Response to reviewer's comments

Both reviewers have provided very thorough and detailed reviews and help us to significantly improve than manuscript.

Overall, we have made a very thorough and extensive revision of the manuscript. We have re-did the analysis for the model evaluation section by extending the period and the atmospheric conditions used for testing the results of the different parameterization approaches.

We have added an analytic sensitivity analysis of z0 as a function of u and of u* as a function of z0. This helped explaining why we got such varied results for z0 from different parameterization approaches, while getting very little effect on the modeled u* that was calculated from these parameterization approaches.

We added the results of an explicit LES simulation of our forest site with lidar-observed canopy. We also conducted an analysis using fixed d (following the classical approach and an approach suggested by reviewer 1, and then, independently, fitting z0. This did not help constraining z0 and we chose not to add it to the manuscript, though we show the results in the response to reviewer #1.

We thoroughly revised the results, discussion and conclusions sections, to reflect the additional results we report and their meaning.

We revised the formulation and added details to our explanation of how simulation results were processed. We revised the language and style of the writing.

Below, we have listed each review comment, in blue italic font, followed by the response and description of revisions done to address this comment, and in most cases, followed by the revised version of the modified section (sentence, paragraph or figure) from the revised manuscript.

Response to review by Ronald Queck (reviewer #1).

General Comments

Representation of natural forest canopies within numerical models is an actual problem within the science community as it limits the accuracy and the applicability of their results.

The authors investigated the effect of changes in canopy structure using large-eddy simulations (LES). From the LES results they inferred statistical relationships between measurable canopy quantities and wind profile parameters (displacement height d and roughness length z_0). This approach is new and the well-defined changes within the canopy characteristics allow insight in the dependencies of the wind profile parameters.

Further, existing approaches for d and z_0 from literature where applied to calculate the wind profile parameters from canopy quantities.

The different d and z_0 estimates where then used within the logarithmic wind profile equation for the calculation of friction velocity (i.e. the momentum transport between surface and *atmosphere) using measured wind speeds and stability parameters.*

These calculations where again validated with direct measurements of the momentum transport.

We thank the reviewer for this supportive comment on our approach.

The authors found a dependence of the wind profile parameters on the maximum canopy height, leaf area index and gap fraction. However, changes of the vertical plant surface distribution resulted in an inconsistent variation of d and z_0 but also in incomprehensible changes of the simulated wind profiles.
As the authors state themselves, this is obviously caused by the interdependence between d and z_0.

Prodded by the reviewer's comments we put some additional work and analysis into this matter. As you will see in our response to the comment below fixing d and fitting only z0 to the resolved wind simulation results did not improve the relationships we find between z0 and canopy structure parameters, and in fact, it had very little difference altogether.
We no longer believe that the z_0-d tradeoffs are the only source of the problem of inconsistent z_0-canopy structure relationships. To understand this point, we conducted a sensitivity analysis by calculating the partial derivative of z_0 with respect to the mean wind speed (<*u*>), and the partial derivative the u_* with respect to z_0. The partial derivative is a direct measure of sensitivity of one variable to errors in measurements or inference of another. We determine that at low mean winds (below 3 m/s) z_0 is extremely sensitive to variation in <u>, with the derivative (the rate of change in z_0 per change in <*u*>) being between 5 and 30 (see Figure 1, below). It means that for a variation of 0.1 m/s, the resulting z_0 (calculated from measurements at twice the canopy height) will change by 0.5-3 m which is a full range of the expected z_0 values for a 20 m canopy. At twice the canopy height <*u*> was less than 3 m/s roughly half of the time in our site in Michigan, and our simulations were set for <*u*> of 2.7 at that height. A variation in half-hourly mean wind speed at the order of 0.1 m/s can be a result of measurement errors, or local variations in the flow field due to topography (in reality) and canopy surface (in reality and the model) or even random large eddies that may affect the 30 minutes average at a specific half hour. However, and consistent with the results of our model inter-comparison section, $u*$ shows very little sensitivity to changes in z_0, when z_0 is above 0.5 m (assuming z0 ≈ 0.1h, the expected z0 for a canopy of h=22 m is around 2.2 m, well above 0.5 and well within the range where $u*$ is insensitive to z0). This explains why, despite the lack of a satisfying model for z_0 as a function of canopy structure, all the models we tested against observations showed high levels of precision in predicting $u*$.

