Paper title: "Investigating the physical mechanism that modify wind plant blockage in stable boundary layers"

The authors simulated an idealized wind farm on flat terrain operating in two different stable boundary layers. The simulations were run back-to-back with a turbine operating in isolation. The results of these simulations were then analyzed to quantify the impact of blockage on the flow upstream of the wind farm and also on the upstream row of turbines. Further, the authors interrogated the solutions to understand the physical mechanisms behind these blockage effects.

If accepted for publication, I think this paper would be a strong addition to the growing literature on wind farm blockage effects. The analysis of the simulated flow is lucid and compelling. The main findings, in my view, are important and mostly new.

The main area where I feel the paper could be improved is putting the research into context. The next section of this review focuses on this aspect. I include a number of miscellaneous comments after the context section.

Putting the findings into context:

Gravity waves – Gravity waves are referred to a number of times in the manuscript, mostly in the vein of we-are-not-dealing-with-gravity-waves-in-this-study. The introduction puts this explicitly: "Here, we investigate how atmospheric stability modifies upstream blockage in the absence of gravity waves." There is even a section in the appendix substantiating the claim that there are no gravity waves in the flow solutions. I do not think this approach to setting the no-gravity-wave stage is necessary and may even be counterproductive, potentially leading readers to believe that the findings are limited to gravity-wave free conditions.

There are good reasons to believe wind-farm-induced gravity waves are very common. The manuscript implies the findings in the two Bleeg papers are not affected by gravity waves (see line 50), but this is not the case. All our wind farm simulations have gravity waves, at least to some degree. Gravity waves, as you know, are the primary means by which disturbances are transmitted in density-stratified flow. Such a disturbance might be an obstacle like a wind farm causing flow to rise. The resulting gravity waves—or speaking more generally, inviscid effects related to disturbed stratified flow—and their effect on the wind farm will depend upon the characteristics of the stratification and the wind farm itself. The authors appear to be simulating a set of conditions where the effects of gravity waves on flow upstream of the wind farm is not as pronounced as in the first Allaerts and Meyers JFM paper. I think it is sufficient to say just that.

There is strong numerical evidence at this point that inviscid effects related to stratification above the boundary layer—and the horizontal pressure gradients associated with them—can have a significant impact on blockage effects. This paper helps bring to light another important influence on the production of leading row turbines relative to what they would produce in isolation: the vertical advection of u-momentum. The finding not only improves our physical understanding of blockage effects, but also has significant implications for the modelling of these effects.

Engineering models for blockage – The engineering models designed to predict blockage effects are inviscid, potential flow-type models, which do not account for shear and therefore are not able to reliably account for what this paper suggests is one of the most important contributors to front row blockage loss: the vertical advection of u-momentum. It is up to the authors, but I think it might be worth emphasizing this in the paper.

The manuscript states that blockage is not currently accounted for in EYA's. This was true in 2018, but it is not true anymore. Almost all EYA's account for blockage in one way or another. And in many cases, a potential flow model is used to account for the blockage effect. Potential flow models, as discussed, will miss a significant contributor to the impact of blockage on turbine production. I feel that the wind industry community would benefit from having this point highlighted.

Has anyone looked into this before? According to the manuscript, the dominant mechanism causing front row wind turbines to produce less than they would in isolation is the vertical advection of u-momentum (at least in these simulations). To me, this is the most important finding in the paper. The Discussion and conclusion sections highlights others who have focused on adverse pressure gradients as a key driver behind blockage, and then explains that vertical advection of u-momentum amplifies the impact on the front row turbines. I'm a bit biased here, but it is worth mentioning that Bleeg and Montavon included a full section on this subject? The section makes the point that the combination of shear and flow rising as it approaches the wind farm, due to the presence of the wind farm and the ground, "appears to be an important factor in determine the magnitude of the blockage loss." Immediately following, the paper reads, "in case 9, for example, the streamline passing through the hub in the wind farm configuration originates approximately 4.5 m below the hub-intersecting streamline in the isolated case. In turn, the wind speed on the streamline far upstream is approximately 2% lower in the wind farm configuration compared to isolated operation. This significant wind speed difference is the result of the vertical flow deflection combining with the increased shear that prevails in stable conditions."

