Response to Reviewer #1 – Patrick Volker

In the Fitch scheme, as in other schemes, drag forces are applied in turbine containing grid-cells. Then, (I) the WRF model dynamics handles the interaction between horizontal grid-cells through advection, lowering the wind speed inside a wind farm as grid-cells encounter lower wind speeds from up-stream cells and (II) model physics determinates the downstream vertical expansion of the wind speed deficit through turbulent diffusion.
In this way the wind speed will, according to the model dynamics/physics, decrease downstream in the wind farm until an equilibrium between the energy extracted and the energy supply from above is reached. This means that the WRF model determines the downstream development of the wind speed, also within wind farms. In this approach the Jensen method is used to estimate the downstream wind speed Ui inside a wind farm that is then used to estimate a wind speed reduction (Ui/Uh).
Some of my concerns are that the calculation of Ui is not consistent with the WRF wind field and that the use of Uh does not follow the definition of a free stream wind speed.

> *The argument that Ui may be inconsistent with the WRF wind field arises from a misunderstanding of the way the Jensen parameterization works and will be addressed at issue (3) on p. 5 of this response.*
> *The issue that Uh is not an obvious proxy for the free stream wind speed is valid in principle and we address it extensively at issue (1) on p. 2. In brief, the advantages offered are greater than the inconsistency and the alternatives are more arbitrary and prone to errors.*

Considering, furthermore, the increasing size of modern wind turbines, the turbine density per grid-cell will eventually reach one. Methods, as the proposed one, trying to estimate sub-grid wind speeds would only introduce errors. In the case of one turbine per grid-cell Ui/Uh should end up being one, which especially in *real* mode simulations) is not guaranteed at all.

> *While we agree that in future wind farms it is likely that the wind turbine density will reach one turbine per grid cell, most of the wind farms today are still going to be around in the next 20-25 years and will need to be properly parameterized in future WRF simulations for wind energy and weather forecasting applications. On the other hand, the grid resolution in WRF should not be finer than 1-2 km (Lean and Clark 2003, Lynn and Yair 2010, Kanth and Rao 2011, Calmet et al. 2018, among others). Thus, wind farms like Lillgrund and Anholt will still require more than one wind turbine per grid cell. Thus a wind farm parameterization that accounts for sub-grid wakes, like ours, will still be needed for a long while.*
> *Even with one turbine per grid cell, especially for cases of alignment or partial waking, our Jensen parameterization is very likely to outperform the Fitch's because, as we further explain below, relying on the resolved wake alone is not sufficient due to the diffusive nature of the numerical model.*
> *Ui/Uh should not be 1 except in three cases: at the front-row turbines, at turbines that are unaffected by wakes, and at distances such that the upstream wake has fully*

recovered (after 20D in our code). Jensen will properly capture this even with one turbine per grid cell and even with empty cells in between because, although the upstream turbines might be in other grid cells, their impact on the wind speed that a turbine in a downstream grid cell experiences is accounted for. Hence, Ui will not be equal to Uh unless the turbine of interest is in one of the three cases mentioned above. By contrast, with Fitch's, Ui =Uh always, which is incorrect.

Major comments:
(1) Initially, when introducing the Jensen model you start by using U_∞ for the velocity deficit and at l.160 you determine a wind speed reduction multiplying the wind speed by $U(x)/U_\infty$. Later in eq.13/14 the wind speed reduction is replaced by a multiplication of the wind speed by Ui/Uh, stating (l.201) that Uh coincides with $U_{\infty,i}$. I doubt that Uh can be seen as the free stream wind speed U_∞. Here, free stream meaning without influence of the wind farm. One should be aware that the wind speed reduction due to the wind farm starts well ahead of it due to the induced positive pressure gradients. On the other hand a Uh is the average wind speed of a turbine containing grid-cell, possibly in the middle of the wind farm. This wind speed is already influenced by all the turbines in that cell (and additionally by turbines in front of that cell). Therefore, in my opinion, U_∞ can not be replaced by Uh. How to determine a free stream wind speed: A free stream wind speed could be determined for *idealized* simulations, but how would you determine a free stream wind speed in *real* mode simulations (or even if you simulate wind farm clusters), with a strongly inhomogeneous wind field?

