Paper Review - WES-2024-12

Title: data assimilation of generic boundary-layer flows for wind-turbine applications - An LES study

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The paper addresses a key challenge in the field of wind farm LES, i.e. nudging the flow solution using real-world observations or data from regional weather models. This allows to introduce the influence of mesoscale physical processes at the scale of the atmospheric boundary layer (ABL) flow, which is otherwise assumed to be laterally homogeneous in conventional, idealized LES setups. The authors compare three different approaches that can be used to achieve this, namely a proportional controller using both the instantaneous (local Newtonian relaxation) and horizontally-averaged (Newtonian relaxation) velocity solved by the LES to compute the error, as well as an integral controller (vibration equation relaxation), where the error is accumulated using the instantaneous LES velocity field. The authors show that, while the controlled variable (velocity) agrees well with the target signal in all methods, turbulence is negatively altered in all cases. In particular, the local Newtonian relaxation kills fluctuations — until laminar flow can even be observed at the exit of the nudging region — while the other two methods increase fluctuations. The authors investigate the sensitivity of the three methods to the grid resolution (two values of grid resolution are tested per assimilation method) and finally apply the vibration method to a weakly stable ABL, with and without an isolated wind turbine simulation.The turbine simulation is compared to the case where the original precursor is used as inflow, i.e. without any type of data assimilation.

I think that this study is relevant to the wind energy community. However, before it is eligible for publication in the Wind Energy Science Journal, several important aspects need to be addressed. In this regard, my major concerns are explained in Sec. 1, while specific and technical remarks are outlined in Sec. 2.

1 General Comments

1.1 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

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

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

$$\begin{cases} \frac{\partial u}{\partial t} - f_c v + F_{u_x}(z,t) = 0 \\ \frac{\partial v}{\partial t} + f_c u + F_{u_y}(z,t) = 0 \end{cases} \tag{2}$$

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

$$\begin{cases} \frac{\partial u}{\partial t} + f_c(V_G - v) + F_{u_x}(z,t) = 0 \\ \frac{\partial v}{\partial t} - f_c(U_G - u) + F_{u_y}(z,t) = 0 \end{cases} \tag{3}$$

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.

1.2 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 nudgng 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) 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.

1.3 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.

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?
- 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.
- 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.

- 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?

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.

2 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).

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).

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

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.

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.

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.

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

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 $f = 2\Omega_z = 2\omega \sin(\phi)$, where ω is the Earth's rotation rate and ϕ is the latitude. Please be precise when writing down the governing equations.

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

line 152: $\boldsymbol{v}_e$ 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?

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)?

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

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.

line 236: any guidelines on how to find this frequency f_0 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.

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.

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.

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.

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.

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 $\boldsymbol{v}_e$ in Eq. 2? Did the authors solve for temperature in the nudged simulation? Please see Comment 3 in 1.

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

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".

line 38: 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 ($\boldsymbol{v}_e$ in Eq. 2, to be precise).

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

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

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.

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.

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

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

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.

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.

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?

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.

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.

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.

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

line 526: "necessary in wind-energy applications", again this is not true (see my first specific comment).

line 568: please change "compare" to compared.

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.

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

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?

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.

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