These author comments are in response to reviewer comments “RC1”. The original comments are quoted here in plain typeface for ease of reference, with author comments in bold.

This manuscript examines how the Coriolis force affects wind farm wake recovery using a linearized, steady-state, two-layer model with turbine drag applied to the lower layer. It extends previous work by the same author(s), which mainly focused on wind-farm-induced gravity waves and their effect on the surrounding pressure field. The strength of this approach is its computational efficiency, allowing quick analysis of large-scale flow features.

Agreeing with this comment, we would also note that another strength of this approach is that it affords a theoretical analysis which leads to some useful insights.

The FFT approach gives us a useful tool, and the present work adds to the literature on this modelling approach by introducing the Coriolis term into the vertical wave number which is key to the upper layer pressure / displacement response. However the FFT approach is not the point of this paper. Our focus is the fluid dynamics of how the Coriolis force impacts the wake and how it interacts with pressure gradient forces. The reviewer does not mention any of our derived closed form expressions (e.g. 31,32,45), symmetry arguments (table 1) or new insights such geostrophic adjustments and the “edge jets”. These expressions, arguments and insights are new to the literature on farm wakes.

However, this modeling simplicity sacrifices some interpretability and accuracy. For instance, in Figure 7, the difference between the FFT model and geostrophic theory is roughly a factor of 2, suggesting limited accuracy.

The authors agree that the difference between the FFT solutions and the ‘geostrophic theory’ in Figure 7 may be characterised as a factor of 2. The value in comparing the FFT solution to the geostrophic theory is in demonstrating that for all of the assumptions and simplifications that go into developing the ‘geostrophic theory’ in this context, the theory appears to be fairly successful in predicting the geostrophic adjustment-related response of the wake (as embodied by the full FFT solution). The qualitative response is well represented, and the quantitative match to within a factor of two remains remarkable given the neglect of tropospheric stability (N term), and vertical or horizontal mixing in the ‘geostrophic theory’. The lack of vertical mixing (C term) will cause the theoretical treatment to over predict magnitudes. In this way, we put forward the geostrophic theory not as an accurate replacement for running the FFT solver, but rather as an aid to understanding the role of geostrophic adjustment in these complex interactions for which confidence is gained by this comparison with the FFT result. The authors agree that as currently written the above points are not adequately described. To remedy this, the authors propose to revise the manuscript with the following or similar addition at the end of Section 8:

“The geostrophic theory developed in this section provides some useful insight into how the interaction of the inversion layer displacement, pressure field and Coriolis term affects the behaviour in the wake. The comparisons with the full FFT solution show a qualitative match and an order of magnitude quantitative match. The match is sufficiently close, and discrepancies attributable to known simplifications (especially the neglect of vertical mixing which would diminish the geostrophic balance effect) that the comparison adds confidence in the theoretical description as being of value. The geostrophic theory is not here recommended as a replacement for running the full FFT model, but rather as a useful aid to interpreting the role of geostrophic adjustment.”

Similarly, it is hard to evaluate how applicable the results are to real-world wind farm setups. In particular, the choice of model parameters and constants needs clearer justification. Explaining how specific values were chosen would increase confidence in the conclusions. Most importantly, how is the value of C, meaning f/C, determined? There should be a more thorough discussion of what values are realistic for actual wind farms. Implicitly, presenting results for f/C>1 implies these values are possible. Is that actually the case?

The most commonly used case in this paper if f=C=0.0001 which is representative of mid-latitude, stable conditions. f/C exceeding unity does occur frequently in reality for more stable conditions. f/C being less than unity occurs more frequently for less stable, neutral or unstable conditions. The authors agree that this should be made more clear and more fully explained.

The parameter space for wind farms is huge and we cannot look at every "real" case. Instead, we attempted to show trends. Our closed form expressions should allow the reader to examine parameter influence.

The introduction would benefit from a stronger connection to the existing literature, especially regarding the role of the Coriolis force in wind turbine and wind farm wake recovery, several relevant studies that have not yet been discussed are:

- Dörenkämper et al. (2015), J. Wind Eng. Ind. Aerodyn., 144, 146–153
- Abkar & Porté-Agel (2016), Phys. Rev. Fluids, 1(6), 063701
- Nouri et al. (2020), Applied Energy, 277, 115511
- Englberger et al. (2020), Wind Energy Science, 5, 1359–1374
- Qian et al. (2022), Energy, 239, 121876

These are valuable references. We will try to include them, but we felt they were less relevant to our work than the papers we cited. The authors also recognise, in the light of overall comments, that additional clarification of the distinction between the present work (focussing on the role of Coriolis forcing via geostrophic adjustment) compared to other work (on other, related aspects of Coriolis contribution to wake behaviour) would be useful in a revised manuscript.

In both the abstract and introduction, the definitions of the "Rayleigh contribution" and "Coriolis contribution" to wind farm wake recovery need clarification. These terms are not common in wind farm literature, and their meanings only become clearer later in the technical sections. This could confuse readers who are unfamiliar with the specific framework used here. In particular, the "Rayleigh contribution" concept needs more explanation. I especially want to see some justification for the constant C used in this context.

