Response to referee #3

> The manuscript describes a method reduce the spin-up time of Large-Eddy Simulations (LES) used to describe the atmospheric boundary layer, with use of LES for wind energy as an important application in mind.
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> The work carried out may be of interest and may introduce improvements in the methodology for such simulations. However, there is a major concern in the way the method is introduced. Rather than a change of method for the spinup, the authors introduce a change in the frame and objectives of the simulation. The formulation and equations are presented in a confusing and misleading way unfortunately (see 'Main concern' below). Reconsidering the work and the presentation as a whole seems necessary to make it possible for the manuscript to state clearly what is at stake.
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> I therefore advise rejection, but encourage the authors to present their approach and results under a different angle.

We sincerely thank the referee for their rigorous and fundamental critique of our manuscript. Their perspective highlights a crucial philosophical distinction between the classical meteorological objective of simulating a boundary layer and the applied engineering objective of targeting a specific flow state. We appreciate the opportunity to clarify our framing, correct our terminology, and better communicate the physical mechanism of the controller.

Below, we address the referee's main and minor concerns and outline our planned revisions.

Main concern

> The authors compare two sets of simulations as if they were comparable, arguing that the difference comes from the method used to control the spinup and in particular the inertial oscillations that arise during the spin up of these simulations. In fact they are comparing simulations that differ in their setup and objective. Hence the comparison is misleading and the comments and vocabulary used to interpret them is at the very least confusing. Part of the confusion may arise from the fact that throughout the manuscript, the authors always present the pressure gradient, imposed by the large-scale atmospheric state above the boundary layer, only though the form of a geostrophic wind (see lines 20-21, 25, 48)).
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> An insidious effect is that the reader may not realize clearly that modifying the geostrophic wind consists in modifying the external pressure gradient. The difference is particularly clear in the 1D example, described in Section 3. These simulations describe a one-dimensional velocity profile in an atmospheric boundary layer with a constant eddy viscosity. Stationary equilibrium solutions are known (Ekman spiral).

A first set of simulations aims to answer the following question: what wind profile sets up at equilibrium given a given environmental forcing in the form of an external pressure gradient force? Here the simulations aim as simulating the response of a small volume of fluid, near the surface, subject to given environmental conditions (an external pressure gradient, that is fixed).

A second set of simulations aims to answer the following question: what wind profile sets up at equilibrium so that a target velocity is achieved at a given level (typically a target wind speed at hub height for wind energy applications)? Here the simulations aim at having specified wind values at a given level in the interior of the domain, with the possibility of modifying the boundary conditions (the external pressure gradient).

The first set of simulations is called 'NoNudge', the second set of simulations is called 'Nudge'. They differ in their aim. A person interested in the development of the atmospheric boundary layer, given a large-scale atmospheric state (the external pressure gradient) is interested in the first set. A person interested in the flow at hub-height, for a given value of the incoming wind, is obvisouly interested in the second set of simulations.

Using a simulation framework corresponding to 'NoNudge' with the aim of having a given wind speed at a given height leads to a "laborious trial-and-error process to achieve specific wind conditions at a target height" (line 295). It is rather clear that the frame proposed here is of interest for LES simulations aiming at wind energy applications.

However, the way the method is presented and the way the simulations are commented is very problematic:

1. the way the method is presented in equations (1) to (7) seems a convoluted way to present that the tendency for the horizontally averaged wind is simply imposed. In equation (3), a modification to the external pressure gradient is introduced (Delta G) so that this tendency is exactly what is targeted (as defined in equation (2)). It may make sense, for the implementation which involves smoothing and filtering, to write things out this way. But it should be clearly stated: "We impose the tendency for the horizontally averaged wind, as defined in equation (2). This is done by modifying the boundary conditions (external pressure gradient)."
2. The way the simulations are commented is very misleading, or even wrong. Examples are given only from Section 3, which has the advantage of having unambiguous solutions. The NoNudge simulation (organge curves in Figure 3) gives the answer to the question of the wind profile that sets up given an external pressure gradient such that the geostrophic wind aloft is 10 m/s. **Very clearly, the simulation gives the right answer (figure 3).** It has the disadvantage of taking time to adjust to it (figure 4).

> The Nudge simulation (green curves) gives the right answer to another question (the profile such that the wind at hub-height is 10 m/s). Statements as in the end of the caption to Figure 3 ("the NoNudge simulation exhibits a large steady-state error") are misleading, if not wrong. This is not an "error". The simulation is simply not designed to yield a given wind at hub-height.

