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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Vortex Particle-Mesh simulations of Vertical Axis Wind Turbine flows: from the blade aerodynamics to the very far wake" *by* Philippe Chatelain et al.

Anonymous Referee #2

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The paper is well written and presents a high quality work, that is both interesting and relevant.

Title: the title of the paper as a sub-title: "from the blade aerodynamics to the very far wake". The actuator/lifting line method does not present detail at chord level; therefore, although the model is suitable for blade scale aerodynamics, the current formulation of the title is not accurate. The analysis is limited to eight diameters downstream, for which the reference to "very far wake" is not accurate. I suggest revising the title.

Abstract: the authors mention "very long distances". I suggest a more precise characterization, as for example, "up to 8 diameters downstream".

Comments in the body of the paper:

1. P5, fig 1 and its discussion, it is stated that for TSR=4.5, the results compare well with experiments, although Fn is clearly underpredicted.

2. P7, the maximum angle of attack are not at \theta=90 and 270 degrees

3. P9, "At the design TSR,the blade exploits the delayed stall at its most:" – please explain what this means

4. P9, "this mechanism, most visible in fig 4a, is well known in vortex dynamics and had already been identified on aircraft wakes." I suggest a figure where you highlight this event. It is a too complex process for a reader to follow form this short description.

5. P9, "One needs to add additional terms to enforce an outflow conditions for this otherwise clipped vorticity field; the present study does precisely that by enforcing a normal outflow velocity through its Fourier-based solver." This explanation is not clear. a. The comparison with the work of Scheurich uses TSR as a term of comparison; wouldn't loading be a more relevant term? The strength of the tip vortex is dependent also on the airfoil used; are these comparable? b. Other authors have used free wake vortex filament models, and have seen the same effect of inboard motion. How does this hold with the suggestion that a term should be added to the FFT solution in a meshed domain?

6. P10, "The number of blades also has a strong influence; the two-bladed machine of Section 3.1 exhibits such vortex-blade collisions, in spite of its high loading." How does this relationship work?

7. P11, For the discussion about figure 6, to get a better insight into the mechanism of turbulence creation in the wake corners, would it maybe be valuable to add an impression from the side as well? Then it is easier to see what happens in the corners.

8. P12, "The deformation of the velocity deficit clearly hints at the presence of mean streamwise vortices along the corners of the wake, a clear departure from a HAWT

wake." This is only true for a HAWT subject to axial flow, but certainly not for inclined inflow for which a lateral force component is apparent (which is in practice always the case).

9. P12, "..., which makes the use of a pointwise velocity deficit unsuitable." - suggestion -Which makes the definition of a velocity deficit profile based upon a single characteristic point unsuitable.

10. P13, The value of S1, due to continuity equation, should always be zero, except for the fact that Uinf is corrected to the local velocity outside of the wake, which is larger than Uinf. Please explain the use of S1, and its modified application.

11. P13, maybe add a reference for the x/D$\sim$50 statement? And probably a discussion about the decay laws for HAWTS, in practise these seem to deviate significantly from bluff-body flow rules in case of turbulent inflow.

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