This is useful feedback. The authors would be happy to more fully explain the Rayleigh friction/vertical mixing terms and Coriolis force term, and to explain and justify the value of C used. A desire to write a brief and compact paper has perhaps gone too far here and sacrificed clarity. A strategy of our analysis was to identify how Turbulence (i.e Rayleigh friction) and Coriolis force contribute to wake recovery. We think this formulation works well at sharpening the questions, so it is important to be clear on what these quantities represent in the formulation used.

While focusing on these two contributions is helpful (as mentioned on line 52), other effects such as pressure effects and turbulent momentum fluxes, are also important. Including a brief explanation in the introduction that there are different approaches to analyzing the flow would be beneficial.

The reviewer may have missed the fact that our Rayleigh friction is a parametrization of turbulent fluxes. We also dig deeply into how pressure gradients work; especially in geostrophic adjustment.

The finding that Coriolis effects become more significant as wind turbine size and the extent of wind farms increase. The Coriolis force influences both the structure of the atmospheric boundary layer and the velocity deficit of the wind farm wake. As a result, previous studies (such as Gadde and Stevens (2025) JPCS, 1256, 012026 and Kirby & Howland, J. Fluid Mech., 1008, (2025)) showed that Coriolis-induced wake rotation can be clockwise or counterclockwise depending on atmospheric flow conditions. The latter demonstrates that wake deflection depends on the Rossby number. Recent studies showed that the Coriolis force can lead to significant wind farm wake deflection, see e.g.

- Kasper et al., J. Renewable Sustainable Energy, 16, 063302 (2024)
- Kirby & Howland, J. Fluid Mech., 1008, (2025)

It should be noted that the Coriolis force can influence flow dynamics in multiple ways, and different studies address these effects to varying extents. The present manuscript focuses only on the direct Coriolis and Rayleigh contributions, offering a simplified representation of the broader dynamics.

Yes. We are aware of these contributions but we decided not to treat veering wind in our analysis.

Overall, the manuscript's conclusion that Coriolis effects deserve more attention in future research appears reasonable. However, as noted in the manuscript, some parameters may have been selected to enhance the observed effects. Therefore, it is important to justify the chosen values clearly. For example, the wind farm size of 1600 km^2 exceeds the dimensions of current real-world farms, although such a size could be plausible in the future. Nonetheless, this assumption should be explicitly acknowledged and discussed. Crucially, as mentioned above, the expected f/C ratio in real situations should be discussed.

This very large cluster size is justified in terms of aggregations of offshore wind farms existing and planned. This can be more fully explained in the text. Also as mentioned above, the f/C ratios can be more fully explained.

The reviewer makes a fair point here. We have provided examples where Coriolis is significant in wind farm wakes, which indicate that its inclusion merits consideration, but we have not shown that Coriolis force is <u>always</u> significant in farm wakes. We have focused on the mechanism by which Coriolis force may act and how stratification may reduce the impact of Coriolis force. We have given a few examples (Table 2) to show how to compute the non-dimensional parameters.

RC1 Additional points

• The quality of the figures is currently insufficient. For example, Figure 1 uses a smooth color bar, but the data are plotted in discrete contours, making it difficult to interpret. Also it is impossible to see where the zero line is. **This will be remedied with revised versions of the figures.**

• The velocity deficits in the wind farms appear to develop quite slowly. Can the authors discuss this observation and comment on how this should be interpreted in the context of the model **The selection of a fairly low C magnitude causes this, and can be explained more.**

• In Figure 7, it is written that the agreement between FFT and geostrophic theory is good. While both show the same trend, I disagree on calling this a good agreement, as the amplitude obtained from both differs by nearly a factor of 2, which clearly matters considerably. **This comment has been responded to above.**

• Why is the domain shown in Figures 1, 2, and 3 not symmetrically around the wind farm? **This is purely as a presentational preference. This can be made symmetric as preferred.** Particularly, why is the vertical range in the direction in which the wind farm wake is deflected smaller? **Assuming this refers to Figure 3, again this is a presentational preference. The negative and positive ranges can be made the same.**

• Table 3: Using a reduced gravity value g' = 10 m/s^2 seems unrealistically high for atmospheric boundary layer flows, as shown in Table 4. Why is this value used for the analysis? **Indeed this is unrealistically high. Indeed this is an omission to not have explained that this unrealistic value is intended to emphasise the physical effects at play, rather than to represent reality in this specific case. This can be remedied in the text**

• The Coriolis parameter is sometimes given as 0.000124 and other times as 0.0001. Please use a consistent value or clarify the reason for the variation. **The former value is an extreme value for very high latitudes which serves a purpose to demonstrate an extreme case, and the latter is a typical mid-latitude value which is realistic for current real world scenarios. This can be more clearly explained in the text.**

• Line 30: Some of the referenced works concern mountain meteorology, not wind farms, as the current wording implies. These references should be removed, or this should be clarified. **The authors find that it is beneficial to introduce established works from meteorology as the atmospheric perturbation analysis is apt to explain wind farms interacting with the ABL. A statement to this effect can be added to the paper, as removing the references would remove proper attribution of foundational work.**

Thank you for the constructive feedback.