Dynamic Geostrophic Nudging (DGN): A Novel Method for Controlling the Background Flow in Large Eddy Simulation

The manuscript presents a control methodology for achieving a prescribed wind-speed and wind-direction pair at a target height in large-eddy simulations of the atmospheric boundary layer by dynamically adjusting the geostrophic forcing vector. The manuscript is generally clear, and the proposed algorithm is presented in a way that is understandable. However, I have several major concerns about the framing, novelty, physical interpretation, and validation of the method.

Major comments

1. My main concern is not with the mathematical formulation of the controller itself, but with the suitability of using a geostrophic-forcing controller to obtain, or claim to obtain, a statistically steady convective boundary layer. In a convective boundary layer, the boundary-layer depth evolves in time due to surface buoyancy forcing and entrainment at the capping inversion. Adjusting the geostrophic forcing to match a target-height wind vector primarily controls the momentum balance, whereas the boundary-layer growth is thermodynamically driven and only indirectly coupled to the momentum controller.

   This distinction is important because the manuscript frames DGN as a way of avoiding excessive transient boundary-layer growth by shortening the spin-up period. However, the results shown in the manuscript indicate that the inversion-height diagnostic continues to increase in time in both the nudged and non-nudged simulations. This is expected for a convective boundary layer with prescribed surface heat flux. The controller may reduce the time required for the target-height wind vector to converge, but it does not by itself produce a statistically steady convective boundary layer in the thermodynamic sense.

   Therefore, there appears to be a mismatch between the controller formulation and the selected demonstration problem. The method is momentum-based, while the main complication emphasized for the convective case is the thermally driven growth of the boundary layer. The manuscript would benefit from a clearer separation between: (i) convergence of the target-height mean wind vector, and (ii) statistical stationarity of the boundary-layer thermodynamic and turbulence structure. Relevant work on statistically steady convective and stable boundary-layer simulations, such as Grylls et al. (2020) and Bon et al. (2024), should be discussed in this context.

2. A second major concern is that the manuscript does not sufficiently engage with prior work on feedback control of atmospheric-boundary-layer LES. The literature discussion mainly contrasts DGN with direct velocity nudging and trial-and-error selection of a

constant geostrophic forcing. However, this misses important prior studies that already introduced controllers acting through the geostrophic forcing rather than by direct relaxation of the velocity field.

In particular, Sescu and Meneveau (2014) and Allaerts and Meyers (2015) should be discussed explicitly. Allaerts and Meyers (2015), for example, used a wind-angle controller to regulate the direction of the driving pressure gradient so that the hub-height wind direction remained aligned with the desired turbine-array geometry.

The present manuscript therefore should not frame the broad idea of controlling ABL LES through geostrophic or pressure-gradient forcing as entirely new. The more specific contribution appears to be a vector geostrophic-forcing controller that targets both wind-speed magnitude and wind direction at a reference height.

3. The manuscript controls not only the geostrophic-wind direction but also the geostrophic-wind magnitude. While this provides an additional degree of freedom for matching a desired wind vector at the target height, it also changes the governing nondimensional state of the boundary layer.

   The structure of an ABL depends on nondimensional parameters such as the geostrophic drag coefficient (u_*/G), the Rossby number, stability parameters, and, where stratification or gravity waves are relevant, Froude-number-like parameters. Changing (G) during the simulation can therefore alter the nondimensional boundary-layer state, not merely accelerate convergence toward the same physical solution.

   This point is particularly important for the interpretation of Fig. 7. The comparison of turbulence structure between the nudged and non-nudged cases is not necessarily a comparison between two approaches to reaching the same boundary-layer state. It may instead be a comparison between two boundary layers evolving under different effective geostrophic forcings. The manuscript should either demonstrate that the final nondimensional states are equivalent or explicitly acknowledge that DGN changes the large-scale forcing and therefore the boundary-layer regime during the controlled transient.

   A stronger validation would compare DGN against a conventional simulation using the final converged geostrophic forcing inferred by the controller, or against an established wind-angle/geostrophic controller from the literature. This would separate the effect of the controller from the effect of solving a different forced ABL problem.

4. The manuscript identifies the boundary-layer height using the altitude of maximum vertical gradient of potential temperature. This diagnostic is more accurately interpreted as a thermodynamic estimate of the inversion-layer height, rather than a general turbulence- or momentum-based boundary-layer depth.

5. The 3D WRF-LES validation uses a vertical domain of only 1.2 km, with a Rayleigh damping layer applied over the upper 300 m. The manuscript itself reports that the diagnosed boundary-layer height begins to interact with the Rayleigh damping layer after approximately 18 h.

   This raises concerns about using the full 24 h simulation as validation of controller performance and turbulence preservation. If the boundary layer or inversion region interacts with the damping layer, the resulting statistics may be contaminated by the artificial upper-boundary treatment. The authors should either restrict the statistical analysis to the uncontaminated period, increase the vertical domain height, or provide additional evidence that the damping layer does not affect the reported conclusions.

   The concern is not simply that a 1.2 km domain is always inappropriate; rather, in this specific simulation, the authors' own results indicate interaction with the damping layer. This should be treated as a limitation of the validation case.

Minor comments

1. Lines 44–47: Is this sentence misplaced?

2. Figure 2: RMSE and standard deviation are used inconsistently. The manuscript defines the forcing convergence error (epsilon_G) as an RMSE of the controlled geostrophic wind relative to the analytical Ekman solution. However, the middle panel of Fig. 2 and its color bar refer to the standard deviation of the geostrophic wind. These are different quantities. RMSE relative to the analytical target measures convergence to the correct forcing, whereas standard deviation measures temporal variability about the mean. There is no mathematical justification for treating the two as equivalent unless the mean is exactly equal to the analytical target, which must be demonstrated rather than assumed.

   The authors should clarify whether panel b shows RMSE relative to the analytical forcing or the temporal standard deviation of the geostrophic forcing. The figure label, caption, and text should be made consistent.

3. The manuscript would be strengthened by comparing DGN with established wind-angle or geostrophic-forcing controllers rather than only with a fixed-(G) “NoNudge” case. The fixed-(G) baseline demonstrates that DGN is better than a naive forcing choice, but it does not establish superiority over the relevant state of the art.

4. Given the formulation of the controller, a neutral or conventionally neutral boundary layer may provide a cleaner validation case. Such cases are more directly tied to the momentum balance controlled by geostrophic forcing and avoid conflating target-height wind-vector convergence with thermally driven boundary-layer growth.

References:

[1] Grylls, T., Suter I., Reeuwijk, M.: Steady-State Large-Eddy Simulations of Convective and Stable Urban Boundary Layers, Boundary-Layer Meteorology, 175, 309–341, 2020

[2] Bon, T., Bayoán R., Meyers, J.: Stable Boundary Layers with Subsidence: Scaling and Similarity of the Steady State, Boundary-Layer Meteorology, 190:42, 2024

[3] Allaerts, D., Meyers, J.: Large eddy simulation of a large wind-turbine array in a conventionally neutral atmospheric boundary layer, Physics of Fluids, 27, 065108 (2015)

[4] Sescu, A., Meneveau, C.: A control algorithm for statistically stationary large-eddy simulations of thermally stratified boundary layers, Q. J. R. Meteorol. Soc. 140(683), 2017–2022, (2014).