Wes 2020-86
Overall

Overall, you have the makings of two good papers here and by shoving it into one paper, you are short-changing some of the depth in analysis on the different components. I would either expand in several areas or potentially cut some content and think about including it in a follow-on paper. For example, the complex terrain - since this is on wake steering, how much value does this add? Couldn't you do a whole additional paper looking at complex terrain and perhaps some application in either design or control? Sections 4.1 and 4.3 seem much more relevant since the main application targeted in this paper is on wake steering

The model proposed is proposed as an alternative to 1) lower fidelity models (linear engineering models, linear rans (e.g. FUGA)), and 2) higher fidelity models (full steady RANS, etc) and is trying to hit a sweet-spot in terms of the capturing relevant physics at an acceptable computational cost for control and optimization applications. This is clear and articulated at some points in the paper but could be pulled out even more strongly and with more thorough comparison to the state of the art on the former in comparison to both alternatives.
See more detailed comments by section

We thank the editor for their positive feedback and suggestions. We have revised the abstract to better describe the work in the paper. We want to keep the article and its content in the same publication. This article shows the mathematical method in detail with rigorous derivations that are new and have not been done before. We also believe that the test cases are an excellent way of showing the extent of the model capabilities. We would like to pursue future work in complex terrain and either design or control, however, this is out of the scope of the current work within our organization. This paper has also undergone an extensive review process with 2 revisions as part of the WES journal review and 3 revisions within our communications department. The new version of the manuscript meets all our internal requirements, and the external reviewer feedback has been addressed in detail.

The abstract of the text has been rewritten. The new abstract is:

"Wind turbine wake models typically require approximations, such as wake superposition and deflection models, to accurately describe wake physics. However, capturing the phenomena of interest, such as the curled wake and interaction of multiple wakes, in wind power plant flows comes with an increased computational cost. To address this, we propose a new hybrid method that uses analytical solutions with an approximate form of the Reynolds-averaged Navier-Stokes equations to solve the time-averaged flow over a wind plant. We compare results from the solver to supervisory control and data acquisition data from the Lillgrund wind plant obtaining wake model predictions which are generally within one standard deviation of the mean power data. We perform simulations of flow over the Columbia River Gorge to demonstrate the capabilities of the model in complex terrain. We also apply the solver to a case with wake steering, which agreed well with large-eddy simulations. This new solver reduces the time–and therefore the related cost–it takes to simulate a steady-state wind plant flow (on the order of seconds using one core). Because the model is computationally efficient, it can also be used for different applications including wake steering for wind power plants and layout optimization.”

Abstract

some of the results reporting is pretty vague. What does good agreement mean?

In most cases, we defined good agreement for power predictions as being within one standard deviation from the SCADA or high-fidelity simulations. We have eliminated some statements in the paper where “good agreement” was vague.

When you say minimizes, that is an exaggeration. There are even simpler models that can do such simulations in fractions of a section

To our knowledge, there is no other model that can solve a similar set of equations with a reduced computational cost. We have shown in section 3.1 that the problem of solving a simplified form of the RANS equations is reduced to a computational cost of order N, where N is the number of grid points in the domain. We acknowledge, that there is always room for improvement and to reduce the computational cost. We have replaced the word ‘minimize’ with ‘reduce’:

“This new solver reduces the time–and therefore the related cost–it takes to simulate a steady-state wind plant flow (on the order of seconds using one core).”

Saying "about a second on a personal laptop" is vague

We cannot use an exact number to describe computational time. “Order of seconds” is a good reference to use. The exact time varies depending on the computer, processor architecture, clock speed, software version, etc. To clarify the statement, we have rephrased it:

“on the order of seconds using one core”

Generally, was the minimization of time an explicit goal in the sense that you optimized aspects of the model parameterization for time minimization, or is it that you were seeking to implement a cost-efficient model that adequately accounts for wake deflection under steering. It seems like the latter is likely the goal and no explicit optimization of the modelling approach is done. It would be better to be explicit that the model is particularly advantageous for addressing specific flow phenomena (such as steering) that are a challenge for conventional engineering flow models...

The model was developed with the idea of saving computational cost. Many of the derivations and approximations were done with the explicit goal of reducing the computational cost. We originally developed the model to address wake steering. However, after different tests as shown in the paper, we have learned that the model can also be used for different physics phenomena, including shear, veer, complex terrain, etc.

Introduction

Okay, I just read the first sentence of the introduction and got more out of that about what the paper is about than the entire abstract. I recommend rewriting the abstract

Yes, we have re-written the abstract following the recommendations.

