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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on “FLOWSTAR-Energy: a high resolution wind farm wake model” *by* Amy Stidworthy and David Carruthers

Anonymous Referee #3

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Review of Amy Stidworthy % David Carruthers: FLOWSTAR-Energy: a high resolution wind farm wake model

The paper describes a wind farm wake model that essentially treats the momentum deficit caused by rotor drag as passive tracer. This is not a new idea, and it might even be a good one.

I have several concerns about the results in this paper:

The whole set of model equations is not presented, and proper references are not give. There should be references, including equation numbers, to each and every model eqauation so that the (enthusiastic) reader would be able to recreate the calculations. There should also be a table of values of numerical constants used. Otherwise the model is just a black box of little interest except to its owner.

The 'typical value' of the Charnock constant is extreme.

The source term for the momentum deficit is 3 times too high. Equation (6) is simply stated witout any attempt to argue for it, and unfortunately it is wrong.

The origin of (8) is a mystery, except that is should somehow be 'based' on a wind tunnel experiment with laminar inflow. The factor 0.4 appearing in (8), indicating a large influence of shear on the turbulence, appears out of the blue without explanation. WT wake measurements indicate an enhancement of turbulence in the wake combined with a reduction of the turbulent length scale so that the turbulent diffusivity does not change that much.

The constants TIupper and TIlower have been 'determined during validation of the model'. This is strictly forbidden.

The fractional bias (Nysted data) is miraculously close to zero given the fact that the momentum source term has been set a factor of 3 too high. I can't help speculating whether this may have been achieved by tweaking the Charnock constant and perhaps other constants. There is nothing in the text that can make me think otherwise.

I recommend not to publish the paper in its present form. Perhaps after substantial revision, but in any case the calculations have to be done again using the correct source term. This will probably change the model results substantially and it is difficult to guess the impact on conclusions.

Comments on the fly (while reading):

p. 2 'Error! Reference source not found'. Twice

My library does not have the CERC reports referred to in section 2 and I could not find them on the net, not even on the CERC web site. I have had troubbles finding other references too, such as Carruthers 1988. This is serious, because model asumptions are only explained very rudimentally in the your paper. I am missing a concise explanation of what your model is all about.

p. 3

line 17 You say that the dispersion of the wake from a given turbine is influenced by the wakes of upstream turbines. How exactly? It sounds as if you are not treating the momentum deficit as a passive tracer after all.

According to Hansen ac=0.4, not 0.2. This gives a critical Ct of 0.96 instead of 0.64 (in eq. 2). Wind turbine Ct values as high as 0.96 are rare, but Ct>0.64 occurs often. It therefore matters if you set ac as low as 0.2. Is there any experimental evidence for ac=0.2? Why not simply use measured values?

p. 4 The source is a square disk, that has a volume! But ok, fig. 1 explains it.

The correct source strength must be

Q Vsrc = Thrust/rho = $\frac{1}{2}$ Ct V^2 pi R^2

and, since Vsrc=dx pi R^2(1-a)/(1-2a), I get

Q=2a(1-2a)V^2/dx

This differs from (6) by a factor 1-2a. Taking the typical value a=1/3, 1-2a=1/3 so that you get 3 times larger Q than I do. I think the reason is that you the advection speed at the 'virtual' source as V instead of (1-2a)V. It is true that the dispersion model does not see any reduction of advection speed, but we have not begun to disperse anything yet. In other words, first Q should determined so that it is consistent with the thrust, and then we decide what wake model to use to disperse the momentum deficit. This is quite serious, a factor of 3 will of course completely change the results.

Who is 'the receptor'?

p. 5

What is the sign of the reflection term in (7) and why? ADMS uses non-Gaussian plumes in unstable conditions. Has the been drooped in your model?

You don't give many detailes about dispersion model, and the references (Hunt 1899, Hanna 1989, Weil 1985) do not seem to adress the ADMS model. You need to give a reference that contains the exact equation that are using. It would have been nice if you had presented the whole model here, and I understand that it would perhaps be a too long story. Onthe other hand, 14 lines is perhaps too short. I suggest you add a short description of how the dispersion parameters are determined from the turbulence and the need to take turbulence generation by wake shear into account.

Section 2.3 presents formulas based on Bevilaqua and Lykoudis (1978), but they cannot be found in the reference. Where do they come from?

B&L used an essentially laminar wind tunnel with Ti<0.3% in the inflow. What makes their results relevant for wakes with turbulent inflow?

p. 6 In (12) '100' should be deleted. If you insist, you could write '100%' here, which of course is equal to one 1.

Line 5: "Tilower and Tiupper are threshold values determined during validation of the model". Tweeking model constants during validation is not allowed. It invalidates the 'validation' and it is not acceptable.

A Charnoch parameter of 0.08 is extreme rather than typical. 0.01 to 0.02 is typical.

p. 7

What role does humidity play in the model?

What exactly is it that is located 750m downwind from the coast?

You limit Ct to 1, so (1) was not used after all. Ct>1 in fig.3. Confusing. In fig. 3 I take it that Ct was made from using (1). Where does the power curve come from?

1 degree wide bins are dangerous because 'the' wind direction cannot be known with that precision. Two different wind vanes will produce two different 10 minutes averages, often deviating several degrees. Successive ten minutes averages differ typically by

about 5 degrees, and there can be large scale spatial inhomogenities. As a result many models will predict too large wake effects for narrow wind direction bins centrered around a direction aligned with a WT row. Predictions for wide wd bins, which are less sensitive to wd ubcertainties, work much better. You may claim the your model is based on measurements and therefore the wind direction uncertainty is built into sigma_y. In that case your results should be ok for both narrow and wide bins. You should check this.

p. 8

Results for the 5 degree wd bin should be supplemented by results from wider bins. Ok, you do it for the Nysted data.

Why is the power from a turbine used to obtain the windspeed instead of the measurements at the met mast?

Section 0???

It is inconsistent to assuming neutral conditions when calculating z0, and then feed the model with very unstable conditions.

I cannot reproduce the roughnesses listed in table 4.

Where do the stability distributions in table 3 come from (what measurements)? They don't immediately seem to be very realistic.

The relevant error bar is the standard error = the standard deviation of the estimated mean value = standard deviation/sqrt(#observations).

p. 9

How was LMO measured at Nysted? With a sonic?

p. 10

Section 0 again.

Both power production and probability vary across a 1m/s wind speed bin which can affect the result as you say. It is probably better to take averages of the ratio of the turbine production and production of the reference turbine(s).

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