Review for "Offshore wind farm global blockage measured with scanning lidar" by Jörge Schneemann et al.

Referee: James Bleeg

General comments

The paper reports on the results of an effort to use lidar PPI scans to discern the effect of global blockage on wind speeds upstream of a large offshore wind farm. It is a welcome addition to a small club: research articles focused on field observations related to wind-farm-scale blockage. This paper, to my knowledge, is the only one so far to show in any sort of detail how the effect of global blockage varies spatially. Of additional note, the paper demonstrates that the global blockage effect is much more pronounced when the boundary layer is stable (in fact, no effect was evident when the boundary layer was unstable). The paper is well-written and includes an illuminating treatment on the uncertainties of the analysis. I consider the paper to be a significant contribution to the understanding of wind farm flows and blockage effects in particular.

Having said that, I do have a number of questions and comments about the paper. For one, the potential impact of wakes from neighboring wind farms should be discussed. These wakes could potentially influence the observed horizontal variation in wind speed and perhaps even the vertical profiles, which would have implications for the height extrapolation of the measurements. I also wonder whether more could be done to investigate blockage effects during unstable conditions and, more generally, to better target the wind speed conditions where blockage effects are expected to be most influential.

I often struggle to determine how to numerically rate a paper or to distinguish between minor revisions and major revisions. The upshot here is that this is important research that deserves publication. However, I also believe the paper would benefit from addressing the comments below.

Specific comments

Potential impact of other wind farms on the results. The BorWin wind farm cluster is 24 km to the SW of the lidar devise. The Gemini wind farm is 54 km to the SSW. These are the primary scanning directions. According to "Cluster wakes impact on a far-distant offshore wind farm's power" by Schneeman et al, these wind farms are close enough to influence the lidar measurements during stable conditions and perhaps even during weakly unstable conditions.

The objective of the measurement campaign is to isolate the impact of global blockage on the wind speeds upstream of the wind farm. If the background wind speeds (i.e. the wind speeds absent the influence of the Global Tech 1 wind farm) were horizontally homogeneous on average, then we could more confidently quantify the impact of Global Tech 1 on the horizontal variation of wind speeds as equal to the observed average horizontal variation of wind speeds.

I think the paper needs to discuss this issue and whether we should expect the background wind speeds upstream of Global Tech 1 to be horizontally homogeneous on average. I suspect that on average that the background wind speed may have a positive streamwise gradient as the wakes from neighboring wind farms recover—at least in certain meteorological conditions. The measurements in Platis et al ("First in situ evidence of wakes in the far field behind offshore wind farms" in Nature) show a good illustration of this with strong streamwise gradients 15 km to 30 km downstream of an offshore wind farm (e.g. Figure 5a). Such a situation could potentially hide/offset blockage effects that might otherwise be observable or reduce the strength of effects that are observed.

Ideally, these questions could be answered using on-site observations; however, the lidars do not appear to reach far enough upstream or far enough back in time to assess the background flow. Nevertheless, the authors may have some data or insights that can shed light on this matter, which again, I think needs to be discussed in the paper.

Uncertainty in profiles used to extrapolate to platform height. The uncertainty section is very good, allowing the reader to understand exactly what is accounted for and how in the uncertainty estimates. The applicability of the log profile is listed as one of the items not accounted for—and I'm not sure how you could quantify it absent more detailed measurements at the site. But it may deserve additional comment. Might, for example, the wakes from BorWin or even Gemini cause the actual profiles to deviate from theory?

The findings and discussion around the unstable flow results: For unstable conditions, there was no evidence of global blockage in the presented measurements or in two additional sets of lidar measurements that the authors analyzed, but did not show in the paper. The paper concludes that the observable blockage effect is much stronger when the boundary layer is stable as compared when it is unstable. This is an important finding.

There are two related points, however, that at present I hesitate to fully embrace and perhaps more could be done to improve and/or substantiate them. The first one relates to the physical explanation for why blockage effects are so much more evident when the boundary layer is stable.

The paragraph starting on line 386 covers a lot of ground and is a good starting point for the matters I would like to discuss:

"In unstable conditions with wind speeds from 10 m s−1 to 13m s−1 and a moderate to high thrust coefficient (Scenario 1, Figure 3) we could not identify decreasing wind speeds in front of the wind farm and thus no global blockage effect. This result is plausible since wind speed fluctuations in unstable flows are much higher due to convection than the assumed magnitude of global blockage. Convection leads to more mixing in the boundary layer and thus repeals global blockage. Furthermore, 390 in unstable stratification, the boundary layer is typically higher and thus the flow can pass obstacles like hills (Stull, 1988) or in this case a wind farm more easily. Additionally to Scenario 1 we performed the analysis for unstable stratification and the wind speed ranges above rated wind speed and below cut-in wind speed respectively (c.f. Section 2.4). In both cases, we could not identify decreasing wind speeds in the inflow of the wind farm. As explained earlier we do not show these results here for brevity."

