These author comments are in response to reviewer comments "RC2". The original comments are quoted here in plain typeface for ease of reference, with author comments in bold.

In the manuscript "Coriolis Recovery of Wind Farm Wakes" the authors present a linearized two-layer model of the effects of the Coriolis force on wind farm wake recovery.

General remarks

Wind turbine and wind farm wakes have been studied extensively using numerical models, both engineering models (e.g., Calaf et al. 2010, Porte-Agen et al. 2020) and numerical weather prediction models (Aitken et al 2014, Rosencrans et al. 2024). The authors developed a linearized two-layer for wind farm wake recovery. The model accounts for the wake recovery by the Coriolis force. While recent work by Heck and Howland 2025 showed that the Coriolis force can play some role in the wind turbine wake recovery that effect is relatively small. Considering the length scale of (in particular) offshore wind farms it can be expect that the Coriolis effect on wind farm wake recovery is larger. However, the study presented in the manuscript does not provide a convincing argument for this hypothesis.

We agree that we have not offered convincing evidence for the importance of Coriolis force overall, for example in an AEP sense. We have provided examples where Coriolis appears to be significant in wind farm wakes, which indicate that its inclusion merits consideration, but we have not shown that Coriolis force is always significant in farm wakes. Our primary goal was to investigate how Coriolis force might act and how it interacts with stratification. Our closed form expressions (e.g. 31,32,45) allow readers to investigate whatever environmental parameters they choose. If the reader expects to see Coriolis playing a strong role in large offshore wind farms, as appears to be implied by the reviewer comment if we interpret correctly, then the present work provides some new insight into conditions when this may or may not be the case, particularly through the mechanism of geostrophic balance and the relation to farm size.

While the authors presented an elegant mathematical model that, for the most part, can be treated analytically, there are several assumptions that are not well articulated. First, the linearization used in the derivation does not properly account for the effects of atmospheric boundary layer (ABL) stability. Not accounting properly for the ABL stability effects likely exaggerates the impact of Coriolis force on the wake recover. Furthermore, specific application, wind farm wakes, imposes certain constraints on the problem that are not addressed in the manuscript. For example, wind turbine and wind farm wake recovery under convective atmospheric conditions is significantly faster due to energetic convective eddies, i.e. requires shorter distance from a wind turbine or a wind farm than under stably stratified conditions. The effect of Coriolis force and associated wind veering in a convective atmospheric boundary layer (ABL) are negligible and therefore the approach presented in the manuscript is likely not applicable to such cases, however, this was not considered since ABL turbulence was neglected. Furthermore, wind turbine and wind farm wakes depend also on wind speed. Under weak winds wind turbines either do not operate or generate relatively weak wakes. This means that the impact of wind farm wakes is most significant under near neutral to weakly stably stratified conditions. This is also ignored in the manuscript.

The reviewer may have missed the fact that we have parametrized ABL turbulent stresses as Rayleigh friction. The reviewer seems to think that we neglected turbulent stresses. We show an important competition between turbulent stresses (parameter C) and the Coriolis force (parameter f).

The Rayleigh friction coefficient(s) are where this model is sensitive to ABL stability. In this way, the effects that the reviewer mentions – e.g. Coriolis being relatively unimportant in convective conditions due to high rates of turbulent momentum transfer, are captured in the model. In previous work (see for example references Smith 2007 and Gribben and Adams 2023 in the manuscript) it has been explained and is referred to in Section 3.3, that the Rayleigh friction coefficient is sensitive to ABL stability conditions. In other words, unstable air provides a high value for coefficient C. In the present work, a new expression (see 14b) indicates directly that in that case the turbulent contribution to wake recovery (FRR) dominates, and correspondingly FCR is small.

The authors treat stratification through reduced gravity, giving values between 0.1 and 10, while never providing a definition of the reduced gravity. If we assume that the reduced gravity is commonly defined as: g' = g \delta \theta / \theta_0 (e.g., Jiang, 2014, JAS), where \theta_0 is ~300, and \delta \theta potential temperature difference between the surface and the top of the boundary layer, then a reasonable value of the reduced gravity for conditions relevant for an operating wind farm is between 0 (neutral stratification) and 0.03 (weakly to moderately stable). Notice that the reduced gravity of 0.1 would mean that the potential temperature difference between the surface and the top of the boundary layer is 30 K. Such strong stability of an atmospheric boundary layer is achievable when the winds and therefore shear are weak. Under such conditions wind turbines do not generate power and therefore there are no wakes.

