Brief communication: A fast parabolic RANS numerical model with analytical pressure closure for continuous induction-to-far-wake prediction in wind farm flows by Frédéric Blondel and Majid Bastankhah

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November 1, 2026

The authors employ a recently introduced analytical solution of a single wake pressure field to replace the turbine forcing in a simplified momentum equation resulting in a reduced-order RANS model (FM-RANS). The authors compare the FM-RANS results to a full RANS model, for a range of single wake cases and one small wind farm case consisting of 4 aligned turbines with 7D spacing, and conclude that their model performs well. The article is relatively well written and easy to follow, but the present comparison cases and analysis are too brief. I am aware that the article is a brief communication; however, the conclusion must be supported by the main results. The present results are not sufficient to conclude that the FM-RANS model performs well in general, especially for larger wind farms where one would expect a wind farm induced pressure field that differs from a superposed single wake pressure field. Therefore, I strongly recommend the authors to include a wind farm test case consisting of 50-100 turbines using different inflow wind directions, and a below- and above rated inflow wind speed to trigger different thrust coefficients. I have listed a number of main and minor comments below.

Main comments

1. A brief communication article can only include 3 figures or tables; the present article has 5 (4 figures, 1 table). The authors can address this by replacing Tab. 1 with a sentence in the text, i.e. $\{C_R, C_\mu, ...\} = \{4.5, 0.03, ...\}$, and by removing a figure, I suggest Fig. 1 because the plotted data represents simple analytical expressions.

2. The use of a single wake pressure field including an upstream part results in a numerical model that requires an iterative solution method because the turbine effective wind speed (used to set the thrust coefficient) is also affected by downstream turbines. This is similar to engineering wake models including a blockage model. The authors briefly discuss the iterative nature of their model and mention that only a few iterations are needed. One could argue that the proposed model is no longer parabolic but rather elliptic. The authors could rename the model and rephrase the article title without the word *parabolic* by for example changing the title to *"A fast reduced-order RANS numerical model....*

3. The proposed and full RANS models use a uniform thrust loading. While this is logical first step, a uniform thrust load does result in stronger wake effects compared to a more realistic thrust load [Simisiroglou et al., 2017]. The fact that the root section is normally characterized by a relative small thrust load helps the wake to recover faster, which is the main idea behind the wake-diffusion rotor concept of Equinor [Mordasov, 2022]. Is it possible to derive an analytical single wake pressure model for a more realistic thrust force distribution for the present numerical model? This could be discussed in the paper and may be suggested as a future work.

4. Some variables are undefined or written in different forms: k (Line 25), p (Eq. 2), P (Line 57, I guess it should be p?), is u_0 (Line 59) the same as u_h from Fig. 1?

5. Equation 3: Is it correctly understood that the single turbine pressure fields are superposed.

6. The single wake pressure field depends on C_T, but the latter is not known in a wind farm. Are the turbine thrust coefficient also updated iteratively similar to u_0 as discussed in Sect. 3.4?

7. Figure 3 depicts the single wake centerline streamwise velocity. I would suggest to to plot the rotor-averaged streamwise velocity as is it more representative for wake loss modeling. The author could consider to add a second y-axis, or sub figure showing the model difference/error in percentage of freestream velocity between the models, i.e., $(U_{FM-RANS} - U_{RANS})/U_\infty \times 100\%$. The discussion of Fig. 3 does not mention the magnitude of the model errors.

8. The authors test the RANS-FM model for a single wake using different inflow cases set by a range of ambient turbulence intensities and two thrust coefficients, and the results compare well with RANS. The latter may be somewhat expected since the applied analytical solution for the pressure field is based on a single wake. The authors test the FM-RANS model for a small wind farm of 4 turbines with 7D spacing, for a single inflow case representing an aligned wind direction, a single TI value and an unreported inflow wind speed. I recommend the authors to report which inflow wind speed is used. While this is a fine first test, it is not sufficient to conclude that the FM-RANS model performs well for wind farm simulations in general. I expected larger errors for large wind farms, since the employed analytical solution of pressure may not hold for such cases. Therefore, I strongly recommend the authors to perform larger wind farm simulations, i.e. in order of 50-100 turbines for different wind directions, and a below and above rated wind speed to trigger different thrust coefficients. Furthermore, Fig. 4 does not show how large the model errors are. The authors do write that error in the streamwise velocity is only 1 %, but inspection of the near wake results of Fig. 4 suggest larger errors. Finally, the authors do not report any results or errors on wind turbine power, which is often the main output of reduced-order wind farm models based on RANS.

9. Computational costs. The authors report a reduction in computational effort of the FM-RANS model of 2000 to 7500 based on RANS-AD simulations of a single wake and a wind farm of 4 turbines. The authors report the same 32 CPU hours for both the single wake and wind farm simulations, which may be a typo? Or is this because the same numerical domain is used for the RANS simulations? Furthermore, the authors choose a conservative cell spacing of 16 cells per rotor diameter. However, RANS wind farm simulations employing PyWakeEllipSys (see for example van der Laan et al. [2026]) are often performed with coarser grids, namely, 8 cells per diameter. The reviewer is aware that the required resolution is solver and case dependent. However, is the achieved reduction in computational effort similar for a resolution of $D/8$? In addition, a parametric inflow study of RANS wind farm simulations, e.g. a range of inflow wind directions and wind speeds required for an AEP calculation, can be simulated consecutively by employing Reynolds-number similarity and homogeneous terrain [van der Laan et al., 2019, 2022]. Such a simulation method can reduce the number of required iterations by factor 2 to 3 for a wind farm and a factor 10 for single wake simulations, because a previous results is used as a hot start for the next inflow case. An example of a recent work [van der Laan et al., 2026], employed RANS simulations of a wind farm of 8×8 turbines with $8D$ spacing, for 32 inflow cases, which required 3176 CPU hours or about 100 CPU hours per inflow case or 1.5 CPU hours per turbine and inflow case.

Minor comments

1. The citations in the text do not link to the reference list. I am not sure why this is the case but normally such links are achieved by using the Copernicus latex template.

2. Line 107: *non-linear* should be removed from *the k-ε-f_P non-linear eddy-viscosity model* as it is a linear model (using a variable C_μ that acts as a local turbulence length limiter).

References

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