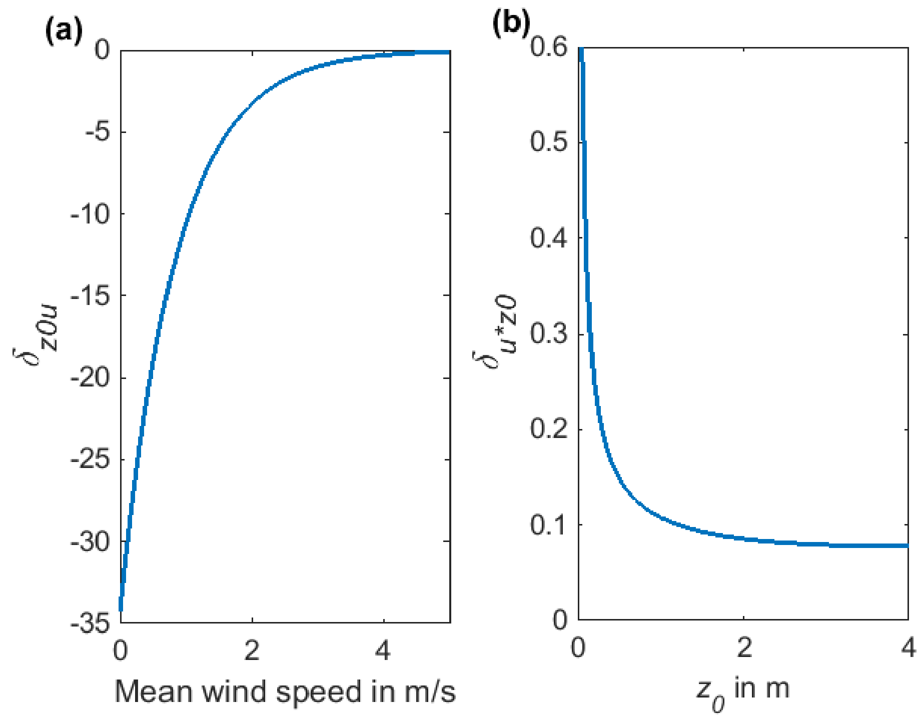

Figure 1. (a) Sensitivity analysis of z_0 as a function of variation of the mean wind speed (δ_{zou}). We illustrate it here is a particular range of parameters, choosing a canopy height h=22 m (roughly the height we used in the simulation and observation site), displacement height d=0.67h, observation height of 2h (the recommended observation height for a flux tower) and u_* of 0.35 m/s. The results are similar for other canopy heights and u_* values. (b) Sensitivity of u_* to variation in z_0 (δ_{u*z0}). We plotted the response curve over the same parametric range expected for z_0 values, wind speed at the center range of 3 m/s. u_* is relatively insensitive ($\delta_{u*z0} < 0.15$) for any z_0 above 0.5 m.

Unfortunately, they did no deeper investigation of this problem. This may be caused by their indifferent consideration of d and z_0. Both of the parameters were named 'roughness parameters'. However, only z_0 represents the roughness of the canopy, differently, d is introduced in the wind profile to reduce the height above the ground z, as z is used as a scale for the mixing length $l=\kappa(z\text{-}d)$.
In many case the fitting of d and z_0 at the same time leads to tradeoffs. The determination of one parameter first, with an independent method, and calculation the other afterwards on the basis of the wind profile circumvents the problem. For example estimating d based on the canopy structure (i.e. depending on the canopy height or the gap width between the vegetation elements) would probably lead to a more reasonable behavior of z_0 within the presented work. If you want to use only wind measurements you can also use the methods described in Rotach (1994) or De Bruin and Verhoef (1997) to determine d.

We acknowledge the fact that setting *d* as a function of canopy height, and independent of z_0, is a common practice. We thank the reviewer for the suggestion of studying how z_0 changes when d is assumed fixed. We hoped that this analysis approach would allow us to improve the empirical model of z_0 as a function of canopy structure. We therefore followed the reviewer's recommendation and reanalyzed our results with an additional approach of fixed d. We set *d*=0.67*h*, and fitted the M-O equation for z_0 only, given <u>. The results, however, were disappointing, and were very similar to the results obtained with a variable *d* (see figure 2 below). This made us reevaluate our hypothesis that the tradeoff between z_0 and d drive the large variation in z0. As we show above, we find support for an alternative hypothesis: that the sensitivity of z_0 to <u> is responsible for the large variation in z_0 (see the results and explanation of the sensitivity analysis above). We will add the results of the sensitivity analysis to the discussion section in our revised manuscript as it shed further light on our findings, and on the prospects of finding an accurate empirical model for z_0. To add depth and interest, we also added the results of an additional simulation case – the Explicit-LES where we prescribed the canopy structure as observed by lidar and calculated the d and z_0 directly from the simulation results. We expanded the set of conditions and period over which we modeled u_* and the different approaches now show more differences in their ability to predict u_*. We rewrote the discussion section and the result sections completely, and are confident that the revised discussion holds more depth.