The authors agree that the reduced gravity term used in this context needs more explanation. It is referred to at the beginning of Section 5.2 but really requires the reader to go to Smith 2010 reference to understand its use here which is probably too much to ask the reader. The manuscript can be updated to improve on this. In this context, it refers to a step change in potential temperature at the inversion. It does not represent a temperature gradient within the ABL as may have been understood from RC2 comments.

Finally, the treatment of turbulence mixing induced by the presence of a wind farm is very simplistic and does not account for the stronger mixing and momentum entrainment induced by the shear at the top of the wake.

As mentioned above, the vertical turbulent mixing is embodied as Rayleigh friction. This is certainly a more simple treatment than is used in other more complex models (e.g. RANS CFD, and many others) in the sense that once you have a coefficient value the model is simple and quick to run. It puts a strong emphasis on selection of a Rayleigh coefficient value which represents the conditions, which the authors consider to be a key challenge in model application to real scenarios. A methodology for selecting a surface-layer-stability-sensitive Rayleigh friction coefficient has been worked out (see references) which is promising and requires further validation. In the current context, the value of the Rayleigh friction formulation is in permitting the analysis of friction vs Coriolis contribution to wake recovery.

Taking all the above into account I do not recommend the manuscript for publication in the present form. The authors should attempt to put their work in proper context of a realistic conditions under which a wind farm operates. An analysis unconstrained by realistic conditions yields unrealistic results and leads to false conclusions.

We agree that a clearer explanation that the examples presented represent mostly a very large cluster, stable wind case, with one carefully selected extreme case, would benefit interpretation. It may also be useful to point out that the closed form expressions allow the reader to explore scale and atmospheric conditions effect for themselves very easily, i.e. without having to implement or run an FFT solver.

Specific remark

- Line 109 – It is stated that "The vertical mixing process is difficult to model." This statement should be qualified – it is difficult to model in simple models like the one presented in the manuscript. **Agreed.**
- Line 110 – Two occurrences of the word "may" should be omitted and/or replaced with "is." **Agreed to modify this. The second "may" can be changed to "are sensitive to buoyancy effects" or similar.**
- Line 112 – It is not clear why is Barstad (2016) cited here when the concpets are fundamental textbook concepts. **We can remove this.**
- Equation (9) – The second term on the left-hand-side should be FRR not FRC. **Thank you . This is a typographical error.**
- Line 177 – "Understanding infinitely wide windfarms" is of no real value, since such a wind farm is unrealistic. **Perhaps we should expand on this. The infinitely wide wind farm modelled by Maas(2023) shows a case where there is a seemingly underdamped harmonic wake recovery response which can easily be compared to eqn 13a. That the '1D' formulation (i.e. infinitely wide) of Section 4.1. seemingly matches, in this regard, a very complex flow solution is interesting. As is of course the work elsewhere in the paper which indicates that geostrophic balance will work to inhibit this response in the real world, i.e. with finite width wind farms.**

- Line 274 – Periodic solutions always wrap around from the exit to the entrace of the domain – the question is how the outflow impacts the inflow and the part of the domain that is of specific interest. **Agreed.**
- Line 301 – Instead of “warm” it should be “farm.” **Thank you . This is a typographical error.**
- Table 2 – Instead of “Inversion strength” better would be “reduced gravity.” **See note above explaining how the inversion strength is represented as a reduced gravity, and that this can be clarified.**
- Table 3 – The values of “reduced gravity” are not relevant for an operating wind farm, a more realistic values should be chosen. **See note above explaining how the inversion strength is represented as a reduced gravity, and that this can be clarified. With this in mind, we believe that the values are realistic.**
- Equations (42) and (43) – instead of “Deficit(y)” a symbol representing deficit should be defined and used. **This can be changed as suggested.**
- Line 489 – However, the model does not include ABL turbulence and its effects, e.g., under convective conditions. This is a serious omission. **As explained above, this is not the case.**
- Line 516 – Symbol “H” should be defined before it is used. **Agreed, thank you for spotting this.**
- Table 4 – It is not clear why would a reduced gravity value be dependent on the fram size (FS). See line 466. **The non-dimensional farm size FS is dependent on the reduced gravity, see line 466. We are selecting realistic values which help explore the FS parameter space.** In particular, the value of 0.1721 is likely unrealistically large for an operating wind farm. **See note above explaining how the inversion strength is represented as a reduced gravity, and that this can be clarified. This value pertains to a potential temperature step change of 5K, which is considered to be a large but not unrealistic value.**
- Line 540 – It is not clear what is meant by “low wind,” turbines do not operate below 3 m/s. **We could clarify this. A lower wind gives more time for Coriolis forces to act before dominated by friction forces, but of course only relevant for wind farms above cut in wind speed. We will consider expanding on this use of ‘low wind’**