This file consolidates all of the ‘Report #1’ and ‘Report #2’ suggestions for revision provided by referees 1 and 2 respectively, as well as the author responses to these suggestions. The referee suggestions are in plain typeface, the author responses are in **bold**, and where the manuscript has been changed accordingly this is indicated in *italics*.

Both referees have clearly reviewed the manuscript in great detail and offer salient and important observations. The authors appreciate the time and dedication required to accomplish this.

Report #1

Reference correction: “Engleberger et al. (2020)” → “Englberger et al. (2020)”.
Thank you. *Manuscript corrected accordingly at line 48 in Section 1*.

Line 162: Clarify the definition of C in clearer and more accessible language/format.
We expect that the referee refers to line 142 rather than line 162. A slightly fuller explanation has been given, making it clearer that C is the constant of proportionality for Rayleigh friction. *Manuscript has been revised accordingly on lines 142 and 143 of Section 3.3*.

Table 2: Provide more explanation or references for the parameter values (e.g., Troposphere Stability, Lateral Diffusivity).
References have been added. *Manuscript has been revised accordingly on lines 396 to 399 of Section 6*.

Farm widths statement: The phrase “Typically, within two or three farm widths” seems too strong without sufficient evidence. Were enough cases analyzed to justify “typically”?
This is a fair point. We have changed “typically” to “for the case studied”. *Manuscript has been revised accordingly on line 620 of Section 10*.

Line 664: Make clearer that “Coriolis and outer wake acceleration” effects can be important in some scenarios, but will not always be important. This motivates the inclusion of extreme cases to illustrate the effects.

We expect that the referee refers to line 624 rather than line 664. We have addressed this comment by adding to the closing paragraph of the Conclusions section. *Manuscript has been revised accordingly on line 631 of Section 10*.

Report #2

While the authors addressed all my specific comments their replies to my general comments still leave some of my concerns regarding the limits of applicability of the simplified two-layer approach to a complex problem of relative impact of Coriolis force on wind farm wake recovery open. In the revised manuscript the authors still did not provide sufficient justification for critical parameters of the model presented in the manuscript as relevant to a realistic situation. The problem of the impact of Coriolis force on wind farm wakes is a complex practical problem that can be treated in realistic way using high resolution numerical simulations. When this problem is treated with a simplified approach there is a significant danger of reaching misleading conclusions.

The authors do not disagree that high resolution numerical simulations are worthwhile. Analysing simplified models, for example as is pursued in this paper, can allow additional insights to be gained on the underlying mechanisms that are important in the flowfield. It is important to understand all of the assumptions going into the model, which is why they are carefully explained.

For example, the authors conclude that "When FS > 1, the Coriolis Recover (CR) is effective in accelerating air in the "inner" wake. By pushing/pulling the adjacent "outer" wake air leftward, it also creates narrow "edge jets" to the left and right of the jet. In the opposite case of FS <1, the CR in the inner wake is weak as most of the geostrophic adjustment occurs via the PGF rather than flow acceleration." Here, FS is non-dimensional farm size, defined as a ratio of the farm half width and the Rossby Radius of Deformation (RRD) defined as $\sqrt{(g'H)}/f$. The case FS > 1 implies that the farm half width is larger than RRD. The authors use 0.017 ms-2 as the lower value of the reduced gravity. This value corresponds to a 0.5 K inversion, a very weak inversion that most likely occurs only as a transient state. **The authors agree that this represents a weak inversion. (This value appears in Table 4). Our appreciation that this represents a weak but not unrealistic inversion is based on our analysis of radiosonde data. As is explained in the text, we have selected a value that deliberately constructs a lower limit estimate for FS, and we think it is clear that this is not very precise, but nonetheless results in an order of magnitude, or arguably more, difference between realistic values for small and large FS values.**

