on the impact of the discussed assimilation methods in regard to the characteristic atmospheric turbulence. As our final aim is to simulate real atmospheric situations, which for example may include veering inflows, Sec. 5 exemplifies how the idealized approach can be modified towards the reproduction of a measured near stable atmospheric situation in the wind-farm WiValdi. Here, we focus on the parameter space of the vibration approach and the importance of a proper precursor simulation and use a combination of measurements and a WRF simulation for creating target wind profiles. Finally, we test the applicability of the vibration method in a wind-turbine simulation in Sec. 6 where the wind turbine is exposed to the generated inflow from Sec. 5. This is a first test case of the developed tool chain using a precursor simulation and the vibration method to generate a stably stratified atmospheric inflow situation for a wind turbine and the subsequent simulation of the wake behind the wind turbine."

Section 5 demonstrates the assimilation of the zonal and meridional velocities coming from a stably-stratified precursor simulation towards more complex target velocity profiles which were measured at the wind-farm site WiValdi and reproduced by a WRF simulation delivering continuous target profiles. This assimilated atmospheric flow field is used as inflow for the wind-turbine simulation in Sec. 6. This section shows that the proposed approach is capable to assimilate the velocity profiles upstream of the wind turbine while the general characteristics of the wake for a stably-stratified inflow can be preserved. Indeed, the reviewer is right in the point that the computed wake needs to be validated. However, to the time of the inflow measurements the nacelle lidar has not yet been mounted on top of the nacelle. Therefore no measurement data of the wake was available at this point. We tried to highlight the importance of Sec. 6 but also the missing validation measurements for the wake in the end of the section: "This section presented the interaction of the wake behind the rotor within a stable atmospheric boundary-layer flow. The results for both simulations show the main features of a wind-turbine wake in an SBL. The asymmetry and elliptic shape of the wake (cf. Fig.10) are as well described Abkar and Porté-Agel (2015) (Fig.17) and Englberger and Dörnbrack (2018) (Fig.6) who considered wind-turbine wakes in SBLs too. The deflection of the wake if wind veer is dominant in near-stably stratified ABL regimes is also described by Bhaganagar and Debnath (2014) (Fig.11) and Mirocha et al. (2015) (Fig.11). The applied vibration method adapts the wind profile to the target profile and changes wind veer and shear in the atmospheric flow accordingly. Due to these changes of the mean inflow conditions, the developed wake is different in its shape and also considering the velocity deficit compared to the reference case without assimilation. There is no measurement data available for wakes behind the wind turbines at the wind-farm site WiValdi for the considered situation. Therefore, a validation of the wake against observations could not be done at this point. However, the results of this section present the first step towards an efficient method to generate inflow fields for a single wind turbine in a near-stably stratified ABL."

line 141: the pressure perturbation p' is not solved for?

Response: We solve for pressure perturbation in the elliptic equation for pressure (see Prusa et al. (2008)[Eq. 14]). It is required to approach in divergence-free conditions for the velocity field.

equation 2: the Coriolis term is wrong. The x momentum contains the y velocity and vice versa. Check out this paper (Allaerts and Meyers, 2019) to see how the authors expressed the Coriolis term in vector form, it may help. Moreover, as it is right now, Ω is not the rotation rate vector of the Earth, but rather that of your reference system, which is referred to as half of the Coriolis parameter f , with f = 2Ωz = $2\omega\sin(\varphi)$, where ω is the Earth's rotation rate and φ is the latitude. Please be precise when writing down the governing equations.

Response: This is true and we apologize that the formulation in our manuscript led to irritations. The angular velocity of the Earth is highlighted now as vector with the correct components $\mathbf{\Omega} = (0, 0, \Omega sin(\phi))$.

equations 2, 3: why the authors do not express the material or total derivative as D/Dt?

Response: We did not express the material or total derivative as D/Dt as previous publications and especially the literature describing the heart of the numerical code EULAG (cf. Prusa et al. (2008); Smolarkiewicz and Margolin (1997)) use d/dt for the total derivative. As we use the same body of the code, we would like to be consistent with the developers.

line 152: ve is more precisely the geostrophic wind, that is what drives the flow. For simulation P3 the authors should mention what the geostrophic wind and the Coriolis parameters are. Moreover, do they apply them in the nudged simulation SO, SOW and SW as well?