The argument that Uh does not exactly follow the definition of free upstream wind speed is valid. We struggled with it ourselves, but eventually we concluded that any alternative would be even more arbitrary and would potentially introduce more errors than using the grid cell wind speed. For example, we could try to identify an upstream grid cell that could be considered free stream. But this grid cell would be wind direction dependent and grid size dependent. If you have a wind farm that is distributed among dozens of grid cells, each with its own wind direction and wind speed, which wind direction do you even pick to identify an upstream grid cell? If the wind farm is, say, 50 km long, does it really make sense to pick an upstream wind speed (and direction) that is 60 km away? What about the possible induction zone? We concluded that any other choice would be more arbitrary than just choosing Uh.
In addition, a parameterization should be easy to implement and fast. It would be impractical to propose a parameterization that relies on idealized simulations without the farm. Choosing Uh is straightforward, easy to implement, and appears to work well.

We changed the notation at lines 160 to be consistent and replaced U(x)/U_inf with Ui/U_inf.

We also added this discussion on p. 8:
"Alternative choices could be made for Uinf,i, such as the wind speed at a grid cell located at some distance upstream of the wind farm along one wind direction (among the varying wind directions simulated inside the farm), or the wind speed from offline

simulations without the wind farm. However, any alternative would be even more arbitrary and would potentially introduce more errors than simply using the local grid-cell Uh. Another advantage is that Uh is straightforward to implement in the codes and fast to execute."

(2) Section The wake superposition methods:
(I) I could not find your eq.10 in Katic et al. 86 that only uses velocity deficits.

The equation given in the Katic et al. (1986) paper is:

$$\left(1-\frac{V}{U}\right)^2 = \left(1-\frac{V_1}{U}\right)^2 + \left(1-\frac{V_2}{U}\right)^2$$

where U is the free stream velocity. Each term in the parentheses is a normalized wind speed deficit, thus, using our notation, this equation can be rewritten as:

$$\delta^2 = {\delta_1}^2 + {\delta_2}^2 = \sum_{i=1}^{2} \delta_i^2$$

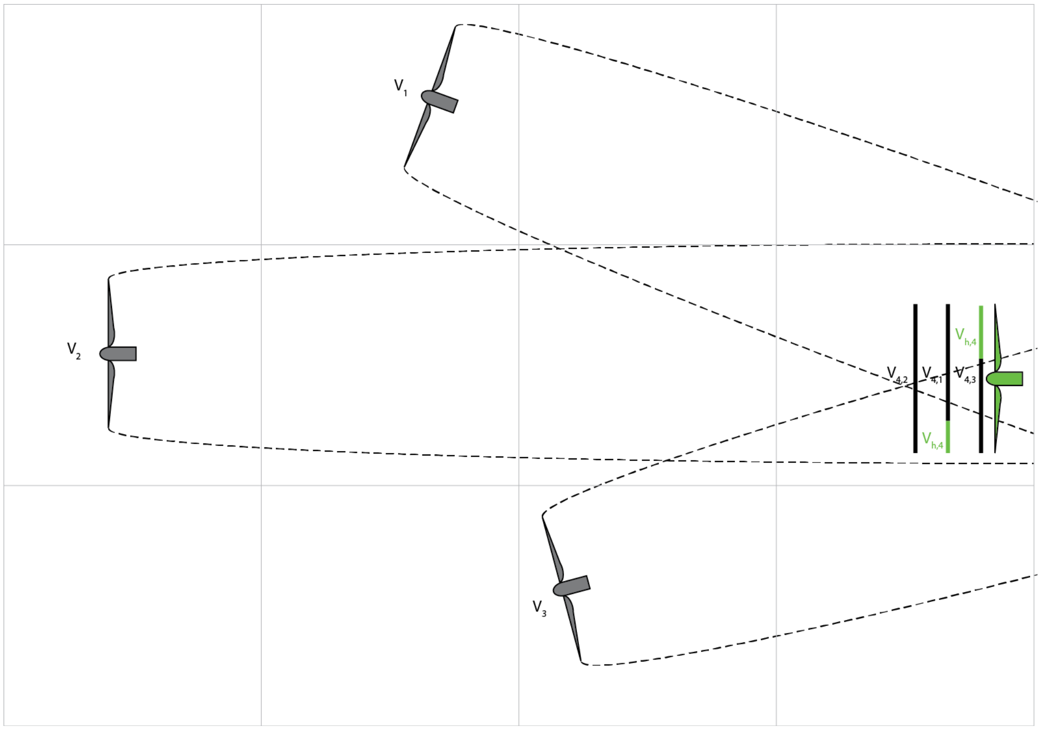
which is the sum of the squared deficits as stated in our manuscript.

(II) The methods M1, M2 and M3 all depend (through δij) on the downstream distance x. Instead, the proposed M4 method seems not to depend on a downstream distance x. If this is the case, I would expect it to give the same wind speeds for a narrowly and widely spaced turbines and is therefore not very useful to predict wind speeds inside wind farms.

