Fast blockage models for wind-farm power prediction

This well written and simple article presents two fast reduced-order models to predict wind farm power including the impacts of hydrostatic blockage effect. The derivation of both models is presented and the models are novel. This article can be published after the following questions and concerns are addressed and changes are made accordingly.

1. The authors aim at improving the speed of reduced-order models. However, it was not established why it is desirable to have faster models? How fast is fast? Also, is this the only way to build a faster model or is it that the authors are improving their previous models? I think the authors should motivate the article in a clear way; I think they are improving their existing models and that should reflect in the title and the introduction. Otherwise, convince the reader that this is the only approach to producing faster models.
2. Line 89-91: I understand that limiting the height from reaching into the free atmosphere is a valid reason, but the justification given for using a lower height is rather vague. If using a shorter domain is closer to turbine-scale flow, why not consider the wind farm layer (D)? Later in this paragraph, the authors talk about including the actuator disks in the lateral direction. This contradicts with the justification given earlier. It is desirable to elaborate a generic approach to choose the height of the control volume.
3. Line 92-93: Kirby's observation sounds better representative of the wind farm aerodynamics in shallow boundary layers as recent LES studies have shown how wind farm yield is affected by low boundary layers. I think, the authors should calculate C*p based on the CNBL LES Uf and they might see variations. Basing C*p on a truly neutral simulation inherently excludes the stratification effects that are captured by the CNBL LES cases.
4. Equation 9: It is interesting that the contribution to p' of the interfacial and internal waves is disentangled. Is it possible or is this an assumption? I would expect that the total displacement of the boundary layer is a coupled effect of the wind farm perturbation and all kinds of gravity waves.
5. Line 172: Linked to first comment, is this the only way to build a fast model using 2SM? How about wind farm canopy models in a CFD setup that could exploit the essence of the 2SM to project a reasonably correct wind farm drag to the flow in an iterative manner? The model can run on a mesoscale grid and exclude the assumptions (linearization and gravity wave feedback) made by APM. When the authors talk about "fast" they should justify how is APM their chosen fastest yet reasonable accurate approach? Or is it that the authors want to exploit the availability of an APM model?
6. Equation 16: I am confused about this assumption and how it holds when the tuning parameter is zero. No perturbation above the 2SM essentially means the zero-mean displacement of the wind farm layer in the APM. Then equation 16 becomes: H1*u'1 = H1*u'f = H1*(Uf - U1). Thus, H1 = Hf. So where is the difference in Uf and U1 coming from if

there is no perturbation/displacement? This assumption is the core of this derivation, and it is unclear to me how it holds. This requires further discussion and clarity.

7. Line 212: Is tuning of α generic or specific to the CNBL LES cases in the dataset? Note that CNBL itself isn't fully representative of all atmospheric conditions and the idea of using a fast model should not rely on availability of higher fidelity solutions. Also, tracing a particle through a turbulent field resolved in LES to find the height of the plaint surface seems overly simple and a straightforward assumption that this depicts the authors' wishful interpretation of a distinct wind farm layer and an upper layer. This requires further explanation and justification to support the method used to acquire APM states.
8. Line 288-290: Presumably the wake model will give C*p and C*T for a neutral or at best a specific ABL stability case. Then using the 2SM-APM for arbitrary boundary conditions means that the wind farm thrust and power is independent of the atmospheric state to simulate. Since the flow conditions (i.e. U_inflow) depend on atmospheric state, the predicted power will inherit the state simulated by the wake model. Thus, a workflow coupling a wake model with the 2SM-APM, for consistent atmospheric conditions across the models, is desirable. Can the authors comment on this or propose a better strategy? There is a typo too: "These can then be used by".
9. Section 5: At this point, the formulation is a bit hard to follow as the equations are repeatedly related to expressions in previous sections. I suggest the authors should give a flow chart with steps clearly showing the relations among the main assumptions made. This will also help the reader follow the structure of this paper. I also suggest the authors include the formulations of operators in the appendix as the derivations deeply rely on them.
10. Equations 27-29: On what basis was the contribution of velocity deficit dropped here? I think a small perturbation or displacement would cause little blockage. More importantly, how is perturbation related to velocity deficit? Please establish this assumption, preferably with some expressions. I also don't follow that the wake component is included back in 28 and later estimated in 29.
11. Line 351: Clearly, averaging over velocity is dropped because it's a slower operation. Yet the operator suggested depends on the turbine force given by the wake model, as mentioned on line 394-395. Can the authors establish if this estimation is more accurate and there isn't a trade-off between speed and accuracy?

Minor Issues:

1. Statements on Line 37-39 are not entirely correct. The AFMs resolve the turbine scales on a coarser grid than required by ADMs. More importantly, these are CFD models which inherently couple the resolved scales. Thus, there is no separation of scales, consider rephrasing these lines and maybe there is no need to classify them as 2SM approaches; drawing the analogy is fine.
2. Line 75: I don't see why C*p cannot be calculated for stable or unstable flow simulations. Perhaps, the authors are confined by the CNBL dataset they have for validation. This doesn't mean the same approach is not applicable to SBLs and CBLs.

3. Line 86: “farm averaged velocity”, I assume?
4. Figure 1: I don’t understand why the authors did not use CNBL data to calculate C*p?
5. Line 96: Which other cases? You didn't refer to a specific case or cases in the previous sentences of this paragraph.
6. Line 111: Maybe it is useful to mention a few tools or a class of tools. I assume the authors mean to compare APM with engineering tools and not CFD tools.
7. Line 153: For completeness, include the expression for vertical wavenumber.
8. Line 165: I guess APM assumes, through equ 9, a steady-state impact of all types of atmospheric gravity waves and not only internal waves.
9. Line 279: Please also specify computational load in quantifiable units instead of saying standard laptop, e.g. core hours or flops etc.
10. Table 2: Some of the short forms are not defined at first use.
11. Line 303: The authors aim to demonstrate that wake models can provide Uf and the remaining routine follows. Since wake models are bottom-up approaches and not ideal for wind farm interactions, I am wondering if there is an opportunity to establish a method to estimate Uf for an arbitrary wind farm? If so, why use wake models to estimate Uf?
12. Line 345: “we can now assume”?
13. Line 411: How do these assumptions affect the solutions produced by the solver?