Response: $\boldsymbol{v}_e$ refers to the ambient background flow in the simulation which is predefined in the initialization of the simulations with data assimilation. The ambient background flow for the simulations SO, SOW and SW is derived from the horizontally averaged velocities of the fully developed SBL state of precursor simulation P3 (cf. Fig. 4). The Coriolis parameter in P3 is $f = 2\Omega_z = 1.0 \times 10^{-4}\ \mathrm{s}^{-1}$ for midlatitudes. In the simulation SO, SOW and SW, the Coriolis force is not active.

line 164: what about the rotational frequency of the wind turbine, is it fixed or controlled using the PI controller from Jonkman et al. (2009)?

Response: The actuator disc method in EULAG is implemented according to Mirocha et al. (2014). The rotational frequency of the rotor is calculated according to the inflow velocity at the rotor. A PI controller is not implemented.

line 184: it looks more like a squared sinusoidal function rather than a Gaussian, am I missing something?

Response: This is correct, according to Nakayama and Takemi (2020) it is a bell-shaped damping function. The formulation

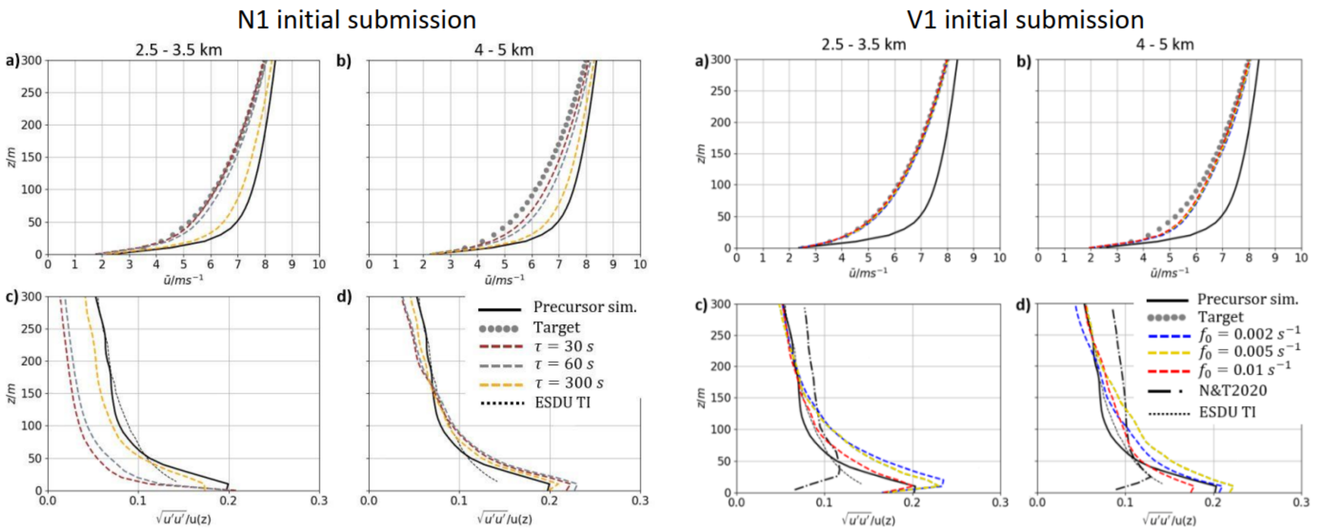

Figure 6. Averaged zonal velocity profiles and streamwise turbulence intensity for the simulations regarding N1 and V1 from the initial submission indicating the performance of different relaxation time scales τ (for N1) and frequencies f_0 (for V1).

is changed:"[...] applied in a nudging area a bell-shaped damping function damp(x) [...]".

line 191: please see my Comment 1 in Sec. 1. There are two major differences in what you are referring to as the Allaerts et al. (2020) method in your paper, and the actual method of Allaerts et al. (2020). Mostly referring to the fact that their method mathematically preserves the nudging to a stationary ABL flow, yours does not. Moreover, their forcing term is applied in the entire domain.

Response: The description of the methods is modified in the manuscript in order to represent correctly the application of the methods compared to others.

line 236: any guidelines on how to find this frequency f0 rather than saying that it should be lower than the peak frequency in the energy spectrum of the precursor simulation? Maybe try to plot the L2-norm error between the target and assimilated profile as a function of τ and f_0 for the Newtonian relaxation and the vibration equation approach for the tests that were not shown in the paper. Some conclusions could be extracted from it. Since the authors are comparing and analyzing the methods, any information on the sensitivity they have performed is welcomed. I really think it would help the reader to gain a better grasp on the effect of these parameters. Please include it.