A more realistic value for the reduced gravity would be between 0.06 ms-2 (corresponding to a weak 2 K inversion) and 0.3 ms-2. **From our own analysis of radiosonde data, there are soundings where the inversion is undetectable so we do think that 0.5 K is a reasonable low value to assign. Also, the referee may have missed that the Table 4 ranges are used in the paper to demonstrate chiefly that the FS value is less than unity, and similarly to indicate an expected range of Froude number, rather than to assert these conditions (e.g. weak inversion case) are frequently occurring.**

Reduced gravity of 10 ms-2 listed in Table 3 is not realistic and the corresponding case should be omitted. **The authors do not agree with this point. It is made very clear (see lines 390 to 395) that this high value is not realistic, and is included to illuminate stability's role. The inclusion of case c is really useful in highlighting the strong role that the PGF plays in the balance of forces: as it is very strong in case c so its effect can be clearly discerned, thus better understood for realistic cases.**

A realistic atmospheric boundary layer height, H, is between 100 m and 3000 m. Since the Coriolis parameter at 45 degrees latitude is f ~ 0.0001 the range of realistic values of RRD is between 24 km and 316 km. Currently, the largest wind farm in the North Sea, Seagreen wind farm, covers 2830 km2 with the corresponding half width of ~27 km. The conditions when FS > 1 occur only when an atmospheric boundary layer is shallow (i.e. stably stratified) and the inversion capping the boundary layer is relatively week. **Agreed (that the FS>1 condition will be at most very infrequently occurring in the context of current wind farms and wind farm clusters).**

Therefore, the speculation that the observed edge jets could be due to the Coriolis effect (including in the additional reply) is not likely correct. **First of all, the FS>1 condition should not be seen as a threshold either side of which the flow regimes suddenly switch. Flow regimes with FS < 1 but approaching FS=1 will have an increasing degree of the FS>1 type of behaviour. Secondly, the augmentation of the observed edge jets in this paper can be clearly attributed to Coriolis (i.e. Coriolis being added to the force mix) because we can contrast with and without Coriolis force results, e.g. Fig1a versus Fig 1b and in Fig 9. Fig 9 in particular clearly shows accelerations outside the wake with f=0, but much greater accelerations with non-zero f.** A simpler explanation is that the jets are resulting from blockage effects. Blockage effects were previously recognized in both observation and simulations (e.g., Hasager et al. 2023, Sanchez Gomez et al. 2023, Schneemann et al. 2025). **Our statements on edge jets may have been misinterpreted as intending to convey that any real-world observations of accelerations are solely attributable to Coriolis, but this is not the case. A separate study would need to be made to ascertain the physical causes of wake-edge acceleration observations e.g. how much is attributable to aggregated turbine blockage and how much may be attributable to the modelled Coriolis-related edge jets. We draw your attention also to the observation that in our models the Coriolis-related accelerations are at the side of the wakes not the wind farms, see Figure 1: "Cases b and c have small regions of negative deficit (i.e. wind speed above ambient) to the left and right of the wake".**

Considering the assumptions made in the development of the simplified, two-layer model, the authors should address in greater detail the limitations of the model. For example, when applying similar two-layer model, the co-author, Smith (2010, cited in the manuscript) stated: "The present model of pressure gradients, gravity waves and BL response is oversimplified however. Future work should include wind shear and turbulence within the BL. A numerical large eddy simulation (LES) may be required." **The authors mostly disagree with the view that the simplifications and assumptions inherent in the models developed are not clear. However, in case there is any residual risk of misinterpretation, we have added some words highlighting that this is a purposefully simplified model to the beginning of Section 10.** *Manuscript has been revised accordingly on lines 600 and 601 at the start of Section 10.*

There is tension in the review comments concerning realism versus understanding basic principles, with the thrust of the work concerning the latter and the referee seeming to prefer the former. We suggest that the insights afforded by the simplified models/closed form solutions developed here are useful and it is hard to conceive how the same insights would be manifest from running high fidelity simulations. We have added a closing statement recognising that validation of modelling results is always constructive in case there is any doubt on that point. *Manuscript has been revised accordingly on lines 635 and 636 at the end of Section 10.*