[wes-2024-79] Reply to editor

Dear Professor Archer,

Thank you for your comments regarding the reviewer 2's comments. We have thought through these comments very carefully and made some further changes to our manuscript. However, we still do not agree with some of the reviewer's comments, as you will see in our response to the reviewer. We believe that we have sufficiently explained about our opinions (on why we do not agree with some of the reviewer's opinions) in our previous and current responses. We hope our response will be found satisfactory by the reviewer, but please let us know if we need to make any further changes.

[wes-2024-79] Authors' 2nd response to Referee #2

We thank the referee for reviewing our revised manuscript. Following their additional comments, we have made some further changes to the manuscript (highlighted in blue).

For the first point, regarding the assumption of C_T' = const., we have added the following sentence after Eq. 4 (page 4):

Note that the two-scale momentum theory summarised in Section 2.1 is for general cases where the turbine thrust T_i and power P_i may vary across the farm, whereas the analytical model described here is for less general cases where the turbine resistance coefficient C_T' is constant across the farm (such as the LES cases shown later in this paper, where C_T' is fixed at 1.94 for all turbines in the farm).

With regard to the "main point" of the referee's comments, i.e. "the physical interpretation of η_{FS} and η_{TS}", now we understand that the referee's concern mainly comes from the fact that the 50 LES results reported by Kirby et al. (2022) were only for idealised infinitely large farms. Because of this, we suspect the referee did not think that the key finding of Kirby et al. (2022) for those 50 infinitely large farms (i.e., C_T* may only slightly exceeds the theoretical prediction of ND20) was applicable to finite-sized farms in general. However, as the referee also agreed in their comments, our new LES results shown in the present paper have confirmed the two-scale separation for finite-sized farms; in particular, we have shown that the "internal" thrust coefficient C_T* is insensitive to "external" conditions. This two-scale separation means that the upper limit of C_T* (with respect to the turbine layout, for a given value of C_T') should also be insensitive to "external" conditions, and therefore, the aforementioned finding of Kirby et al. (2022) for infinitely large farms is expected for finite-sized farms as well. This is why we believe that it is reasonable to state that the performance prediction of ND20 is "near-ideal" for finite-sized farms as well.

To better explain about this point, we have added the following sentences after the first paragraph of Section 4.3:

*"It should also be noted that the 50 LES results of Kirby et al. (2022) are for idealised infinitely large wind farms; hence, their findings are not directly applicable to finite-sized farms in general. However, as shown in the previous section, our new LES results indicate that the internal thrust coefficient C_T**

is insensitive to external conditions. This means that the upper limit of C_T (with respect to the turbine layout, for a given value of C_T') should also be insensitive to external conditions, supporting our argument that the 'near-ideal' farm performance predicted by Eq. (6) is a good measure for finite farms as well."*

We also understand that the referee's concern comes from our use of the terms "turbine-wake interaction" (causing the reduction of η_{TS}) and "farm-atmosphere interaction" (causing the reduction of η_{FS}). For the former, we would like to clarify what we mean by "interaction" here. When η_{TS} = 1 (or when we say "there is no power loss due to turbine-wake interaction") we do not mean "there is no turbine wake". There are, of course, turbine wakes in the farm, and we have never ignored their existence. What we mean by η_{TS} = 1 is that, even though there are turbine wakes in the farm, those individual turbine wakes (or more specifically, local flow regions having a lower flow speed than the "average" flow speed) are not directly causing the reduction of downstream turbine power (in the sense that how the downstream turbine power would be reduced if they were located in such a locally slower flow region). We tried to demonstrate and explain about this as clearly as possible in Section 4.1 of our original manuscript, but now we have also made the following changes in Section 5 (Discussion) and Section 6 (Conclusions):

"In this study our LES results showed that, for a large staggered array of 160 turbines, the downstream power degradation was not due to turbine-wake interactions, i.e., individual turbine wakes (or more specifically, local flow regions having a lower flow speed than the "average" flow speed) were not directly causing the reduction of downstream turbine power (in the sense that how the power of downstream turbines would have been reduced if they had been located in such a locally slower flow region)." (Page 23)

"The present study further supports the argument that farm-scale flow effects could play a leading role in power losses for large offshore wind farms." (Page 24)

"These results suggest that farm-scale flow effects could play a leading role in power losses in large wind farms." (Page 25)

For the term "farm-atmosphere interaction", we agree with the referee that this term would remind the reader of "atmospheric flow features, like Coriolis, stability, mesoscale circulation, blockage, gravity waves". However, we still believe that it is reasonable to use this term to explain about the reduction of η_{FS} (or the reduction of farm-average wind speed β) since these atmospheric flow features are indeed the factors affecting the wind extractability factor ζ, which in turn determines β and thus η_{FS} (for a given set of "internal" conditions, provided that the two-scale separation is valid). The point here is that $C_{p,Nishino}$ (Eq. 6) has been given as a function of ζ, which, by definition, captures all these atmospheric flow effects (through all mechanisms of momentum transfer between inside and outside the farm, including the turbulent momentum transfer noted by the referee).

However, we admit that our current analytical model of ζ (Eq. 20) is a highly simplified model and ignores some of such atmospheric flow effects. We have therefore checked our revised manuscript and confirmed that we are not giving a wrong impression that our analytical model (Eq. 20) fully captures the effects of such farm-atmosphere interactions.

We have also either removed or replaced some of "farm-atmosphere interaction" in our manuscript with a more appropriate expression, such as *"vertical mixing due to turbulence"* (page 20).

We have also changed the statement "*the downstream power degradation was not due to turbine-wake interactions but entirely due to the farm-atmosphere interaction*" (in the first paragraph of Section 5) as we agree with the referee that this statement was misleading.

We hope that the above response to the referee, together with the additional changes made to the manuscript, will be found satisfactory by the referee and the editor. We thank the referee again for all their comments.