Response: In the first version of the manuscript we showed an investigation of the impact of different frequency values on the results. However, as suggested by one of the first reviewers, we excluded a detailed discussion of the frequency and now only present the case for the best fitting frequency. For the appropriate determination of the guiding frequency in the model we refer to the work of Nakayama and Takemi (2020) as they described the process for the identification of the frequency. Figure 6 shows the averaged zonal velocity profiles and streamwise turbulence intensity for the simulations regarding N1 and V1 from the initial submission indicating the performance of different relaxation time scales τ (for N1) and frequencies f_0 (for V1).

line 240: please specify how the frictional drag is calculated. I expect it should perfectly balance the streamwise pressure gradient. Why then the friction drag is different between P1 and P2 if the pressure gradient is the same? Please elaborate on this.

Response: Considering the Equations

$$u_*^2 = u^2 c_d$$
$$u = \frac{u_*}{\kappa} ln(\frac{z}{z_0})$$

we derived a drag coefficient of 0.01 for a roughness length of 0.1 m in the 5 m resolution simulation. The value of u_* is adapted from the simulation setup following Nakayama and Takemi (2020). In EULAG we require a drag coefficient, as we have partial slip conditions at the lower boundary and are not applying Monin-Obukhov Similarity Theory. The surface stress model is implemented in EULAG according to Epifanio (2007).

The reason for a higher drag value in a coarser resolution is the dominance of the advective transport, which is faster in higher resolved cases. The drag coefficient enters the subgrid-scale part. So the difference between SGS and resolved vertical advective transport increases in case of higher resolved grids.

Figure 7a shows the mean zonal velocity of the precursor simulations after they have reached a statistically stable state. For the given drag coefficients a good accordance between $< u >_y$ in simulation P1 (coarse) and simulation P2 (fine) can be achieved.

line 249: from the paper it seems that the slice acquisition i started after 148.950 s? This is a very long time for a channel flow simulation to reach statistic equilibrium, also given that the friction velocity is pretty high. In general, I would appreciate more details here, the authors should imagine that, based on this paragraph, one should be able to reproduce their results, even using a different code.

Response: The simulation time is determined by the evaluation of the mean of the TKE in the domain. As it reaches statistic equilibrium the synchronized slices are copied from the simulation domain.

line 256: same as above, these numerical absorbers should be explained. Are they removing turbulence at the outlet? Where can the reader find the exact equations used to implement them? If this is not possible they should be reported.

Response: The relaxation term α at the r.h.s of Eq.2 in the manuscript represents 'fictitious forces whose eventual role is to attenuate the solution to the prescribed states in the vicinity of the open boundaries' (cf. Smolarkiewicz et al. (2007)). $\alpha = 30$ s is used for the outflow boundary in the zonal direction while $\alpha = 200$ s for the domain top. The values and the reference paper are mentioned now in the text.

line 262: why the drag coefficient has been reduced? It is not clear to me what it means "to fit for the velocity profile prescribed in P1". Are the authors trying to achieve the same inflow profile as in P1 case? Why given that P1 and P2 cases are never compared to each other in the paper? Why the authors did not change the pressure gradient? The flow is not in balance then. Maybe the SGS model is influencing this and so the flow is in reality in balance? Please be more specific on the reason behind the adopted choices.

Response: In EULAG the lower boundary is not parameterized by the MOST but instead with a partial slip condition (cf. response to specific comment on line 240). With our choices of drag in the manuscript and the same frictional velocity $u_* = 0.45$ m s^{-1} as in Nakayama and Takemi (2020) we achieve the best accordance with the idealized reference case

