Review : RANS modelling of a single wind turbine wake in the unstable surface layer

The present paper deals with the modelling of wind turbine wakes in the unstable atmospheric boundary layer using a RANS approach. Two models are proposed: the first on aims at accounting the buoyant production of TKE without relying on a temperature equation. The second model improves the so-called k-epsilon-fp RANS turbulence model, based on observed discrepancies and inconsistencies against experimental data and higher-fidelity simulations, i.e. LES. The model is globally clear and very well written.

I would like to start this review with a general discussion about the proposed approach. It is my understanding that in an unstable ABL, the faster wake recovery (with respect to neutral conditions) that is observed can be attributed to the large levels of wake meandering that smear out the wake. The meandering itself is due to a large amount of lateral (y-wise) turbulence intensity in the ABL. In other words, it seems difficult to me to neglect the anisotropic nature of the ABL when dealing with wind turbine wakes. I think it is necessary to include a discussion on that topic in the paper and explain how the authors think they can deal with such anisotropic flows using a two-equation turbulence model, based on an isotropic turbulence assumtion. I understand the main objective is to propose an efficient, intermediate fidelity model (i.e. in between analytical and LES), but the necessary physics should be there and properly represented.

The authors introduce a new so-called “cstB” model to account for buoyant TKE production. Although the reasoning is clearly explained, there is, from my point of view, a major drawback in this paper: the model, that seems to make sense physically speaking, is never quantitatively validated, and thus none of the assumptions are properly justified. Only a brief qualitative discussion is provided. It is surprising, since two cited references (Zhang et al. 2013, Hancock and Zhang 2015) contain quantitative data that could be used for validation purpose, if I am correct. Furthermore, a minimal validation/verification of the model consistency, that should be provided, is a comparison to the flux-gradient approach (section 3.1), with the integration of the temperature equation in the system. I guess this is feasible with Ellipsys and should be integrated for comparison/validation purposes.

It is my point of view that these drawbacks also apply to the presented modifications to the k-epsilon-fp model. Some improvements are introduced. These are mainly based on mathematical consistency (i.e. neutral ABL limit) but are not properly justified (no real physical explanations are provided). And, in the end, the proposed validation cases focus on “global” flow properties such as wake velocity deficit or TKE levels. It is my opinion that this paper would gain a lot by showing proper comparisons to LES simulations (or experimental data): one might be able to extract the dissipation rate, the eddy-viscosity (and then estimate fp through (16)), or other quantities that would help to asses the pertinence of the choices that are made. See P.E. Réthoré PhD Thesis as a typical example.

About the presented results:

- the TKE levels appear to be overall over-estimated, while the velocity profiles match well. Can the authors provide some analysis on this inconsistency?

- Only single-wake results are compared with LES and/or experiment, as indicated in the paper title. However, it seems that some of the presented LES are based on multi-turbine simulations… Which may rise some doubt about the capacity of the model to deal with wake superposition. Does it perform well in such cases, as for the single wake cases?

About the grid convergence study: wake TI and velocity profiles are extracted 1D behind the wind turbine. However, the presented validation results are taken at 3, 4, 5… up to 12D. And I have the feeling it is easier to converge at 1D than at 12D, since there are much less diffusion effect. What is the reason for this choice? I personally think it is necessary to include the profiles at, let's say, 6 and 12D behind the wind turbine.

One last general remark: can this model be adapted to stable conditions? And if already done, how does it perform? I guess it is the authors objective to end-up with a model that is valid for all the classical ABL stability a wind turbine may encounter.

In the end, both models seem to lead to improve previous modelling approaches, and it sounds like a step forward is achieved in this paper. Thus, I strongly encourage the authors to provide more justification and adapted validation to their work. This will surely provide some confidence in the proposed approaches, although I am aware having high quality data for such unstable cases is not that easy. Most probably running and comparing the RANS approach with an LES simulation would help.