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 U_i inside a wind farm that is then used to estimate a wind speed reduction (U_i/U_h). Some of my concerns are that the calculation of U_i is not consistent with the WRF wind field and that the use of U_h does not follow the definition of a free stream wind speed. 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 U_i/U_h should end up being one, which especially in *real* mode simulations) is not guaranteed at all.

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 U_i/U_h, stating (l.201) that U_h coincides with $U_{\infty,i}$. I doubt that U_h 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 U_h 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 U_h. 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?

(2) *Section The wake superposition methods:*
(I) I could not find your eq. 10 in Katic et al. 86 that only uses velocity deficits.
(II) The methods M1, M2 and M3 all depend (through δ_{ij}) 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.
(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 $U_2 = \sqrt{64/1} = 8\,\mathrm{m/s}$ and at the thirst turbine it would be $U_3 = \sqrt{(64+64)/2} = 8\,\mathrm{m/s}$, is this right?

(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 U_i throughout the whole wind farm (row) and neglects therefore WRFs wind speed variability in the downstream direction. Consequently, the calculated downstream wind speed U_i 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.

(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?

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

(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 $(u(z)-u_0(z))/u_{0,h}$, where u_0 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.

(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.

(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.

Further comments:

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

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?

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).

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.

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.

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

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.

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