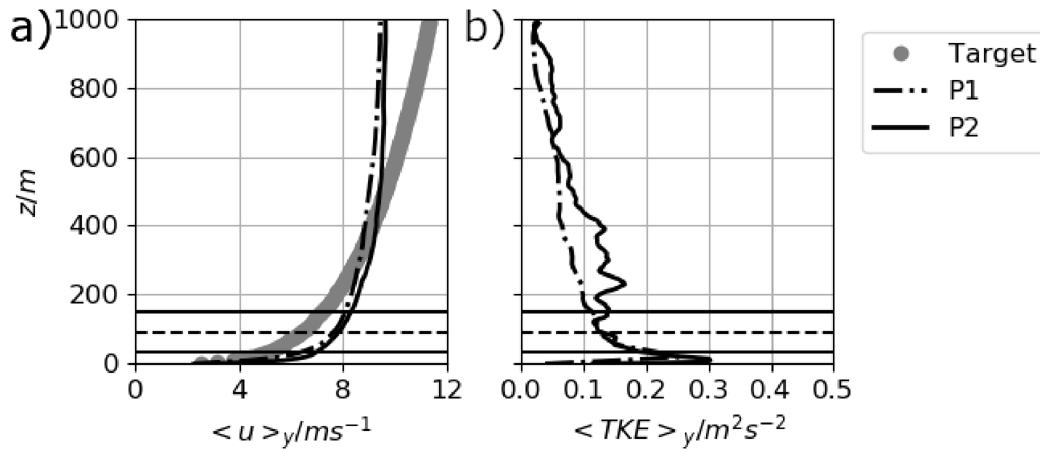

Figure 7. a) horizontally averaged zonal velocity $< u >_y$ (x=3000 m) for the precursor simulations P1 (black dashed-dotted line) and P2 (solid line) after they have reached a statistically stable state. The gray dotted line indicates the target profile for these idealized cases. b) horizontally averaged TKE $< TKE >_y$ (x=3000 m) for the precursor simulations P1 (black dashed-dotted line) and P2 (solid line) after they have reached a statistically stable state.

presented by Nakayama and Takemi (2020) as well as matching zonal velocity profiles comparing the coarse grid simulation P1 with the fine grid simulation P2 (cf. Fig. 7a).

line 269: did the authors re-run the precursor themselves, or the data was taken from Englberger and Dörnbrack (2018)? This is not explicitly stated. The authors mention that here the flow has stratification and veer (as also mentioned in the paper where the numerical setup of the P3 simulation is described). Do they apply the Coriolis term also in the nudged simulation? If yes, what is the value of the geostrophic wind ve in Eq. 2? Did the authors solve for temperature in the nudged simulation? Please see Comment 3 in 1.

Response: The precursor simulation has not been re-run by the authors themselves, the inflow data has been taken from Englberger and Dörnbrack (2018) and used as inflow for the simulation with data assimilation. The Coriolis-force term is not applied in the nudging simulation (cf. responses to comment 1 and 2). Temperature is solved in the nudged simulation SO too (cf. Fig. 2).

line 274: why damping has not been used at the domain top here?

Response: Negative buoyancy during night diminishes vertical mixing in the atmosphere. Therefore no numerical interferences of the top boundary are affecting the atmospheric flow field. A sponge layer at the top did not lead to better results.

line 335: the authors do not explicitly show Nakayama and Takemi (2020) data in their plots, hence they cannot say that results are in close agreement with theirs. I would suggest to use something on the line "similar conclusions can be drawn".

Response: Thank you for the advice. The formulation is adopted and we write now: "Although similar conclusions can be drawn comparing the results in this study with the work of Nakayama and Takemi (2020), there are differences in the flow fields downstream when implementing the Coriolis force, as they do in their setup."

line 338: the authors should really expand on this. When using Coriolis, it is not possible to control the flow with the streamwise pressure gradient anymore, and the geostrophic wind components should be used instead (ve in Eq. 2, to be precise).

Response: There is no Coriolis-force term active and no pressure gradient in the simulations with data assimilation (cf. responses to comment 1 and 2).

line 342: "For wind energy purposes", I don't agree. See the first of my specific comments.

Response: We now write: "In order to resolve smaller turbulent structures and motions, LESs with a higher resolution than that used in Sect. 3 are performed in this part of the study. As the power output and performance of a wind turbine is impacted directly by the atmospheric turbulence it is crucial to resolve a large part of the turbulent spectra in order to simulate the interaction of the rotor blades with the flow more precisely."

line 368: please change "difference between these both Newtonian" to "local Newtonian and Newtonian methods" for clarity.

Response: We now write: "[...] difference between local Newtonian and Newtonian relaxation methods [...]".

lines 372-375: I don't understand the sentence "explains the nearly perfect adjustment of the vertical profile in simulation N2". Why the fact that the fluctuations are suppressed should explain the match in the mean? I don't think these two effects are related in general (see Newtonian and vibration relaxation as an example). Or maybe I misunderstood the text. In either case, please elaborate or rewrite it more clearly.