In my opinion, the physical explanation provided in the manuscript under review is more clear, complete, and convincing that what we provide in Bleeg and Montavon. Much credit is due to the authors for this important finding. That said, I think it is fair to say that the Bleeg and Montavon paper did highlight the important influence of shear in combination with the upstream vertical deflection of flow as it relates to the impact of blockage on the production of leading row turbines relative to a turbine in isolation.

I am not an academic and am not familiar with what is required in a situation like this (also, I suppose I am not without bias in this regard), so I leave it to the authors and the editor to decide whether this should be acknowledged in the paper. For what it is worth, I think referencing the earlier result could further strengthen the credibility of the current finding. A sentence or two would be sufficient. And if it is followed by something indicating that your analysis more complete, I would not object, because it is.

Miscellaneous:

The following questions, comments, and suggestions are in roughly order of priority

A. I can't quite tell where the overall control volume in Figure 13 ends in the x-direction. The caption says it is bounded at the first turbine row (x = 5670 m). Clear enough, but where is that location in Figure 12? Is it at 0 D or just upstream? The reason why I ask is that that thrust body forces from the GAD are being applied to cells that include locations just upstream of 0 D, resulting in a rapid drop in pressure. If the end of the control volume in Figure 13 does correspond to 0 D, how would the results change if the end of the control volume were moved to just in front of the GAD?
B. Again, I found the analysis of the results in sections 3 and 4 to be clear and convincing. That said I wonder if you could go just a little further to provide more physical insight and help the reader connect the dots towards your key finding. I refer specifically to the significant influence of the vertical advection of u-momentum. Perhaps you could break this down a bit.

It could help drive home the point of the importance of shear. The x-component of velocity is clearly higher at the top of the control volume than the bottom. I'm not sure how the vertical component of velocity varies streamwise upstream of the wind farm, but I suspect it is positive and generally increases as flow approaches the wind farm. Of course, the trend may differ between the top and bottom of the control volume (in fact, the vertical component of velocity may be negative close to the rotors). I think it would be nice to have these things related to the vertical advection of u-momentum broken down, though I concede that what you already have in the report is sufficient to make your point.

C. This is a big statement in the paper: "Given that the normalized pressure gradient force remains unchanged with atmoshperic stability and turbine array size, differences in blockage are caused by momentum redistribution in the induction region." Firstly, you'll want to correct the misspelling of atmospheric. Secondly, I just want to put this into context. In practice, when evaluating the impact of blockage or wakes on a wind farm, what we care about is the power production of the wind farm turbines relative to what they each would produce in isolation. In other words, we care about the wind conditions that each turbine experiences relative to the conditions it would experience in isolation. My interpretation of your analysis is that, at least for the simulated conditions, the vertical advection of u-momentum is *the dominant factor* affecting the production of leading row turbines relative to what they would produce in isolation. It is by far the main physical mechanism behind blockage loss for these turbines—again, for the simulated conditions. Am I interpreting your work correctly?

D. I'm not sure your simulation setup can reliably capture gravity waves. With our own steady-state RANS model, a domain height of 3500 m (too low) and a damping layer thickness of 1000 m (too thin) would yield significantly different results with respect to gravity waves than our standard domain (top boundary at 17,000 m and much thick damping layer). That said, if you were to re-run your analysis with a much larger domain, I doubt your main findings would be much different. Thus, in my view, it is not required to run this sensitivity check, though it would be a nice-to-have. If future studies focus on gravity waves—and more broadly the influence of the stably stratified atmosphere above the boundary layer—such a sensitivity study would be needed.

E. As suggested above, I would consider just dropping Appendix C. However, if you keep it, could you please clarify the height at which the values in Figure C1 are being plotted?

F. If you pursue this research further, it would be interesting to know what you find when simulating a neutral boundary layer and/or an unstable boundary layer