Response to Review

Reviewer #1

General Comment

The manuscript was revised extensively and improved significantly. I have some minor concerns for the authors to address before the manuscript is published.

We followed all the reviewer's suggestions and hope that the revised manuscript reads better, and that the minor concerns listed below have been addressed. We include the reviewer's original comments, and list our responses to specific comments in red font.

Specific Comments and Technical Corrections

1. Page 13, line 29: Please explain why h = 22 m was used for the classical case.

This is a long-term average canopy height over the flux footprint in the observed site over the entire observation period. This has been made explicit in the paper.

2. Page 14, line 1: For the realistic LES case, z_0 = 0.094h was given on page 14 (line 1). It is inconsistent with z_0/h = 0.05 and z_0 = 0.9 m given in Table 1 and z_0 = 0.94 m given in Table 3.

The values in the text are correct, we made a typo in the table (we did not updating from previous incorrect results). We corrected the table. Thanks for catching this.

3. Table 1, case (e): The values d/h = 0.67 and d = 14.2 m give h = 21.2 m; whereas the values z_0/h = 0.05 and z_0 = 0.9 m give h = 18 m. These values of canopy height are inconsistent with each other. Nor are they consistent with h = 27 m given in Table 1.

This is the result of the same table typo. We corrected it. The numbers in the table are now correct and consistent.

4. Table 1, case (e): Please explain why the result of h_a for this case is significantly lower than the canopy height, whereas the results of h_a for all the other cases in Table 1 are very close to the canopy height. Putting case (e) into Figure 2(c) will change the conclusion on page 17 (line 16) that a linear relationship exists for h_a and gap fraction.

The realistic case (e) varies in a number of ways from all the other cases in Table 1. The other cases represent simplistic virtual canopies and were made to only alter one variable at a time, to isolate potential results from just that variable – LAI, gap fraction, etc. So for instance, in case (d), the portions of the canopy that are 27m are all the same (in height, LAI and vertical leaf density profiles) and likewise for the parts at the gap (at 9m, otherwise the same as the 27 m parts, and uniform in all aspects within the gap parts). However, in case (e) the entire canopy is heterogeneous, and based on a lidar scan of the actual canopy. Similar to the gap fraction text, the taller portions of the canopy fluctuate around 27m, and the gaps around 9m. But unlike the other tests LAI varies as well as height, vertical leaf density profile and the shape and organization of gaps, throughout the domain. As a result, we would not expect this particular case to be very similar to the others, since more variation takes place than just gap fraction, it shouldn’t be included in Figure 2(c) which is only looking at the effects of varying gap fraction.

5. Table 1, from cases (d) and (e):

Experiment	LAI	LAD	Height (m)	Gap Fraction	d (m)	z_0	h_a
(d)	4.2	Natural	27	0%	20.1	2.9	27.1
(d)	4.2	Natural	27	10%	20.4	2.7	27.0
(e)	4.2	Natural	27	5%	14.2	0.9	16.7

It looks that the setup of these simulations are only different in the gap fraction. Please explain why the results of d, z_0 and h_a for the case of a gap fraction of 5% are so different from the other two cases, whereas the results of d, z_0 and h_a for cases of gap fractions 0% and 10% are very similar to each other.

As we explain in the answer to the previous comment, because of the many differences and variations in the realistic case beyond just a different gap fraction, we wouldn't expect the resulting values of *d*, z_0 and h_a to be similar to the results from the primarily homogeneous cases of the gap fraction experiment.

Reviewer #2

General Comments

The authors accomplished an improvement of the manuscript by the revision.
However, there are still some points to clarify (see below the remarks on Fig. 4) and minor corrections necessary. The discussion section contains repetitions and could be written more concise.

We followed all the reviewer's suggestions and hope that the revised manuscript reads better. We include the reviewer's original comments, and list our responses to specific comments in red font.

Specific comments and technical corrections

The following comments are indicated by page and line numbers of this manuscript; whereas, for better readability, the page number was reduced by 16000 (e.g. P350L21 indicates page 16350, line 21). Recommendations are led by an arrow '→'.

P5L12: → "In brief, MOST describes the functional relationship between surface stress and the parameters *d* and z_0 and wind speed using a logarithmic function" – This sentence disguises the physical background. It is rather the integral form of the flux-gradient relationship using the parameter z_0 and *d* to parameterize the unknown offset in *u* and *z* respectively). Check the consistency with the first sentence of the paragraph.

The section have been reordered and wording changed to better reflect the physical background accurately and consistently with the first sentence. It now reads: "*Monin-Obukhov similarity theory (MOST) describes the relationships between the mean horizontal wind speed and the friction velocity in the inertial sublayer (Monin and Obukhov, 1954). In brief,* *MOST describes this relationship using a logarithmic function with parameters d and z_0. Further details on the formulation of MOST used in this work are described in Maurer et al., (2013)."*

P7L8: "distance" → "range"
Done

P7L5: →Please, shift the sentence "We determined the effective aerodynamic canopy height, h_a, by identifying the height of the inflection point in the vertical wind-speed profile. This height marks the transition between the sub-canopy and above-canopy flow regimes (Thomas and Foken, 2007b)." from P11L8 into this paragraph.
Done

P10L16: → delete "at that height level"
Done

P13L15: "A 'Biometric' *d* was then calculated using Eq. 10." How is this done? Did you use Eq. 11 instead?
Yes, we used Eq. 11. We corrected this in the paper.