Response: The sentence is maybe misleading we changed the formulation to:"[...] This behavior in the relaxation region at $x = 1500$ m continues for the downstream position $x = 2200$ m. From Fig.5 it can be concluded that the turbulent structure of the eddies in the simulation domain are severely affected by the local Newtonian method, while the Newtonian relaxation and the vibration method seem to be less intrusive."

376: turbulence characteristics are not "preserved" it would seem. Moreover, this cannot be said. Turbulence is characterized by spatial scales, time scales and lifetime other than spectra. The authors should show all these things to be able to say something about turbulence characteristics. Finally, it is not clear to me why the authors are relating the small changes in the mean to the turbulence characteristics. Maybe the relation is there but it should be clearly motivated.

Response: We agree with the opinion of the reviewer. The passage is completely deleted and rewritten according to the previous specific comment.

line 407: please change "density behind" to "density observed behind".

Response: Formulation changed to:"[...] reach the final spectral energy density observed behind the nudging zone."

line 414: "itself too smooth" is very non technical language, please remove.

Response: Formulation changed:"[...] as the TKE of the assimilated flow fields is significantly reduced and the flow becomes nearly laminar. "

lines 420-421: I do not agree with the sentence "Despite these minor differences, these two methods are suitable for wind-energy applications with grid spacings of up to 5 m". First, the Newtonian approach is not suitable, as turbulence (and the shape of the TKE profile) is completely different and 2-3 times higher at some locations. This will have a huge impact on the wind turbine wake. Moreover, this is a very generic sentence which does not minimally take into account the limitations of the methods. Please adjust it.

Response: In fact, we agree with the reviewer that this sentence very generic. The sentence is deleted and we highlight the main results of Sec. 4 and the limitations of the approach in the end of Sec. 4: "The coarse and fine resolution results show a very similar behavior of the resulting flow field using the Newtonian and the vibration approach. The investigations in this section for the higher grid resolution show an improved capability of the vibration method - compared to the simulations in the coarser grid - to assimilate the mean zonal velocity component while the impact on the TKE is smaller. Compared to the Newtonian relaxation the impact on the TKE is as well less dominant when the vibration approach is applied. The following simulations, which perform an assimilation to observed profiles from the WiValdi wind park, are conducted by using the vibration method only, as, to our knowledge, this method has not been previously tested for wind-turbine relevant resolutions. It has to be emphasized, that our approach includes a nudging region smaller than the numerical domain and omits the Coriolis-force term in the simulation with active additional forcing of the assimilation methods. However, within the scope of interest to generate an inflow situation for a single wind turbine with small domain sizes and short simulation times, we are convinced that the presented model setup is a first step towards this final goal. The performance of the Newtonian relaxation and possible adaptions in the model are investigated by Allaerts et al. (2020, 2023) for an assimilation which is implemented as a grid nudging method acting on every grid point in the domain while an active Coriolis-force term and pressure gradient lead to a geostrophic equilibrium in the simulation. They also investigated the vibration method, but only in combination with the Newtonian approach, with no significant difference in the behavior of their algorithm (Allaerts et al., 2020)."

line 426: please mention that there are differences between the methods. Allaerts et al. (2020) did not use a nudging zone and they had Coriolis force in their simulations.

Response: We changed the whole paragraph, please see the response to the previous specific comment.

line 443 onwards: it is not clear what geostrophic wind was used in Englberger and Dörnbrack (2018) paper to drive the flow, it is not clear what Coriolis parameter has been adopted (presumably 1.0×10-4), how that relates to Ω in Eq. 2 and EULAG? Why the authors mention that they did not use Coriolis in the first two tests — as the flow was deviating from equilibrium — then Coriolis is used here? The flow is not deviating in this case?

Response: The mathematical description of the equations is presented now clearly to avoid any misunderstandings. We hope that we could clarify all questions regarding the nomenclature. The Coriolis-force term is explained now correctly reading Eq. 2 in the manuscript. The application and negligence of the Coriolis force in the different simulations has been explained more detailed in the responses to comment 1 and 2.

line 448: 200 m is very much too close to the nudging, the authors should really show what happens at different downstream locations, until the flow exits the domain.

Response: Fig. 6 now includes more downstream positions in order to show the spatial evolution of the flow (cf. Fig. 2 in this response document).