*Method M4 is a function of x, via the U_ij term (given by Eq. 8), which is a function of δ*ij.

(III) Regarding the M4 method. One would assume that proceeding downstream the wind speed would decrease and ideally reach at a certain point an equilibrium. When I tried just an example with 3 turbines having wind speeds of 7, 6 and 5m/s. Then, the wind speed of the 4th turbine would be sqrt(110/3) = 6.1m/s, which is above the 5m/s of the 3th turbine. However, it is also not clear if the M4 method would not just start at the first turbine and proceed one by one downstream. If the first turbine would face say 8m/s, then the wind speed at the second turbine would be U2 = sqrt(64/1)=8 m/s and at the thirst turbine it would be U3 = sqrt((64+64)/2) = 8 m/s, is this right?

The reviewer is correct that M4 will always give you basically an average of the wind speeds of all the wakes involved, which by definition will be higher than the lowest wind speed (and lower than the highest). This makes most sense when the turbines are actually ***not*** *aligned with the wind direction, which is the case for which this method was designed (multi-cell).*
To understand this, let's look at the figure below, in which each turbine is in its own grid cell and the wind directions are slightly different in each grid cell, as is the case in real simulations (also the wind speeds are different, of course).



Turbine 4 is affected by 3 wakes, each of which causes its own wind speed (e.g., V4,1 is caused by turbine 1 and is a partial wake case, with some of the rotor affected by the wake and the rest unaffected, thus experiencing Vh,4). The example that you suggested is such that V4,2 = 7 m/s, V4,1 = 6 m/s, and V4,3 = 5 m/s. These are the resulting wind speeds at turbine 4, they are not the wind speeds at the upstream turbines. [Maybe this was a reason of confusion?] The resulting wind speed at turbine 4 will be 6.1 m/s, as you calculated, as a result of the mixing of the three individual wakes. Why should the resulting wind speed be lower than 5 m/s? If that was the case, neither mass nor momentum nor energy would be conserved and the equilibrium that the reviewer correctly referred to would never occur (it would monotonically decrease).

If, as the reviewer implicitly assumed, the turbines are aligned, then this is less intuitive and may even appear to be incorrect. We are used to seeing the squared deficits from the Jensen's top hat shape overlapping and reducing the wind speed more and more with more overlapping wakes via the M2 method. This is actually not happening in the real world. We know that the wind power and wind speed plateau very quickly at the second or third turbine in a row, due to partial wake recovery and entrainment. If you look at the figures in our manuscript for directions 222° or 180° at Lillgrund, or 339° and 183° at Anholt, you will see observational data that indicate that the third turbine generates more power than the second. With method M2 this cannot be reproduced, but with M4 it is.

Method M2 (and M3) does not conserve energy or mass or momentum. Why should the squared deficits be conserved? They are a proxy for kinetic energy, but the mass is not *accounted for and air is incompressible, thus density cannot change and it is physically impossible that kinetic energy sums up that way. However, the research community has been doing it regardless and has suffered the consequences, i.e., more significant errors than those of M4.*

To shed more light on the underlying physics of M4 versus M2, let us assume n equally-distant aligned turbines. Then the equation for M2 is basically a geometric series like this (where x is the normalized deficit):

$$\sum x^{2n}$$

with |x|<1. Thus, as n increases, it converges to:

$$\lim_{n\to\infty}\sum x^{2n}=\frac{1}{1-x}>1$$

which is greater than 1. This means that each additional wake deficit incrementally reduces wind speed and can even cause a zero wind speed or even negative. This is not physical and would never occur in reality. Method M4, however, does not suffer from this problem.

M4 does not conserve mass, momentum, or energy either, but it will never result in a negative or zero wind speed and can be thought of a way to account for entrainment and partial wake recovery. With M4, the resulting wind speed will be the average of the (partially recovered and therefore relatively high) wind speed from the farthest turbine and that (fresh and relatively low) of the nearest turbine, plus all the others in between. M4 effectively avoids the unrealistic continuous drop in wind speed as more and more turbines are aligned that other wake superposition methods suffer from. The fact that it outperforms other methods is also an indication that it is not incorrect.

We added the following additional sentence on p. 7:
"In a sense, M4 can be thought of as a way to indirectly account for partial wake recovery due to the added turbulence from multiple wakes and entrainment."