With regards to the collective naming of d and z_0 as roughness parameters: to some degree, one may argue that z_0 and d are two shape parameters of a single curve. Observations in the real world have shown that they tend to trade off, even when obtained as the distinct (and independent) solutions from observations at multiple heights (see Nakai et al 2008 and we have similar observations in our site). Nonetheless, we agree with the reviewer that they may be considered separately. We will revise the language in our manuscript to better distinguish them and clarify that they are not one exchangeable entity.

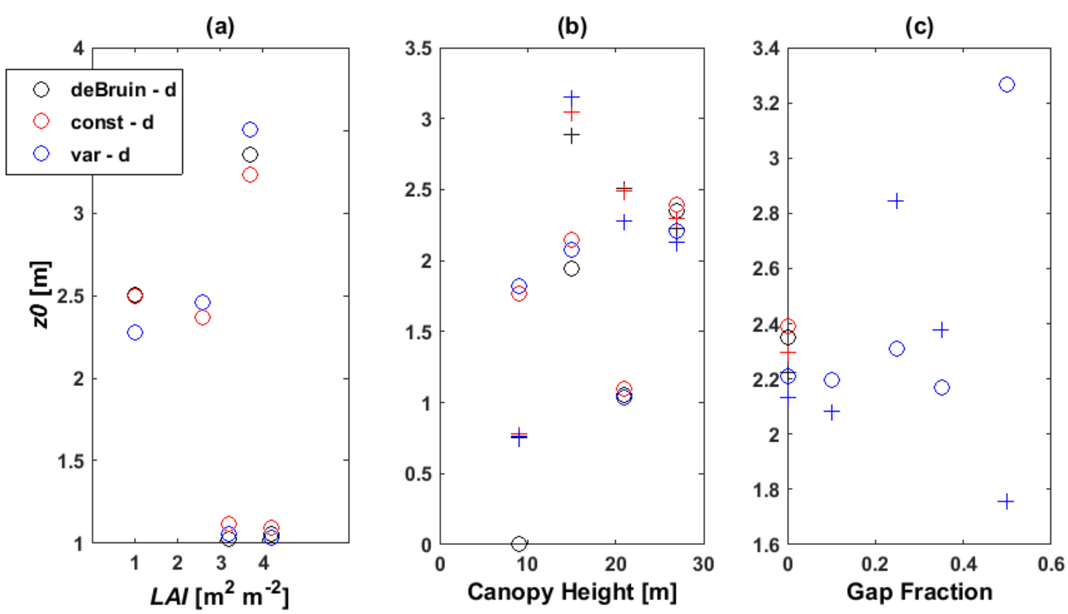

Figure 2. The relationships between z0 and canopy structure characteristics ((a) LAI, (b) Canopy Height, (c) Gap Fraction). Z0 was calculated in 3 ways – (1) 'var –d' – is the one presented in the manuscript and done by fitting MOST to the wind profile; (2) const-d'- following the reviewer's suggestion, fixing d at the 'Classic' 0.67h and fitting MOST to find z0 only; (3) 'deBruin-d' – also following the reviewer recommendation, using the approach by deBruin to determine d, independently of z0, and then fitting MOST to find z0. Crosses mark the results of leaf-off simulations and circles are the leaf-on simulations.

The wind profiles in Figures 4-6 are not easy to compare, as they were not normalized with the wind speed at the top of the canopy or the wind speed at a reference height within the inertial sub-layer. Moreover, the figures indicate a different wind speeds within the inertial sub-layer far above the canopy. Thus, the different LES are probably not really comparable.