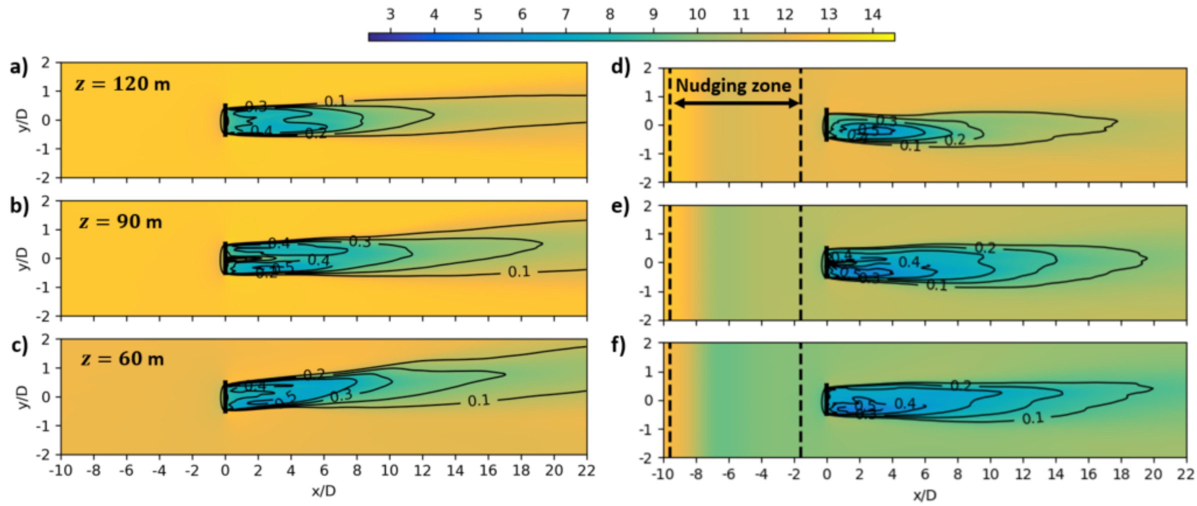

Figure 8. Coloured contours of the time-averaged zonal velocity component $< u_{i,j,k} >_t$ in m s^{-1} without data assimilation in a), b) and c) and with vibration assimilation in d), e) and f) averaged over 20 min at the end of the simulation. b) and e) show the x - y plane at hub height $z = 90$ m. a) and d) (c and f) correspond to the x - y planes at $Z = 90m + R/2 = 120$ m ($Z = 90m - R/2 = 60$ m). The black contours represent the velocity deficit $VD_{i,j,k}$ at the same vertical location calculated in relation to the upstream velocity at $x = $-200 m. The axes are normalized by the rotor diameter D = 126 m, whereby $x/D = 0$ indicates the position of the rotor. The target profile for d), e) and f) is rotated in order to prescribe a spanwise velocity component of $< v >= 0$ m s^{-1} at hub height.

Figure 8: why the flow acceleration around the hub is not seen in the assimilated case? I do not understand why the authors have to cut a large part of the domain from the image. Please justify or include the entirety of the domain, highlighting absorbers if applicable.

Response: In order to show the most important part of the wake we limited the region of interest to the shown spatial dimensions. Showing the whole 5120x2560 m^2 wouldn't be of great value for the comparison of the velocity deficits in the wake especially in the proximity of the rotor. Figure 9 shows the wake behind the turbine for the simulation SW. A clear and comprehensible discussion of the velocity deficit in the proximity of the rotor is in our opinion not feasible. The acceleration around the hub is presumably not visible because of the entrainment of the flow due to the veering wind. Furthermore, the inflow velocity is reduced at hub height due to the assimilation which leads to a reduced rotational speed of the rotor. As a consequence the characteristics of the jet behind the nacelle of the wind turbine are different (e.g. wind speed, elongation of the jet). Fig. 8 presents the simulation SW in comparison to SOW where the target profile for the assimilation is rotated in order to prescribe a spanwise velocity component of $< v >= 0$ m s^{-1} at hub height. This presents a more suited orientation of the wind turbine towards the incoming flow. In Fig. 8e the jet behind the nacelle is slightly more visible than for the strong veering inflow in Fig. 8e in the manuscript. However, we wanted to present the assimilation towards the non-rotated target profile as it is more challenging for the vibration method.