(3) You mention that the wind speeds in large wind farms are not expected to be homogeneous (l.87-l.93) and that it should be accounted for. This is exactly why physics parametrisations operate one dimensionally and do not intervene in the horizontal model direction. In this way the wind speed field remains the inhomogeneous model wind speed and a wind speed reduction in a turbine containing grid-cell follows the local grid-cell velocity. On the other hand, this approach seems to assume a constant wind speed in the downstream distance, when calculating downstream wind speeds Ui throughout the whole wind farm (row) and neglects therefore WRFs wind speed variability in the downstream direction. Consequently, the calculated downstream wind speed Ui seems not in line with WRFs wind field. Problems can be expected when (large) wind farms are in regions with large wind speed gradients (coastal wind farms) and in unsteady flows.

We do <u>not</u> assume a constant wind speed along the downstream distance. There is a different Uh in each grid cell and a different Ui at each turbine in each grid cell. We were very careful with our notation and only used i,j for the turbines, not for the grid cells. Thus in a grid cell with cell-average hub-height wind speed Uh, there are several values of Ui, one for each turbine within that grid cell, calculated via the Jensen model. The next grid cell will have another value of Uh and so on. The wind speed variability in a multi-cell farm is fully retained.

(4) In case of multiple upstream wind speeds the M4 method changes and becomes a function of the rotor area, instead of being dependent on wind speeds only. Could this sudden change be clarified?

The M4 method does not change, it was a function of the rotor area before as well (compare the expression for Ui given by Eq. 8 with that given in Eq. 20). The only difference is that for the single-cell case there is only one value of U_inf, whereas in the multi-cell case there are different values of U_inf, one from each relevant grid cell, due to the wind direction variability.

(5) The total thrust per mass for a turbine i that faces a wind speed Ui should be proportional to Ui^2. Eqs 13 and 14 look differently. Could you explain the difference?

Each turbine i experiences a wind speed Ui at hub height. In Eq. 13 and 14 we instead want the change in the wind velocity components u_k and v_k at each level k. We first calculate the wind speed at that level U_k, and then we reduce it by an amount that is proportional to the reduction at hub height Ui/Uh. This new speed is then multiplied by u_k or v_k to get units of m2/s2.
Eqs. 13 and 14 are the same as Fitch's Eqs. 1 and 2, but they are using a reduced U_k (due to the sub-grid wakes, via the correction factor Ui/U_h) rather than the original U_k. Basically, in both the Fitch and Jensen parameterizations, the thrust force is calculated at each level using the wind speed at that level (U_k), not the hub-height wind speed (Ui). At hub height, however, Uk=Uh and the equation becomes the familiar one.

(6) Figs.11 a and c show the wind speed. However, it is important to understand additionally the shape of the (normalised) wind speed deficit: at which height is the maximum deficit? Is there an acceleration at the lowest model level? Theory predicts for the far wake in neutral conditions a Gaussian shaped deficit with a maximum deficit on hub-height (confirmed by CFD and LES results). If a result show other features they should be discussed and compared to reference studies (neutral measurements/LES simulations). The (normalised) wind speed deficit could be obtained by (u(z)−u0(z))/u0,h, where u0 is the free stream velocity. Volker et al. used e.g. a reference simulation without wind farms (same simulation time) to determine the free stream velocity.

This paper should not be treated as a study of fundamentals of wind turbine wakes, rather as an improvement over the existing default wind farm parameterization by Fitch *et al. (2012). Your request to run additional simulations without wind farms in order to calculate a normalized wind speed deficit is extremely difficult for us to do at this point due to a lack of resources. Comparing Jensen results side-by-side with Fitch's indicates that the two are extremely consistent with one another, e.g., the wind speed deficit peaks at hub height for both. However, please let us know if something is suspicious and the results of those additional simulations would help clarify. We will try to obtain the resources required for executing them.*

(7) Fig. 4. You show point measurements indicated by dots (they shouldn't be connected with straight lines) and model results. My question for Anholt is, how did you plot the figures? Are the small dots above the turbine numbers the grid-cell centres? Also, because in the figure all the distances for the 1-86 row seem the same, whereas in the layout the distance between turbine 1 and 31 seems to be larger than the distance between the other turbines in that row.

The distances between turbines in the figure are not proportional to the real distances, basically each turbine is just one tick mark regardless of their actual relative distance. This is a common way to display relative power along turbine rows, especially if some rows are short and others are long (e.g., Archer et al. 2018).
The WRF code calculates the power generated by each turbine, with both Jensen and Fitch. With Fitch, the wind speeds to calculate wind power are those at the grid centers, but, with Jensen, they are the actual Ui at each turbine in each grid cell.

We agree that the figures could be improved by removing the lines that connect the observations. However, the first author, who prepared the figures, left for another job in China and no longer has access to the computer cluster and is no longer in this field of work. Unless absolutely necessary, we would prefer not to change these figures. It would be a huge undertaking for us at this point.

