Dear Pieter Gebraad

Thank you for your follow up comments - please, find our comments to those below in red.

-Yes, it seems that the combination of RPM and pitch is still interesting to still consider in axial-induction based wake control (perhaps mostly for closely spaced wind farms), as was discussed in earlier work "Optimal open loop wind farm control" by Vitulli.

We assert that for a *given* Cp contour, the intelligent choice of Ct is the smallest possible, because it results in the smallest/weakest possible wake. This is justified in our earlier work "Optimal open loop wind farm control", and can in general only be obtained using two design variables (pitch and rotational speed). The superiority of this strategy is in our opinion independent/unaffected of/by WT spacing - the weakest possible wake for fixed production is, in our opinion, always preferable from a wind farm production perspective.

I would still like to add some critical notes to your reply:

- Deshmukh and Alison used an early version of the FLORIS Multizone wake model with a FLORIDyn extension (aimed at modeling wake steering) for optimizing axial induction based control. This version of FLORIS/FLORIDyn Multizone has been shown to be inaccurate for optimizing axial-induction based control.

Thank you for the information about the inaccuracy of the FLORIS/FLORIDyn Multizone version used by Deshmukh and Alison (2017). This is of course unfortunate, but their paper has not been redrawn or updated, and we have not found public work that states that their conclusions are invalid, or that they are using an inaccurate FLORIS/FLORIDyn Multizone version. In fact, they do not mention FLORIS nor FLORIDyn in their paper. On this basis, we cannot dismiss their work as it is the only other study that combines layout and derating optimization as far as we know. We will, however, keep it in mind and not draw conclusions based on their work alone.

- Extensions to the FLORIS Multizone model are made in Annoni et al (DOI: 10.1002/we.1891) to match LES results with axial-induction based control, in which case the predicted benefit of axial induction based control disappears (at least if we use pitch control separately).

We had a look into the mentioned publication by Annoni et al (DOI: 10.1002/we.1891). With the 3.3% power increase, seen in the field study by van der Hoek (2019), in mind we find it strange that Annoni et al. finds no benefit of axial-induction-based control. The suggested empirical ad hoc modification of the basic Jensen top-hat model for turbines on a single row (fully overlapping wakes) is based on a fitting to SOWFA (CDF LES) simulations for one single ambient inflow speed (8m/s) and one ambient turbulence intensity (6%). For the investigated cases, the SOWFA simulations shows very little effect of the investigated de-rating cases. However, these are, as you correctly mention, obtained by adjusting the (collective) pitch setting only, which is sub-optimal (cf. the above comment on an optimal WT de-rating setting). The FLORIS-fitting is based on an adjustment of the Jensen expansion rate, which is argued to relate to the upstream WT axial induction parameter. It is difficult for us to follow the physical reasoning behind this relationship. We consider the downstream linear wake expansion assumption in the Jensen model to be primary dictated by wake meandering (cf. E. Machefaux et al. (2014). Empirical Modeling of Single Wake Advection and Expansion using Full Scale Pulsed Lidar based Measurements; WE) and consequently associated not only with turbulence intensity but also with turbulence structure (primary turbulence length scale). The paper by Annoni et al. clearly illustrates, that model simplifications comes with a price - more to this in the following.

- In your reply you state that Fuga is "not able to capture all aspects of wake flow and WT interaction", while in the paper, the model is advertised as a "full-blown CDF (sic.) simulation of the complex WPP flow field with its complicated WT wake interactions". There seems to be a mismatch in formulation. I think it would also be good to refer to the state-of-the-art in this field of research, where wake controls optimizations are in fact done with LES code (for example https://doi.org/10.3390/en11010177).

Perhaps this formulation about the capabilities of Fuga is unclear. What is really meant is that no model - including Fuga - is able to capture all aspects of wake flow and WT interaction. CFD LES is also an approximate approach, where the smaller scales are modelled, and where the precise interface to mesoscale flow boundary conditions is still an open question. Regarding the actuator line approach often used together with CFD LES simulations (also in SOWFA), work is still ongoing regarding the regularization kernel convolution of the blade forces (to avoid singularities in the numerical formulation), which also has an impact on WT power production (cf. recent work on Actuator-Line-Smearing-Correction by Alexander Meyer Forsting, Georg Pirrung and Néstor Ramos García; Wind Energ. Sci., 4, 369–383, 2019). Thus, high fidelity CFD simulation is not an unambiguous concept … and not necessarily the universal truth.

Fuga is a "full-blown CDF solver" in the sense that the solver consistently solves a set of first principles NS equations. This set is simplified as well as conventional CFD RANS and CFD LES are simplified, but it remains a "full-blown CDF solver".