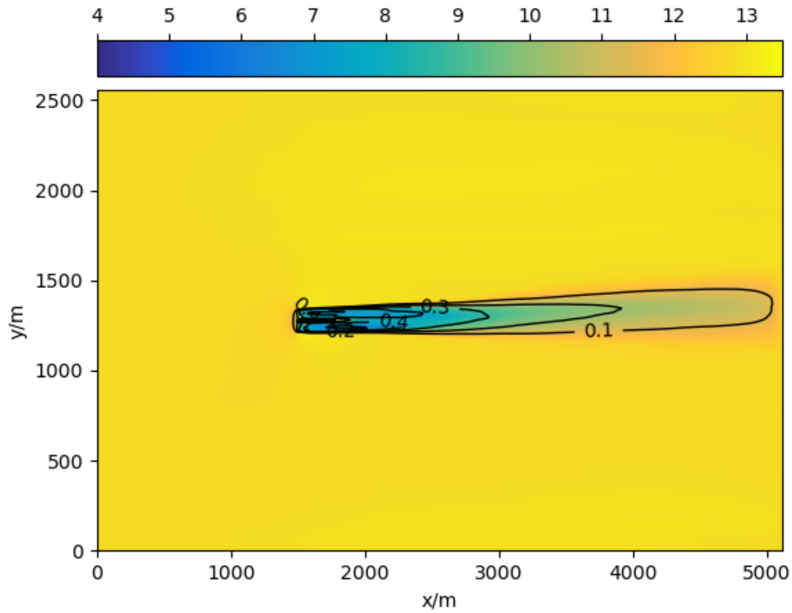

Figure 9. Coloured contours of the time-averaged zonal velocity component $< u_{i,j,k} >_t$ in m s^{-1} for the simulation SW in the whole domain of 5120x2560 m^2.

lines 508-509: "The results for both simulations are in good accordance to other studies" this has never been shown — not qualitatively nor quantitatively — in the paper. It cannot be used as a concluding remark of a section.

Response: The formulation has been changed:" The results for both simulations show the main features of a wind-turbine wake in an SBL. The asymmetry and elliptic shape of the wake (cf. Fig.10) are as well described by Abkar and Porté-Agel (2015) (Fig.17) and Englberger and Dörnbrack (2018) (Fig.6) who considered wind-turbine wakes in SBLs too. The deflection of the wake if wind veer is dominant in near-stably stratified ABL regimes is also described by Bhaganagar and Debnath (2014) (Fig.11) and Mirocha et al. (2015) (Fig.11)."

line 523: "the 3D turbulent structures are preserved", this cannot be said by looking at 2D slices.

Response: The sentence has been changed: "In the case of the Newtonian and the vibration approach, the TKE as well as the power spectra are influenced by the relaxation, whereas turbulent structures are impacted less compared to the local Newtonian approach."

line 568: please change "compare" to compared.

Response: - changed in the manuscript -

lines 568-571: to be really honest, I think that the setup proposed by Allaerts et al. (2020) still outperforms the approach proposed by the authors, as it allows to simulate very large areas without the flow evolving past the nudging region. With this, I mean that it is less idealized than those presented by the authors. Moreover, Allaerts et al. (2020) also nudged potential temperature, which is not done in this paper, raising serious questions on the consistency of the obtained flow profiles without validation against observations. Regarding the assimilation of simultaneous measurements and the application of the method proposed by the authors in complex terrain, they have never been showed in the paper, nor have been mentioned before, so they cannot be used to sell the proposed method against that of Allaerts et al. (2020). This is very bold, data assimilation over complex terrains comes with many more challenges (among which, the wall models going against the assimilation, leading to erroneous turbulence close to the wall) and the authors simply cannot assume that their method is ready to be used in complex terrains unless they can show it. I would rather concentrate on the difference between "domain nudging" and "region nudging" approaches, as well as on the strengths and weaknesses of each of them, and leave the assimilation of simultaneous measurements and the application of the method to complex terrain as future works.

Response: The conclusion is completely rewritten in regards to emphasize the advantages and drawbacks of our setup for the generation of inflow fields for wind turbines. The difference between the sophisticated method presented by Allaerts et al. (2020) and the approach proposed in the manuscript is now made clear throughout the whole manuscript.

