

Review of “Fast blockage models for wind-farm power prediction”

General comments

The paper describes how the recently proposed two-scale momentum (2SM) theory can be coupled to a atmospheric perturbation model (APM) developed by the authors. The approach is novel and the paper well written, mathematically sound and mostly easy to follow, however the structure, validation and “fast” element of the paper need improvement.

The only part that seems to be “fast” is hidden in subsection 4.3.1, so either the title needs to be adapted, as it is currently misleading, or more work needs to be spend on timing the three different flavours of the coupling that are presented and show how the currently not so “fast” approaches can be sped up. Whilst going through section 4 and 5 the motivation of using one approach over the other remains somewhat obscure and currently just seems like a list of possible methods.

While the paper is mathematically rigorous, some decisions and sensitivities need to be explored further, especially how to choose the size of the control volume in the 2SM approach such that the error in determining the average wind farm wind speed, U_f , does not depend on the wind farm layout. The authors currently are able to avoid addressing this issue, as they use idealised square wind farms, the same idealised conditions also the 2SM theory is based on. At least one non-rectangular wind farm (or at least run one different wind direction) test case need to be added to showcase that the proposed approach has more general applicability and provides similar results as for an idealised setup. Additionally, one case above rated wind speed could be added, such that the farm has some CT, CP variation (potentially already the case but not explicitly discussed in the paper).

Specific comments

Title

The title is a little too general with regard to the content as there are fast blockage models out there, however not when gravity-waves are considered.

Abstract

The abstract should more clearly define to what problem the model should be applied to. I guess the aim is to provide accurate estimates of wind farm power for large wind farms when considering gravity wave effects at low computational costs? Please clarify.

One might consider providing a reference to “two-scale momentum theory” as it is not that established yet.

Introduction

First paragraph: Whilst the numerical proof that large farms can trigger gravity waves is compelling and referenced here, this section of the text does not reflect that all existing studies related to gravity waves remain numerical - physical or operational validation of their significance remains missing. Please at least include a statement that these are numerical studies (Bleeg’s paper is different but also does not discuss gravity waves or atmospheric interactions).

Second paragraph: It is not just the cost, but also the complexity of the inputs.

Third paragraph: One might want to mention what type of atmospheric dynamics it captures.

Section 2

L44-51: The LES dataset is not only used for validation but also used in deriving model constants. This should be mentioned.

L73: "...assumed to only depend on the internal problem." Is this an issue when coupling it to the APM, as this might change the turbine set-points?

L74: It did work for their test cases, which are idealised square wind farms staggered and aligned configurations with a single turbine type and below rated operation (ie CT and CP not changing much inside farm). This limitation should be highlighted as it might not generally hold.

Paragraph from L78: Please mention the operating point of the turbines that was used. I suppose below-rated or close to that? If so this does align with the assumption mentioned above. This should be made explicit.

Paragraph from L86: "... we believe ..." is not sufficient justification. Kirby showed for their test cases that 2.5zh worked, you found it is 2zh, but mostly because otherwise you were reaching into the upper atmosphere. So is this some implicit tuning parameter? We just need to find some height and averaging volume that ensures that it matches what we would like to see? Please add a sensitivity study with respect to Hf and the choice of averaging volume size, as otherwise this seems somewhat arbitrary. Please also add at least one test case for a non-square wind farm and describe how the volume should be calculated in that case.

Paragraph from L95: Could you elaborate on the scaling? Could the good match between 2SM and LES not also stem from performing this scaling?

Section 3

Equation 3: I am missing a definition of x,y,z

L119: Is this the same filter size as in Equation 11 and is it fixed?

L160-: Does this mean that this tight coupling at individual turbine level is the the issue for high computation costs? It could be nice if such a statement could be build in here and maybe already be included in the introduction to make it very clear. The 2SM model does not say that the forcing does not vary within the farm (at least the impression I got from a statement in the introduction), but only that it can all be related to some average farm wind speed, which is actually very similar to what the actuator wind farm models have shown, and that is what you are leveraging here, assuming that pressure perturbations will not change forces significantly or the turbine operating points.

