Review of "On the self-similarity of wind turbine wakes in complex terrain using large-eddy simulation", by A. S. Dar, J. Berg, N. Troldborg, E. G. Patton

This paper studies self-similarity of turbine wakes sited on complex terrain using large eddy simulation (LES). Five simulations with varying surface features and number of turbines are performed. Basic features of wake-terrain interaction, that have been observed previously, are reaffirmed here. These include horizontal and vertical deflection of the wake centerline, and sensitivity of the turbulent kinetic energy and rate of wake recovery to terrain. The primary novelty of this paper is assessing whether and under what conditions the mean velocity deficit shows self-similarity.

This is an important topic and is of use since self-similarity is an underlying assumption in several analytical models. The paper is well-written and clear to follow. There are a few issues related to grid independence, where self-similarity is studied, and self-similarity of quantities other than the mean velocity deficit, that should be clarified to further improve this manuscript. Please see below for points that should be further clarified.

Major Issues:

1. The authors establish grid independence of their results by comparing two cases with different grid sizes for the same steep topography (probably cases 3 and 4 in Table 1). As the grid is refined, more features of the topography are resolved. The differences in terrain height between the coarsely sampled and the finely sampled cases seem to be about 30 m (from Fig. 6, panel 1D), which is quite large compared to the 80 m turbine diameter. As a result, the differences between cases 3 and 4 mentioned in the paper (e.g. the 15% difference shown in Fig. 7) can be ascribed partly to grid size and partly to differing terrain. Given this, a grid independence study should be performed with terrain remaining 'frozen' across grid resolutions. This will enable the authors to comment exclusively on the influence of the grid without contamination by the influence of terrain.

2. It isn't clear from the text in which horizontal and vertical planes the self-similarity is being evaluated. Specifically, what are the elevation (z) values for the profiles corresponding to 1D, 2D and 3D downstream of the turbine in Fig. 14? Also, what spanwise locations (y) are being referred to in Fig. 15? These questions arise because the wake centerline deflects in both vertical and spanwise directions, and I suspect that the observations regarding collapse of profiles at different x locations onto a single curve might be sensitive to the planes selected for this analysis.

3. It would be interesting to check if the behavior of a turbine wake that deflects vertically (and laterally) is comparable to other free-shear flows that deflect in this manner. An example is a horizontal buoyant jet studied in Xu & Chen (2012), but there might be other studies as well, such as a jet in cross-flow. The asymmetry displayed in Fig. 15 (b) seems

similar to Fig. 15 in Xu & Chen (2012). Are there any systematic trends in the deviation from Gaussian profile in a turbine wake?

4. It would be interesting to check for self-similarity of other quantities such as RMS of fluctuations and other components of the Reynolds stress tensor (e.g. $\overline{u'w'}$). This is important because of some recent analytical models that rely on the self-similarity of the added TKE (e.g. Ishihara & Qian, 2018).

Minor Issues:

1. Section 2.1, Line 25: References missing here.
2. Fig. 1 indicates that the turbines were not at the center of the domain in the spanwise (y) direction. Why were the spanwise extents chosen in this manner?
3. Was the topography naturally periodic over the chosen extents in the x and y directions, or were some artificial adjustments to the topography introduced to ensure periodicity in x and y?
4. Section 3, first paragraph: 'H' is used without being defined.
5. Page 6, Line 8: "…chosen for this terrain to avoid Gibbs phenomenon." Can the authors explain this sentence in a little more detail? Is it that the terrain without any smoothing at all leads to Gibbs oscillations in the simulation?
6. Page 6, Line 14: Is there a reference to a systematic study where the 1:4 aspect ratio being suitable is demonstrated?
7. Section 3.1, Line 5: Can the authors add the resulting friction velocity values to Table 1? This way, the differences in the terrain-induced drag forces will become apparent.
8. Section 3.1, Line 10: Does the wall model need to be tweaked for correctly handling the recirculation regions on the lee-side of complex terrain? The authors mention in Section 5.3 that such recirculation regions exist in the current simulations.
9. Section 3.2: This is slightly confusing. Are the 30-minute averages from the same simulation or from different simulations? If, say, five 30-minute ensembles are used, how is the resulting average velocity different from a 150-minute average? How is each individual 30-minute average computed: i.e., averaged using fields at each time step, or every few time steps?
10. Fig. 5: By what amount do the u_h values vary? I assume they would be proportional to the respective u_* values, so it would help to have these tabulated along with the friction velocities as mentioned in point 12.
11. Please mention what grid sizes are being used for all cases in Fig. 5.
12. Section 5.1, last line: I can understand the horizontal heterogeneity introduced by the complex terrain being responsible for deviation from log-law, but I do not understand how the boundary conditions affect this. Could the authors clarify why periodicity could be responsible for deviations from log-law?
13. Page 11, last paragraph: Do the authors suggest here that the spanwise deflection of the wakes could be a numerical artifact? It should be straight-forward to determine if the value of the spanwise gradient of the terrain is significant at these locations.

14. Section 5.3.1, lines 1-3: I assume 'lateral profiles' are referring to Fig. 9, and that the steep case uses finer resolution and smooth case uses coarse resolution. What additional features are captured by finer resolution (steep case) in this figure that are not seen in the coarser resolution (smooth case) simulation?
15. Fig. 12: Please clarify why the velocity deficits are so different at x/D = 0. One can understand their evolution being different, but at the disk, these quantities should be very close to each other. Are these differences related to the reference velocity?
16. Fig. 14: The authors should make the inset as a separate subpanel (Fig. 14c) so as to show clearly that self-similarity does not seem to hold beyond 3D.
17. Fig. 15: The caption mentions an inset, but it is missing from the figure.
18. Minor stylistic issues:
    a. Please ensure that the references are in an appropriate order.
    b. Section 2, Line 1: Remove brackets from "formulated in (Sullivan et al. 2014)"
    c. If possible, the authors should use the same color scheme for the different cases in all figures.
    d. Fig. 8: It would be easier to read this if the order between (a) and (b) were to be maintained, i.e. 'Smooth' on the top and 'Steep' below.

References:

T. Ishihara, G. Qian (2018): "A new Gaussian-based analytical wake model for wind turbines considering ambient turbulence intensities and thrust coefficient effects", *Journal of Wind Engineering and Industrial Aerodynamics*, 177, 275 – 292.

D. Xu, J. Chen (2012): "Experimental study of stratified jet by simultaneous measurement of velocity and density fields", *Experiments in Fluids*, 53, 145 – 162.