It is true that the application of data assimilation methods in complex terrain is a totally different topic compared to the investigations in the manuscript and was mentioned inappropriately at the end of our conclusion. In the revised manuscript we mention it in the introduction as a possibility for the application of the method as it is a very interesting and tempting research field. However, the surface parameterization in EULAG is in principal capable of calculating flows over steep slopes (cf. Epifanio (2007)) which would be a precondition for such investigations. In the introduction we write:"However, the application of this method to high-resolution wind turbines would have the advantage that it can reproduce an assimilation of simultaneous measurements, is not limited to horizontal homogeneity and could possibly be applied in complex terrain."

line 582: which data have the authors used? ERA5? This has a maximum resolution of 1h, not 6h.

Response: The source of the data providing initial and lateral boundary conditions for the WRF simulation is already mentioned in the manuscript. The ECMWF global operational atmospheric analyses, which are available every 6 hours, are produced by assimilating a wide range of observational data (https://www.ecmwf.int/en/forecasts/dataset/operational-archive). This dataset is different to the ERA5 dataset, in that it offers higher horizontal resolution, at the expense of higher temporal resolution. Our outer domain in WRF (D01) covers most of Northern Europe. Provided the large-scale flow is not changing rapidly, which it does not during our period of interest, the use of data with a 6 h temporal resolution is justified.

line 590: if the BC update-time is every 6 hours, then it means that in essence this is a stationary WRF simulation, as it lasts for 7h. Please elaborate. Moreover, it is common practice in WRF to conduct a 1 day spin up before the start time of interest. How much of the 7h has been used for spin up and how much for gathering statistics?

Response: The claim that a 7-hour WRF simulation with 6-hourly BCs is "stationary" is incorrect. The boundary fields are linearly time-interpolated from provided boundary times to the current model time, and, as we provide the model BC data that extend beyond the period calculated (to 00 UTC on 20.11.2021), the BCs evolve continuously in time during our simulation.

Although the reviewer raises an interesting point about the relatively short spin-up period, in reality it depends on the scientific objective. For example, when the focus is on a localized-regional simulation, and because the flow is constrained by large-scaling forcing, the spin-up period for winds does not necessarily need so much time. Longer spin-up times are recommended if there is a strong dependence on the underlying soil representation (soil moisture, temperature) or if there is a need to accurately represent deep convection. In this study, a short spin-up period is justified by the study's focus on boundary layer wind fields, which typically equilibrate quickly to the large-scale forcing.

The main focus in this work is not on the WRF simulations, however they are required in this case with incomplete observational data in order to provide continuous wind and temperature profiles through the depth of the boundary layer. Observational data taken from on-site instrumentation has gaps that need to be filled in. In particular, there is no wind field data below 57 m height; and limited data at larger heights when low-level clouds are present. The initial 6 h 30 m of the simulation is used as spin up and not analyzed. At 18:30 UTC the vertical profiles at the location of the wind park are extracted. A comparison of these WRF profiles with observations is given in a recently published conference paper (Kilroy et al. (2024)). This paper shows that the model is well capable of reproducing the mean wind fields at the WiValdi wind park. In essence, the vertical wind profiles extracted from the WRF model at the time of interest, provided as target profiles for the EULAG simulations, are reasonable and match well with observations. In the end, it is not intended that WRF be used to extend observations for each specified observed case, rather, that for the time chosen here to simulate, we did not have meteorological mast data from the on-site observational network to fill in gaps from the LIDAR data. This intermediate step of using WRF for this simulation is not meant to be included as a part of the approach outlined in Figure 2 of the paper.

Reading Appendix A, it has been impossible for me to set up a WRF simulation equivalent to that conducted by the authors. Several information have to be added on top of the mentioned physical models, such as the domain layout, the projection, the numerical schemes, the workflow strategy, the number of levels present in the global dataset used to derive the boundary conditions and the actual data used. As a general remark, I really invite the authors to be much more specific regarding the setup of their simulations.

Response: As mentioned in the response above, the main focus in this work is not on the WRF simulations. WRF is used here as a tool to fill in gaps in the observational data, to provide a vertical profile of mean winds and temperature, in order to perform a more realistic test case using EULAG. It is not a surprise that it would be impossible for the reader to set up a WRF simulation equivalent to that conducted here, as there are between 250–350 unique variables in a typical WRF setup and they simply cannot all be described in an appendix. The amount of information given here for the WRF model is relatively typical, when compared to other scientific publications utilizing WRF. Some more information on the model setup is given in Kilroy et al. (2024), where this WRF simulation is the focus. We would happily make the namelist files available upon request, should others want to reproduce our setup.

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