Section 4

L186: I disagree that this is "straightforward" at all and that's due to Ω_f . How do we define a wind farm planform area in a realistic setup? In you test cases and those of Kirby this might be "straightforward", but in any practical application it is not. There should be a discussion around how to determine Ω_f for real farms in a consistent manner, otherwise U_f could be biased depending on farm configuration. Actuator wind farm models also struggle defining U_f consistently, but some practical solutions exist and should be discussed.

Equation 14: Wind farm forcing is being applied with Gaussians, but forces are applied outside of the wind farm planform if it is defined as the outer turbine perimeter, however Ω_f is not defined so it remains unclear. The 2SM probably requires all forces to remain inside the CV.

215: To have a tuning parameter that provides good results is nice, but it would be good to show how it varies between the different cases. Will it change if we change CV and wind farm size, this sensitivity should be explored.

Equation 20: Should this not be f_{wf} or am I missing something here?

L235: Could make it a little more clear what you mean by “APM state”, as lot seems to be possible once we assume it is not changing.

L247: This is a little unclear. So you are running an engineering wind farm simulation, excluding atmospheric effects etc to get the footprint? Also can it really be chosen “freely”? Is it not somewhat related to the CV used to compute U_f ?

L264: This is not really a validation, it is only a verification, unless I misunderstood your approach. You have derived/trained your model constants using the LM LES cases and now you are running your model and comparing to them. Validation would be if you had another set of data that you could compare to that you did not use in training. Please modify and make this clear unless you have some other test cases using that data you did not use in deriving your model.

L275-: What are the error metrics based on (total wind farm power)? That should be mentioned. Generally errors should be discussed in more detail, as there are some cases with large errors and also a general bias towards overestimating production. Also why are you not also comparing to your fully coupled APM model, maybe that would be a fairer comparison? You mention that errors are similar in the next paragraph, but maybe they are biased differently? There is no way to discern that here.

Table 2: Explain what all the model cases are. This is not mentioned anywhere. In the text one could also go into a little more detail (best case, worst case scenario ...)

L287: It should be highlighted that this is closer to the real use case, as if we cannot run a LES simulation first to get C_{pstar} , C_{tstar} etc if we want a fast model.

L293: Why are you mirroring the wake effects using the original Gaussian model? The expansion parameters of that wake model were derived from simulations of a turbine in ground effect, so implicitly it already accounts for that.

L300-: This is a major issue. Without it we do not get a “fast” model after all so one might argue that this type of 2SM-APM coupling is actually not really fast nor really good. Could you comment on that and its relevance to practical applications?

Section 5

L323: Please check again whether it is actually validation and not verification.

L341: It might be good to mention the “filtered, height-averaged” part already before in connection with U1 in L335 as all the different operators can be a little confusing (or forgotten by this stage).

L346: Typo “new” ?

L411: Typo “... simplify this solver, ...”?

L435-: Maybe this will be discussed afterwards, but clearly this approach is also dropping a lot of the simplicity of the initial coupling and computational speed gains? Especially the latter needs to be discussed in relation to the promises made in the title.

L443: Is it important where the inlet is placed?

L445: The error is actually quite larger at times, one could argue that there is not much benefit in running with a correction for many of these cases. Could you provide some insights as to why?

L457: Use "It is" instead of "It's"

Figure 8: It clearly captures the trend but there is an offset.

Conclusion

L457: Remove "interesting" as it is a very subjective statement.

L458: What are the core hypothesis?

L468-469: But how does it perform when CT and CP are unknown. Was this on purpose not mentioned as it did not perform as "well"? Well is also very subjective, try to quantify your claims and also state the max error. The same goes for the next paragraph.

Last paragraph: Some nice points here, but what about computational cost of the different model variants? Also if adoption in industry is wanted than also model simplicity is key and the second variant seem quite a bit more involved to implement. Future work should for sure also try to validate using datasets that are not using square wind farms and conditions different from those used here.