P13L10: "We calculated a 'Biometric' h_a using the relationship we found …" → "We calculated a 'Biometric' h_a for the US-UMB site using the relationship we found …"
Done

Eq. 13 shows the same relation as Eq. 7
We have added a subscript 'b' to h_{a_b} in this equation, Eq. 12, and the surrounding discussion to reflect that this h_{a_b} is the one calculated from the biometric equation – Eq. 12. While in eq. 7 it is the empirically observed h_a. Though it is the same relationship the different sources for the information merit different equations. Additionally, eq. 13 is in a rearranged form.

P16L19: "parameter" → "parameters"
Done

P17L2: "figure" → "Figures"
Done

P19L9: "placment" → "placement"
Done

P21L11-13: The sentence has got no verb.
Though there was a verb ("required") the sentence has been restructured and reworded to make it clearer, and read better.
It now reads "LAI, canopy height, and gap fraction or stand density are required by both the Nakai et al (2008a) approach and the approach derived by the virtual experiments in this study (the 'Biometric' approach) in order to determine z_0 and *d*. "

P21L14: "'Classical' approach" or Nakai et al (2008)?
It is comparing results by the biometric approach to both the "Classical" and the Nakai approach. It has been reworded to more clearly show this.

P21L23-26: "The 'Biometric' method … adding small perturbations to displacement height based on LAI and gap fraction… " – Using Eq. 11, the displacement height depends only on $h_{\max}$. What influence have LAI and gap fraction on *d*?
Yes, displacement height depends on h_{max}, but (we forgot to include) roughness length is affected by LAI and gap fraction according to equations 11-13. The paper has been

changed to reflect this. The sentence now reads: "The 'Biometric' method presented in this study is essentially a variant of the 'Classical' method, with the major difference being the use of a variable maximum canopy height as opposed to mean canopy height, and adding small perturbations to roughness length based on *LAI* and gap fraction (Eqs. 11-13). "

P21L28: "We predict that this method will significantly improve the prediction of friction velocity…" – On what basis did you make this prediction?

We have toned down this claim to one that follows clearly from the results shown in the paper. "Our simulation results suggest that this method could potentially improve the prediction of friction velocity when applied to situations where canopy structural variability is larger, such as after significant disturbance events."

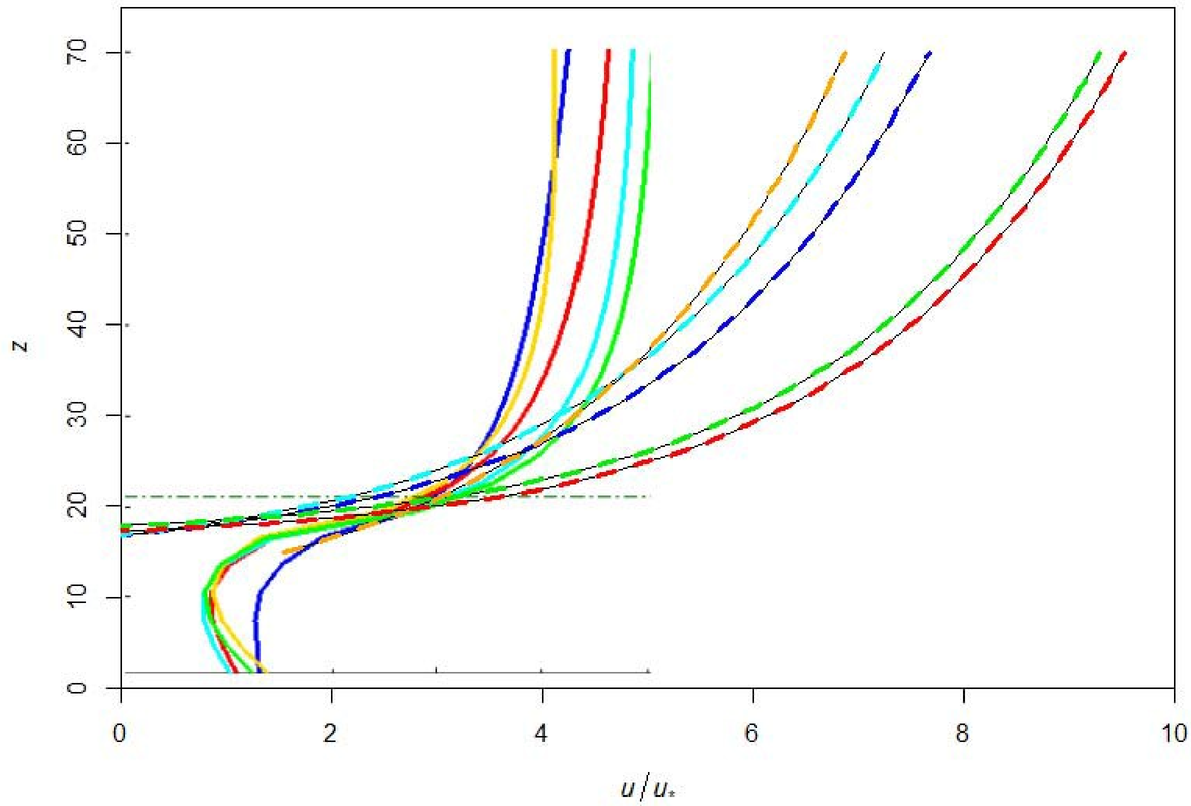
Figures

I found no reference to Fig. 4 in the text.