The wind speed fluctuations are indeed much higher in the unstable boundary layer, and for any given snapshot of the flow, these turbulent fluctuations will probably drown out the global blockage signal. However, on average, the adverse pressure gradient associated with Global Blockage will operate on the flow over large distances and from the ground to the top tip of the rotor and beyond, and I would expect that with enough measurements, turbulence would not, on average, completely hide the effect of this adverse pressure gradient. That said, in highly convective conditions, I could see how such a thing might occur. I'm just not sure if it occurs often. Do you have any analysis to settle this point?

The explanation regarding a higher boundary layer seems reasonable. I would add that inviscid effects related to stable stratification *within* the boundary layer also likely contributes to the pronounced wind speed decreases upstream of the wind farm during such conditions.

Although it makes sense to me that the observed global blockage effect would be more pronounced when the boundary layer is stable (it's also a finding consistent with our CFD analyses), I confess that I did not expect that blockage-related wind speed decreases would be absent from the observations during unstable conditions.

If I were to look such wind speed decreases, I might focus on a different set of conditions than those described in the above quoted paragraph. One would expect the blockage effects to be at a maximum between 4 m/s and 10 m/s, which is the only range of wind speeds not investigated when filtering for unstable conditions. Is there any particular reason for this, especially in light of the fact that the main wind speed filter for the stable cases was 7 m/s to 10 m/s? The coefficient of thrust for this turbine model drops off quite a bit between 10 m/s and 13 m/s. Moreover, the filter is based on the wind speed measured at the transition piece. The hub height wind speeds will be even higher, and thus the thrust coefficients will be even lower—and the blockage effect lower in turn.

Filtered conditions: More generally, have you considered widening and adjusting the filter range to better target the plateau of the Ct curve for both unstable and stable conditions? One approach might be to filter based on turbine power. More data points concentrated at the max Ct may reveal a stronger blockage-related signal and do so with less uncertainty (i.e. increase N_r which seems low, and reduce SEM).

<u>Minor specific comments</u>

- Line 140: Is there any reason why you did not included data taken earlier than February 2019?
- Line 198: How do you know the mean wind direction for the scan?
- The uncertainty section is very good. I was also impressed with Figure 7 and the associated discussion as a way to address additional uncertainties.
- Line 420: "We do not know whether the global blockage effect is distributed equally over height but expect it to be most distinct in the rotor area especially at hub height." That is probably true within the induction zone of an individual turbine, but beyond a few rotor diameters of the wind farm, I do not expect this to be the case. I expect that the streamwise (adverse) pressure gradient associated with global blockage varies little between hub height and the sea surface. The wind speeds will generally be lower closer to the sea surface, and thus the percent impact of the adverse pressure gradient on wind speed should generally be larger. The difference should be small though—at least according to the one CFD simulation I just checked.
- The upstream influence of the wind farm will probably extend beyond 40 D. Whether the influence is material this far out (e.g. >0.5% in wind speed), we do not know. It could be material, and as such, the point should probably be mentioned in the paper. At the very least, the wind speed decreases for scenario 4 should be described as relative to the farthest upstream measurement—to avoid any mistaken assumptions that the decreases are relative to freestream.
- The explanation you provide for the lower wind speeds at the sides of the scan in Figure 6a is credible (see line 445-452). The wind farm is likely forcing the wind to diverge laterally around the wind farm, particularly when the boundary layer is stable. This could potentially cause the wind direction to deviate materially at the sides of the scan relative to the assumed wind direction, perhaps contributing to the trend seen in the figure. Most of us do not have a good feel for how much this could affect the results. If the wind direction is biased just 1 degree, for example, relative to expectations at the side of the scan, what

would be the impact on wind speed? It might be worth a sentence or two to quantify the effect for the reader.

Technical corrections (that may not require corrections)

- Line 22: The paper focused more on bias than uncertainty, though the sentence as written is not incorrect.
- Line 30: I struggled a bit with the sentence starting "The local wind speed…" The "standard onshore setup" probably refers to a power performance measurement, but you may wish to be more explicit about it. Putting the whole sentence together, I think you are saying that the wind speed measured at 2.5 D upstream is considered to be a freestream wind speed, practically beyond the influence of wind turbine blockage. You may wish to rephrase to make this sentence easier to read. Incidentally, I don't agree that 2.5 D upstream is beyond the influence of the test turbine, but I do agree that it effectively "considered" that way in the referenced IEC standard.
- Line 49: The authors write that Allaerts and Meyers, "relate the flow deceleration in front of the wind farm to pressure gradients induced by the gravity waves and not directly to global blockage." This could be a matter of taste, but I might write this differently. As Allaerts and Meyers put it in the paper, "wind-farm induced gravity waves are triggered by the upward displacement of the boundary-layer top due to flow blockage inside the farm." Inviscid effects related to stratification (i.e. gravity waves) modifies the blockage effect that would otherwise be seen in a purely neutral flow. One could therefore view these waves as an effect of blockage. Up to you on whether to make a change. I just wanted to provide another point of view.
- Line 90: Instead of "single points", you may with to consider something like "a small number of scattered points." The number of points we had at each wind farm ranged from 3 to 6. Of course, your main argument still stands.
- Line 417: Some of the measured heights in the 2018 Bleeg paper were at hub height (H), but others were at 0.7 H.