(8) One other concern is the intension to compare model results inside wind farms with measurements. The measurements are locally in strongly inhomogeneous conditions and the model instead is by design very diffusive in the horizontal direction with a true resolution far less than the model grid- size. Therefore, one should be very careful when trying to compare those two worlds.

We added the following to the manuscript:
"We note that the power measurements at any wind farm are the result of local wind speeds at the turbines, often in strongly non-homogeneous conditions, while the model results by design are very diffusive in the horizontal direction, with a true resolution that is coarser than the grid size. As such, a direct comparison of the WRF and MPAS results against the observations reported in the next section should be interpreted with this limitation in mind."

Further comments:

l.141: this link between the induction factor and thrust coefficient is only when momentum theory is applied.

The sentence was modified as follows:
"and, after applying momentum theory, the induction factor a can be related to the thrust coefficient C_T by …"

l.118: in the original definition the added TKE is the difference between the power extracted from the flow and the power converted in electricity. How, can the one quarter of the original value be theoretically justified?

There is not a theoretical reason for 0.25 as opposed to, say 0.3, or 0.1 (but it must be lower than 1 and positive). The value 0.25 simply fits the LES results best. The paper by Archer et al. (2021) actually discusses the limitations of this value extensively. For example: "We recognize that there is not one value that will work for all farms and all resolutions because the added TKE by a wind farm is a complex physical phenomenon that depends on more than just the thrust and power coefficients. However, the current formulation of the Fitch parameterization, especially after the bug fix proposed in section 2b, would dramatically overestimate the TKE added by the wind farm, and therefore even a general correction, such as the 25% factor proposed here, will give more realistic results than would no correction at all."

But the point is that having no correction is theoretically and practically wrong because it implies no electromechanical losses between the turbine and the generator, whereas we know that energy is lost to turn the shaft, the generator, and the gears (if present) and because of other frictional losses. Also, most TKE in the far wake comes from shear generation in the wake, not from the tip vortices.

l.18: it should be that wind turbine wakes *can* significantly decrease the wind farm power production. With high wind speeds and/or large turbine spacing the reduction at a downstream turbine can be low (or approach zero).

The sentence was modified as follows:
"Wind turbine wakes can significantly decrease the wind farm power production (Archer et al., 2018)."

l.39: it should be written that Volker et al. 2015 does *not* apply a TKE source term. Instead, it calculates a grid-cell averaged deceleration and the WRF model calculates the TKE due to the changed wind shear.

The sentence was modified as follows:

"a wind turbine is often parameterized as an elevated momentum sink (Volker et al. 2015) or as an elevated momentum sink and a source of turbulence within the vertical levels of the turbine rotor disk (Fitch et al., 2012; Abkar and Porté-Agel, 2015; Pan and Archer, 2018)."

l.187: as stated in comments before, I would not agree that the first turbine row would face U_∞. Also, what do you exactly mean with: there are no wakes at all inside the grid-cell? There is a grid-cell averaged deceleration, which is the consequence of the wakes inside the grid-cell.

With the Fitch parameterization, each turbine in a grid cell produces the same power regardless of wind direction or layout because there are no internal wakes that would reduce the power production. Thus the Fitch parameterization does not consider any wake effects within a grid cell because it does not include a treatment for sub-grid wakes, thus it has no wakes in a grid cell. It generates a reduced wind speed as a result of the power extraction, not as a result of the wakes. Then the WRF dynamics advects that reduced wind speed downstream, which could be considered a resolved wake, but it is done by WRF and not by the Fitch parameterization.

l.64: could you please clarify what do you mean with *ad-hoc* LES simulations.

"Ad hoc" (from Latin "for this [purpose]") means done for a particular purpose as necessary. Here It means that, if you want a ξ for farm A, you need to run LES of farm A first. If you want ξ for farm B, you need to run LES of farm B. It is not a general correction factor.

l.105: the three equations look a bit misleading, since there is only one source term listed in the model's deceleration (the same holds for the TKE). It would be clearer to introduce an additional source to the WRF deceleration (e.q. force/mass) and define the magnitude of that.

We have followed the same notation and format of the Fitch parameterization paper and we have clearly stated that these 3 terms are "the momentum sink and TKE source terms induced by the turbines" only.

l.185: what do you mean with: M4 is *generally* more accurate? and why?

We present an extensive comparison of the performance of all methods, including M4, in subsequent sections. At this point of the paper, we simply give a quick "spoiler" that the new method is actually good. As shown later, it is the most accurate in all but perhaps 2 cases. We feel that using "generally" is sufficient here.
As for why M4 performs so well, please refer to our previous response to issue (III) on p. 3 of this response.

References

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