The LES is forced by nudging the mean wind speed high above the canopy (from the model to about 5*h*) to a prescribed velocity that was the same for all simulations. Any differences in the wind profile below that forced layer height is the result of differences in surface roughness. All simulation cases are directly comparable, and in fact, we are confident that the approach we took for forcing and canopy description in the model made special considerations not to introduce any implicit or explicit assumptions of roughness length, such that any conclusion of roughness length differences between the models is an outcome of the canopy structure and not a by-product of the forcing. We chose the height of the forcing layer to be high enough such that the forcing will not directly affect the results. The exact height of the inertial sub layer is not easy to define, and as Raupach and Thom (1981) suggested (and Bohrer et al (2009) confirmed with LES simulations) may extend up 5 times the canopy height.
As the reviewer points out, we did make a mistake in the presentation of the profiles by not normalizing them. We now present the wind speed profiles and the $\langle u_r'w'\rangle$ profiles normalized by u_* or u_*^2, respectively, and by extending the height up to which we show the profile. As you can see in the revised manuscript figure 4 (which is copied to this response document as Figure 3, below), this has collapsed the curves and made the results of different simulations directly comparable. We also revised the color scheme to be consistent (low-to-high) across all canopy structure variables in figures 4-6 in the manuscript.

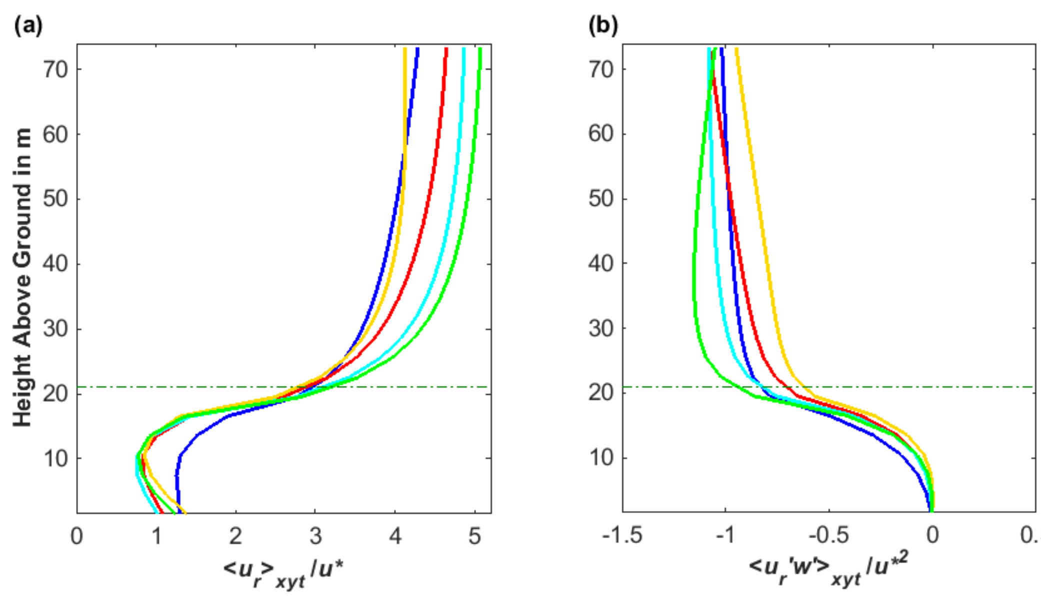

Figure 4. Vertical profiles of: ***(a)*** *mean downstream horizontal wind speed* $\langle u_r\rangle_{xyt}$ *normalized by u* and* ***(b)*** *momentum flux normalized by* $u*^2$*; for LAI=1.0*m^2m^{-2} *(blue), LAI=2.6*m^2m^{-2} *(cyan), LAI=3.2*m^2m^{-2} *(green), LAI=3.7*m^2m^{-2} *(orange), and LAI=4.2*m^2m^{-2} *(red). Canopy height shown as horizontal dashed dark green line.*

The conclusions of the authors are not very productive. The authors state 'consistent relationships between roughness parameters and LAI, maximum height, and gap fraction' (p16371L9). Which is, at least partly, contrary to statements within the document (p16368L3: However, the lack of any relationships between roughness parameters and gap fraction was surprising).
Despite the improvement of the correlation between d and canopy height by the use of the maximum tree height instead of the mean tree height, the general performance of the inferred statistical relationships was not better than the approaches from literature (Raupach 1994).
The inclusion of the vertical plant area distribution seems to produce also no improvements. Thus, the main conclusion which remains is: It is very difficult to determine the influence of canopy structure on the wind profile parameter.