Recommend modifying sentence line 18 - qualify minimizing computational cost. You are minimizing cost while doing what? Preserving physics?

Yes, the model intends to preserve physics by solving the hybrid analytical-RANS set of equations. We have added the following statement to clarify that:

“This solver uses a hybrid RANS-analytical framework that aims to minimize computational cost while still preserving physics from the RANS equations.”

I would like to see a more thorough critique of superposition and where such methods are challenged. It is stated in the beginning that there are differences based on methods but differences aren't necessarily bad, it could be that the differences mean that one model is much better than another... how do they stack up in validation and where in particular are they challenged? Are they challenged more in wake steering compared to normal operating conditions?

That is a good point, and we have looked into wake superposition models in other work. We have learned that wake superposition models can have a big impact on the simulated power of a wind plant with relative errors that go up to 100% per turbine. Errors are typically higher when the number of wakes being superposed is higher, such as in the fully developed region of a wind plant. We have included a new reference in the discussion:

“Hamilton, N., Bay, C. J., Fleming, P., King, J., and Martínez-Tossas, L. A.: Comparison of modular analytical wake models to the Lillgrund wind plant, Journal of Renewable and Sustainable Energy, 12, 053 311, https://doi.org/10.1063/5.0018695, 2020.”

Overall point of paper "The curled wake solver presented in this work focuses on minimizing computational cost and capturing wake steering effects." Should be introduction. Still here, I would not use the term minimize unless you are actively tweaking parameters in a scheme to explicitly minimize the cost while preserving some explicit level of accuracy in wake effects (i.e. the deficit and profile of the wake matches on some statistical factors with agreement of %)

We have replaced the word ‘minimize’ with ‘reduce’.

Formulation

Can you be more explicit in what makes the curl floris standard so much slower? Also, how does it compare to engineering models like floris gaussian or others? Can you somehow quantify the performance of these different models in terms of a twodimensional perspective on computational cost versus accuracy? The discussion at the end of section 3 seems somewhat incomplete and this is such an important contribution of the overall paper-I would like to see more attention paid to it. I think you are jumping to quickly to the case studies which are arguably less interesting in terms of the core contribution

The reason for the significant speedup has to do with the current implementation of the model. In the standard FLORIS implementation, we need to compute every wake individually in the domain. That means that the equation is solved as many times as there are turbine in the domain. After the calculations of individual wakes, they need to be superposed, which can also be computationally expensive. We have included the following text in the manuscript:

“Significant speedup is expected in the presently proposed curled wake model formulation compared to the standard FLORIS implementation. The standard FLORIS implementation solves Equation 16 for every turbine in the domain individually and then superposes the solutions. This superposition approach results in an increased computational cost, especially when more turbines are included, as well as wake superposition uncertainty.

Results

· Again, section 4.1 discussion seems somewhat incomplete. There is a lot of interesting stuff in the results and the plots are great, but there is so little discussion of the results and particularly the results across the different directions - which vary substantially.

We agree that this work has opened the door for more comparisons and future work. We have opted for a concise discussion of the results in this section. The goal of this paper was to present the derivations in detail and demonstrate a few cases where the model can be used. Future work should focus on more in-depth sensitivity studies and other comparisons.

- Okay, you are now killing me a little bit... there is so little analysis and discussion relative to the scope of the work being presented in 4.2!

This section is meant to be a test case to demonstrate the capability of the solver in dealing with complex terrain. We are interested in pursuing research in this area, but unfortunately, at the moment, this work is out of the scope of current projects within our organization.

Conclusions
The conclusion is a better summary of the work than the present abstract
Recommend considering updates to the conclusion following recommended updates in formulation in results section
Consider discussing a bit more in depth the limitations of the current model and avenues for further work (as its own paragraph)

We have revised the abstract following the recommendations to better summarize the work in the article. We have also updated the conclusions to include the limitations of the model. A new paragraph has been added to the conclusion:

“Some of the limitations from the different approximations of the model include: a turbulence model mixing length that only depends on the vertical coordinate, a linearized solution of the vortices from curl that do not decay, a near wake formulation is missing, and there is no pressure term in the equations. These approximations were done in order to reduce the computational cost. Future work will focus on comparing the model with RANS, improving the turbulence model without compromising computational cost, improving the near wake, implementing a vortex decay model, using the solver for yaw-angle optimizations in a wind plant, and improving code performance to increase speed.”