• 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, Lo, 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 C1 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.

-> Unfortunately we did not have access to the full set of experimental data from the Vaudeville site. This limitation was mentioned in (<http://iopscience.iop.org/article/10.1088/1742-6596/555/1/012027/pdf>) and has now been added to the current paper.

It was shown in (<http://iopscience.iop.org/article/10.1088/1742-6596/555/1/012027/pdf>) that the method of identifying the non neutral events in the current paper was “correct” 86% of the time when compared to the standard deviation of direction method and approx. 60- 70% of the time when compared to the Richardson number. The method used proved to be “correct” over 90% of the time when compared to the Obukhov length, but that was for different sites. We thus have not faith in the method used rather than the other available otions (e.g. Richardson Gradient etc). Given that the manner in which the stability demarcation is used for illustrative purposes in the current paper, we feel that this level of accuracy is acceptable. These points have been clarified in Section 2:

“Access to the full 3D Sonic datasets were not available with only 10-minute mean wind speed and standard deviation of wind speed provided for this research, thus it was not possible to calculate the Obukhov Length directly. In order to isolate non-neutral data with which to validate the CFD simulations, the steps described in Section 2.1 were taken. This methodology was previously applied to four sites, including Vaudeville, to isolate non neutral events and was found to provide an accurate demarcation of stability class when compared with more conventional measures of stability such as the Obukhov length, the Richardson number etc [Desmond & Watson (2014)].”

And in Section 2.1:

“The stability demarcation displayed in Figure 6 is used for qualitative purposes in this paper to assess the performance of the CFD model. The quantitative results achieved in determining stability class using this method are discussed in Desmond & Watson (2014) where it was shown that up to 90% agreement was achieved when compared with demarcation achieved using direct measures of the Obukhov Length.”

• 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 ψm(z/Lo) 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.

The first version of the paper may have been confusing when describing a derivation of ustar, which was only trying to explain the mechanics of how the upstream boundary profiles for the velocity were setup following the Zilitinkevich formulation. From this formulation, a geostrophic drag law can be derived for ustar/geostrophic wind speed, which is used in the model to specify the velocity profile from the geostrophic wind speed.

The way the model wall function is imposed is totally independent from this, and has now been explained in the revised version. The wall function indeed calculates the friction velocity from the tke, and the equation for the tke uses an adiabatic condition for the tke at the ground.

Hopefully this point is much clearer in the current version of the paper.

• One of the biggest ﬂaws of the manuscript is that the authors have changed several parameters (hc, Lx, Twall and even Cµ 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.

We would argue that interrogating the commercially available, and thus reasonable approximation of state of the art, software in this manner provides valuable information on its use and limitations to the scientific community.

We have removed the Cu section, and have added the following to clarify the motivation of the paper:

“The motivation of this paper is to assess the use of Computational Fluid Dynamics (CFD) modelling to consider forest canopy flows where there is uncertainty in the canopy density and the level of atmospheric stability. The accuracy of the model predictions within levels of uncertainty of these two parameters is assessed in order to highlight areas where further validation data and research are required.”

• 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 C2

simulation 38 for the stable and unstable cases) just because it ﬁts the velocity data. Furthermore, having Lx = 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.

* You are assuming that the “real height” is the average height of the entire canopy as measured by Intermap? These data include values of up to 35 m if you allow for the margin of error. Also, it was stated in (Texier O., Clarenc T., Bezault C., Girard N., Degelder J., 2010. Integration of atmospheric stability in wind power assessment through CFD modeling. Proceedings of EWEC 2010, Warsaw, Poland, 20-23 April 2010.) that the canopy height on site was 30 m, so the values that we have used is not unreasonably. This detail is contained at the top of p.22.

Lx is the product of the Leaf Area Density and the Canopy drag coefficient. The resulting LAD is high but realistic , this point is discussed at the end of page 24 and suitable references are provided.

• 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 floor temperature was varied to impose non neutral conditions. The fact that the values required were far larger than what would be deemed reasonable is discussed in the manuscript at the top of page 24. We could include an analysis of how the actual stratification in the CFD compared to the stratification observed on site if required. However, as we were examining the ability of the code to simulate the impact of the stratification (TI and Shear) and not the stratification itself, we felt that this was unnecessary.

• 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.

We have now removed the unstable section and have replaced with some comments on what was attempted rather than the full details of the analysis.

• 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.

We feel that any model will have to be adjusted to take local conditions into account. It is not simply a case of getting it right once and using the same configuration for every site. We hope that this paper will assist with the “tuning” process at other sites, at least until we have a fully deterministic model of the ABL.

In terms of the requirement for additional validation data, we strongly feel that validating such complicated flow with data from a single mast is not sufficient. The full development of the forest boundary layer needs to assessed. This will be significantly impacted by the prevailing stability. Thus we feel that the conclusion is appropriate.

• 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 Lx and the forest properties as Lx ≈ LAI/(5hc),where LAI is the leaf-area index, hc is the canopy height and the 5 comes from the assumption of cd ≈0.2. Usually, a LAI between 1 and 4 is observed, so that Lx should be here around 0.04, namely the standard value proposed by WM. C3

The definition of the canopy loss coefficient depends on the formulation used, we are using Lx = LAD x Cd. This is discussed in Section 3 of the paper. The formulation used by Harman and Finnegan results in an Lx which is far lower than what would appear reasonable for the site.

• 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 equations.

We have added the following caveat to the conclusions to calrify this aspect of the paper:

“It should be stated that this paper interrogated the ability of the WM software package, a reasonable approximation of the industrial state of the art CFD model for the purposes of wind resource assessment, to emulate the combined effects of atmospheric stability and forestry drag. As the state of the art develops, the formulation used by WM may be found to be inadequate when considering these complex flow regimes which may require the use of LES, DNS or modifications to the K-Epsilon model such as proposed in Silva Lopes et al. (2013) and Segalini et al. (2016). “

• 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

Agreed, this image was unhelpful and has been removed.

• 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?

Yes buoyancy is implemented via a Boussinesq approximation, now detailed in the paper