# Review of *Optimal closed-loop wake steering, Part 2: Diurnal cycle atmospheric boundary layer conditions* by Michael F. Howland et al.

Reviewer: M. Paul van der Laan, DTU Wind Energy

The authors investigate the benefits of different wake steering control methods to enhanced the power production in a wind farm subjected to a transient atmospheric boundary layer using large-eddy simulations. The article is well written and I only have a small number of comments. Note that I am not a control expert and I have chosen to focus on the overall methods. I have listed main and minor comments below, which should be addressed in order to accept the article as a publication for Wind Energy Science.

**Main comments**

1. Line 50: *Stable ABL low-level jets are generated, in part, by inertial oscillations induced by Coriolis forces (Van de Wiel et al., 2010)*. The mean effect of the low-level jet can be explained in a more simple way see a recent work of the reviewer van der Laan et al. (2021), where it is shown that the Coriolis-induced wind veer causes the jet. Once the wind veer is removed, using the veer-less ABL model, then the jet is neither present. This is the case for analytic solutions, RANS models and LES. (van der Laan et al. (2021) does not include LES results but a colleague has tested the veerless model in an unpublished work using the GABLS test case from Svensson et al. (2011) and neither got a jet.)

2. Figure 2: What is the spacing between the wind turbines? You should mention this important parameter in the text or you could plot the layout normalized by the rotor diameter and use grid line in the plot. It looks like the chosen spacing between the wind turbines in $y$ is relatively small (4D?), which enhances the benefits of wake steering. I think you should note that you are investigating a relative small spacing and that your energy gains due to wake steering are expected to be less for large wind turbine spacing. Why didn't you use a more realistic wind farm layout representing a modern wind farm? In other words, how would your main conclusions on yaw control methods change for different wind farm layouts?

3. Wind direction and stability: Do I understand correctly that you are both investigating the effect of stability and wind direction at the same time and that stable and unstable conditions reflect South-Western and Western wind directions, respectively? If yes, then it is not fair to compare stable with unstable directly (as you do in the paper in lines 340-342, Table 1 and elsewhere) because they represent different wind direction flow cases. If you want to compare stable with unstable, you should have the same wind direction or you could perform multiple wind directions for both stable and unstable such that your model results represent the same wind rose. If the latter is not desired by authors then you should at least rename and clarify the cases (to for example something like stable-SW and unstable-W or stable-diagonal and unstable-row). If the wind direction is the main parameter of interest for the yaw control optimization studies, then you could have used a quasi-steady stable and unstable ABL using a geostrophic wind direction that varies over time (in order to get the same wind direction cases).

4. Section 4.1 and Figure 6: Do I understand correctly that you derive a power-yaw relationship for the two leading wind turbines using the LES results where all wind turbines are active? This can lead to different results compared a wind turbine in isolation, which is normally used to estimate a power-yaw relationship. I think you should mention this in the article.

5. Appendix C: Note on wake steering LES initialization: You mention that you cannot get the same result by running the same LES on a different number of CPUs. First if all, I appreciate the fact that you also report the model challenges. I agree that the chaotic nature of the real atmospheric makes it impossible to measure the same ABL twice. However, for idealized CFD simulations of the ABL, one should be able to get the same result regardless of the number of CPUs, as long as the number of CPUs does not affect the number of cells in the numerical grid, the use of random number generators is avoided (or used with a fixed seed) and the communication between CPUs is handled in a consistent manner. The LES model of the in-house CFD (finite volume) code of DTU Wind Energy (EllipSys3D) does currently not have the parallelization issue. However, we have noticed that round off issues due to inconsistent communication between CPU's previously led to a different times at which the turbulence started to form (which has been fixed). The authors are welcome to contact the reviewer (after the review process) for further discussion.

**Minor comments**

1. Section 3: You could adde that you use a barotropic atmosphere since you use a constant geostrophic wind speed.
2. Figure 5, caption: Hub height velocity should be hub height wind speed (or stream-wise velocity?).

## References

Svensson, G., Holtslag, A. A. M., Kumar, V., Mauritsen, T., Steeneveld, G. J., Angevine, W. M.and Bazile, E., Beljaars, A., de Bruijn, E. I. F., Cheng, A., Conangla, L., Cuxart, J., Ek, M., Falk, M. J., Freedman, F., Kitagawa, H., Larson, V. E., A., L., Mailhot, J., Masson, V., Park, S., Pleim, J., Söderberg, S., Weng, W., and Zampieri, M.: Evaluation of the Diurnal Cycle in the Atmospheric Boundary Layer Over Land as Represented by a Variety of Single-Column Models: The Second GABLS Experiment, Boundary-Layer Meteorology, 140, 177–206, https://doi.org/10.1007/s10546-011-9611-7, 2011.

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