We apologize for this omission. This has been added at to the results section, with the reference to figures 5 and 6.

Fig. 4a: Those are still strange profiles showing very low normalized wind speeds. Considering neutral cases, the shape of the profiles should resemble the form $u/u_* =$

$1/0.4*\ln((z-d)/z_0)$. Assuming d = 15m, one would need z_0 > 5m, to reach u/u_* < 5, within a height of 50 m. z_0 = 3m results in u/u^* = 6.1!
Using the d and z_0 values given in Table 1 and omitting the influence of the roughness sub-layer, I calculated the dashed lines within the following figure for the wind profiles above the canopy.



The straight lines in the background show the LES results, i.e. a copy of Fig. 4a. Did you fit Eq. 1 to the profiles shown in Fig. 4a to derive the results in Table 1? If yes, what are the reasons for the differences in the figure above?

In order to be most realistic and consider times and cases we're interested in, we performed daytime simulations, which took place typically under unstable conditions rather than neutral, and as such the atmospheric stability corrections from equation 1 become important. Our simulations had Obukhov Lengths in the range roughly of -10 to -20 reflecting the unstable conditions. As an example, I have plotted a few u/ustar profiles according to the theoretical equation 1 including the stability corrections – the LAI = 1, 2.6, and 3.7 using values from Table 1, and the black line, z0 = 3, d = 15 the values mentioned above. This brings the values much closer to the range reflected in figure 4. Additionally, the figure focuses on the changes to the wind velocity profiles inside the canopy and ends at 50 m (~2h). This is without the roughness sublayer, were the velocity profile is expected to depart from M-O theory. The roughness parameters were fitted based on values of the wind profile further up 2h-5h).

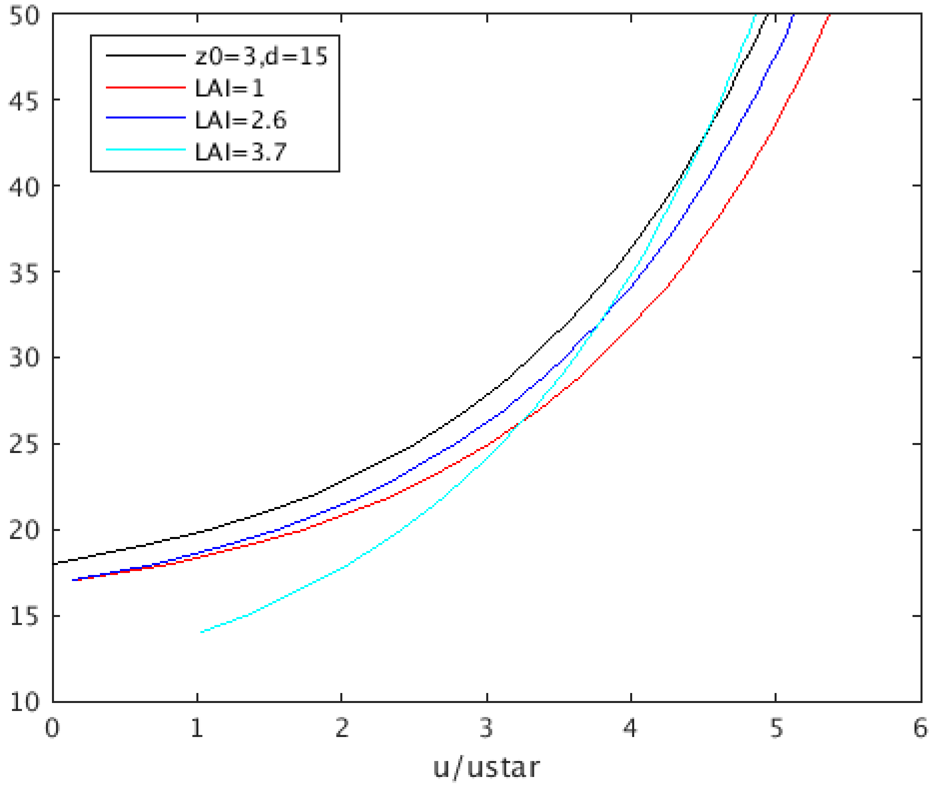


Fig. 4b, 5b and 7b: How do you explain the differences between lines of the normalized momentum flux within the inertial sub-layer (above ~50 m)? Shouldn't they collapse to one line, i.e. the value -1 there?

They do collapse to nearly the same values (but not exactly, there is heterogeneity in the domain, and turbulence is chaotic) further up.