We sincerely thank the referee for this rigorous physical critique. It highlighted a conflation in our original framing and prompted a significant structural improvement to the manuscript. The referee is completely correct that these issues are deeply intertwined: the physical objective, the terminology used for the boundary condition, and the mathematical derivation must all align. We address these interconnected points as follows:

1. Reframing the Simulation Objectives (The Forward vs. Inverse Problem) We fully concede that the original baseline correctly solved the *forward problem* (finding the equilibrium profile for a fixed external forcing) and that labeling its final state an "error" is philosophically incorrect. In wind energy applications, however, researchers must frequently solve the *inverse problem*: determining the required large-scale forcing to achieve a specific hub-height wind vector.
Planned Revision: We will completely reframe Sections 3 and 4 to focus explicitly on strategies for solving this inverse problem. We will remove all instances of the word "error" regarding the baseline's equilibrium. To provide a physically consistent comparison, we will introduce a new three-tiered experimental setup:

- **Naive**: (Formerly: NoNudge) Assuming the geostrophic forcing equals the target wind (illustrating the Ekman veer and inertial oscillations inherent in the forward problem).
- **Calibrated** (New setup): A new benchmark simulation using a constant geostrophic forcing calculated beforehand using the scaling law in Appendix A to achieve the target wind in steady-state. (This correctly solves the inverse problem assuming the precursor run reaches equilibrium, but we will show it still suffers from transient inertial oscillations).
- **DGN** (Formerly: Nudge): Dynamically updating the forcing. (This demonstrates that DGN not only solves the inverse problem but actively damps the inertial oscillations that plague the static `Calibrated` approach).

2. Terminology: Pressure Gradient Force vs. Geostrophic Wind

We agree that jumping straight to "geostrophic wind" obscures the physical reality that our controller modulates the macroscopic atmospheric pressure field across the domain.

Planned Revision: In the Introduction and Section 2.1, we will first introduce the background forcing as the kinematic Pressure Gradient Force ($-\frac{1}{\rho}\nabla P$). We will explicitly define its relationship to the geostrophic wind vector ($\mathbf{G}$) via the geostrophic balance equation before using $\mathbf{G}$ as our control variable. We will also add the referee's suggested plain-language explanation to the methodology: *"We impose the tendency for the horizontally averaged wind... by modifying the boundary conditions (external pressure gradient)."*

3. The Necessity of the Mathematical Derivation (Eqs. 1–7)

While we will adopt the plain-language summary above, we must respectfully retain the mathematical derivation (Equations 1–7) to explicitly distinguish our boundary-condition adjustment from non-physical Newtonian relaxation. In the LES community, "imposing a tendency" often implies adding an artificial source term directly to the momentum equations (direct velocity nudging), which violates momentum conservation and can dampen resolved turbulence.

Planned Revision: We will preface the derivation with the clear, plain-language intent. However, we will add commentary emphasizing *why* the mathematical steps are strictly necessary: they explicitly prove the Coriolis-induced cross-component coupling (e.g., adjusting the X-axis pressure gradient based on the Y-axis velocity mismatch) and demonstrate exactly how the *current flow tendency* is used to damp inertial oscillations without relying on artificial velocity source terms.

Minor points

> l75 should the authors first write the pressure gradient as a pressure gradient, before switching to its expression using the geostrophic wind? For a reader not familiar with geostrophic wind, this would be helpful. Presently, the authors simply assume all readers are familiar, or will go and seek understanding from cited references. More importantly, writing this as a pressure gradient emphasizes that this corresponds to a large-scale environmental boundary condition.

We completely agree. Transitioning directly to the geostrophic wind vector obscures the fundamental physical boundary condition being manipulated—the large-scale synoptic pressure gradient.

Planned Revision: In the Introduction and Section 2.1, we will first introduce the background forcing as the kinematic Pressure Gradient Force ($-\frac{1}{\rho}\nabla P$). We will then explicitly define its relationship to the geostrophic wind vector ($\mathbf{G}$) via the geostrophic balance equation ($-\frac{1}{\rho}\nabla P = f\mathbf{k} \times \mathbf{G}$) before utilizing $\mathbf{G}$ as the control variable in the subsequent formulations. This will make it unambiguous to readers from all backgrounds that the controller acts by modulating the external environmental pressure boundary condition.