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WIND ENERGY SCIENCE DISCUSSIONS

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 #1

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Review on the MS wes-2017-34 Numerical simulation of non-neutral forest canopy flows at a site in North-Eastern France by Cian J. Desmond, Simon J. Watson, Christiane Montavon and Jimmy Murphy

The authors try to validate commercially available Computational Fluid Dynamics software (after some modification) against measurements data from a site in North-Eastern France. They conclude that this CFD model is able "to simulate the joint effects of canopy drag and atmospheric stability when considering stable stratification", but unable "to simulate the unstable events in the validation dataset". While I agree with the general idea that in scientific research the negative results could also be quite useful and publishable, the results of this paper do not satisfy these conditions. I have serious reservations about the methodology and model's application to simulate the key

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Discussion paper

parameters needed for wind risk assessment. The conclusions of the paper are poorly written and don't provide any new results or specific recommendations.

Recommendation

I recommend the paper to be rejected.

Main comments

1. I am not sure that threshold values for wind shear and turbulence intensity chosen for conditions of high wind speeds as indicator of neutral stability of atmosphere are suitable for other conditions. "Narrow range of values for wind shear", which is rather insensitive to solar irradiance, with high probability indicates the convective regime of atmosphere. Simultaneous increase of turbulence intensity with increasing solar irradiance (Fig. 5) also confirms this state of atmosphere. Thus, identification of stable, neutral and unstable events as shown in Fig. 6 is wrong. The authors obviously identified the atmosphere states based on the data from the height of 80 m only, which has nothing in common with real atmospheric stability. It can explain why the model results don't match with identified stability regimes. I do not understand why the authors did not check identified regimes against the measurement data as it has been done in Desmond and Watson (2014) for Norunda site. It seems that the set of sensors used in measurements allows the identification of all parameters needed including heat flux, temperature and wind speed profiles.

2. Description of the model is not sufficient for readers, who do not work regularly with WindModeller software package (the authors didn't provide any references). Thus it is difficult to understand how the model describes the stratified flows, specifically what kind of equations are used? Before applying the model to the real situation, I would advise to test the model against simplified flows over an open place or forest in one-dimensional mode.

3. Without any proof that the model adequately describes main flow properties in

atmospheric boundary layer, the authors were unsuccessfully trying to identify parameters and boundary conditions of model that would provide the better fit with validation dataset. Actually, in conclusions they mentioned, that “due to the fact that validation data is limited to a single measurement location, it will not be possible to fully appreciate the ramifications of such alterations on the overall quality of the simulation”. It seems that only this fact did stop them from new numerical experiments.

4. Generally I did not find any clear strategy in modelling experiments – most of them could be performed in one-dimensional mode. For example, I consider that numerical experiments with $C\mu$ value were absolutely superfluous, because $C\mu$ in CFD models is strictly related to TKE, and therefore to Turbulence Intensity defined in the paper by Eq. 12. Playing with vegetation parameters without information on real vegetation looks also weird. On page 20, lines 18-21, the authors came to conclusion that “the average LAD for the Vaudeville forest is approximately 3 m-1 “, which with h = 10 m will provide incredibly dense forest with LAI = 30. I understand that the model can accept any LAD values as well as any surface temperature, but more realistic values would be better.

5. Finally, the paper provides an impression that it was hastily written; there are many imprecise and incomplete formulations and references in the text.

Desmond, C., Watson S., 2014. A study of stability effects in forested terrain. Journal of Physics: Conference Series 555.

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