Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2017-34-RC2, 2017

WIND ENERGY SCIENCE DISCUSSIONS

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Interactive comment

Interactive comment on "Numerical simulation of non-neutral forest canopy flows at a site in North-Eastern France" *by* Cian J. Desmond et al.

Anonymous Referee #2

Received and published: 17 October 2017

The paper reports results of numerical simulations performed over a forested terrain. Meteorological mast measurements are available for validation purposes of the RANS simulations. The authors describe carefully the measurement equipment and the simulation details, but the result quality is very questionable in several crucial aspects

- I was quite surprised that, despite the fact that the mast has tri-axial sonic anemometers, the authors did not use the fluctuating temperature from the sonics to estimate the Monin-Obukhov length, L_o, which is the actual quantity used to discern between stable, unstable and neutral conditions. Another alternative would be the Richardson number, but the uncertainty in the temperature gradient will limit its calculation. There have been several works (see for instance Medici and co-workers at the EWEA conference 2014) that have analysed proxies to get

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Discussion paper

stratification conditions over forested sites and the problems arise especially on the thresholds to be used to discern between the various states: The authors give some values at page 6 without any clear explanation about their origin. In my opinion, the Obukhov length is the best method to discern between different stability conditions although it is rarely available since many masts have only cup anemometers, vanes and some thermometer.

- The method the authors adopted to get the friction velocity is quite strange and strongly relies on the existence of a logarithmic layer. This is not the way wall functions are introduced, for instance, and the value of k at the wall should be instead used (assuming a Neumann condition for k). In case of stable or unstable boundary layer, the $\psi_m(z/L_o)$ function sums to the velocity profile, with an increasing deviation from the logarithmic behaviour, so that their approach is clearly problematic. The fact that they change the reference height were the friction velocity can be estimated (up to 500 m!) indicates that they have little familiarity about what the friction velocity is and the structure of the turbulent boundary layer.

- One of the biggest flaws of the manuscript is that the authors have changed several parameters (h_c, L_x, T_{wall} and even C_μ in the turbulence model) to get quantitative match with the experimental data. Driven by the idea that every model can fit experimental data if one varies the parameters enough, their approach is justified, but unfortunately this is unacceptable in science. What if they had to do another evaluation where the true answer is not available? Rather than doing 47 simulations to find the right parameters, they could have just estimate the average forest height from the available measurements, estimate the LAI from publications or reported values and do nothing more.

- Following the previous comment, I find quite funny that the authors decide to simulate a forest that is twice higher than the real one (they use the settings of simulation 38 for the stable and unstable cases) just because it fits the velocity data. Furthermore, having $L_x = 0.7m^{-1}$ implies a LAI equal to 70 (according to the estimate of Harman & Finnigan), which is really high. The force is so hight that probably almost no flow is present inside the forest.

- Since many PT-100 were available, the vertical temperature gradient was already known, so that I see no reason to perform the stable and unstable simulations where the floor temperature was changed without any criterion. Simulation 51 for instance uses a temperature decrease of 10 K. Did they observe such a high temperature drop in their experimental data?

- The unstable condition is just inconclusive and counterproductive for the paper. The authors underlined that they could not achieve good results there, so that that section adds nothing to the paper.

- I think that the requirement of more validation data in the conclusions is inappropriate. The reality is that they simply need a better solver or forest model. Once they get acceptable results, they could move to other sites in order to validate their methodology.

Minor comments

- The paper from Harman & Finnigan (BLM 2007) should be probably used by the authors. There the authors reported an analysis of the forest boundary layer and proposed a simplified relationship between the loss coefficient L_x and the forest properties as $L_x \approx LAI/(5h_c)$, where LAI is the leaf-area index, h_c is the canopy height and the 5 comes from the assumption of $c_d \approx 0.2$. Usually, a LAI between 1 and 4 is observed, so that L_x should be here around 0.04, namely the standard value proposed by WM.

- The interesting paper from Silva Lopes et al. (BLM 2013) with title *Improving a Two-Equation Turbulence Model for Canopy Flows Using Large-Eddy Simulation* could provide some suggestions to the authors about how to better account for forestry in the k and ϵ equations.
- The comparison shown in figure 8 is unfair as the image on the right has all tree heights there. Besides, the range 2-5 m is not even around the average tree height
- The authors mention that it is possible to alter the temperature at the ground to introduce stratification effects. Are they using a code with the Boussinesq approximation?

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