The revised results and discussion sections include better explanation of the advantages and disadvantages of each method and provides more constructive conclusions. While 'Raupach 94' came out the most accurate method, it requires full plot censuses which are not available over wide spatial coverage and, therefore, could not be implemented in a general global modeling approach. Our approach requires only LAI and gap fraction which are available world-wide from satellite datasets such as LANDSAT and MODIS, and canopy height, which is commonly predicted by models, can be measured by lidar and is already the source of roughness length estimates. We also show that the model results for u_* (and therefore, surface fluxes) are not very sensitive to interannual variation in canopy characteristics, which is an important and surprising conclusion.

Some of the methods and the results are poorly described. For example it is not clear how the 'Yearly Observed' method works and how the results are gained. The calculated d and z_0 of the 'Raupach 94' and 'Nakai 08' approaches are not reported.

We re-wrote the entire results section to better explain what we did, and how these methods compare to each other. Except the 'Classical' and 'yearly Observed' of specific years, all other approaches (including 'Raupach94' and 'Nakai08') produce variable z_0 and d that are dependent on the observed properties of the canopy at different times throughout each season and year. Therefore, in the previous version we chose not to provide a single value. We have now revised the table to provide the decadal mean, and extremes of z_0 and d for all the parameterization approaches and explain that in the table's caption.
The revised section that explains the different methods is now section 3.2 in the 'Results', just after the derivation of the 'Biometric' approach from the simulation results is explained.

The language of the manuscript could be improved by the use of shorter sentences, often different statements are linked together without break (e.g. P354L15-20, P360L20-23 and P363L3-10). Several parts need a revision with respect to concise and precise use of formulations (e.g. P16362L1, where the authors compare an improvement of something with a result of something).

We have revised the language of the manuscript, with specific focus on breaking long sentences to short ones and creating a clearer and more concise narrative. A language editor (Ashley Matheny) has read and proofed the revised version of the manuscript. A great resource for this revisions are the suggestions of both reviewers, which will be implemented in full. We will revise the formulation to be more precise and better explained (see our response to reviewer #2, as to clarifications of the formulation used to measure the mean properties in the simulation domain, such as u_r, and $w'u_r'$).

The content of subsections 2.6 and 2.7 belong either to subsections "Theory" or "Results" (the statistical methods must not be listed explicitly as far as they are standard methods).

We have removed section 2.6, and list the relevant information from there in the results section. We have moved section 2.7 to the "Results" section (new section 3.2).

Title/Abstract

P350L21: 'We compared it with three other semi-empirical models …'. Is LES a semi- empirical model?

We removed the term 'semi-empirical'

P350L23: 'fixed representations of roughness' please clarify what is meant by that phrase, or better reformulate the last two sentences.

We revised this to 'temporally invariable'

1 Introduction

The introduction could be written more concise and focused.
P352L4: The displacement height d is not a 'surface roughness parameter for momentum'
P354L15-20: Split the sentence.

We shortened the introduction by removing several redundant sections, and split this sentence, and many others.

2 Materials and methods

2.1 Theory

P356L21-22: Up to this line it is not clear how ha, d and z0 will be determined. The reader does not know what is 'simulation-specific'. "the horizontal wind profile": presumably the "vertical profile of the horizontal wind speed" is meant (see also P360L21, …).

We revised to: "In simulation results, where the detailed 3-D wind field is known, we can use Eq. 7 to calculate λ for each simulation using h_a, which can be calculated from the vertical profile of horizontal wind speed and the empirically fitted d and z_0."

*P356L3: Check the indices (uw = 0.1 * uw_ha ?)*

We removed this formulation and replaced it by the sentence: "We investigated the eddy penetration depth (δ_e), which is the length scale describing the vertical distance from the top of the canopy that is influenced by turbulent mixing from above. It is defined as the distance between h_a and the height where the momentum flux value is only 10% of its value at h_a (Nepf et al., 2007)."

2.2 Site description

2.3 Large eddy simulations

2.4 Virtual experiment setup: sensitivity analysis to quantify the effects of specific canopy-structure characteristics on roughness parameters

*P359L24: '**these** structural characteristics' Which? Please describe or name them.*

Done

P359L24: 'surface-aerodynamic properties' → 'aerodynamic properties of the surface'

Done

P359L27: 'that describe such canopy structure.' → delete or exchange with 'that characterize such canopy structure'

Exchanged to 'that describe such canopy structure'.

P360L1-6: Combine both of the lists (maybe as table)

We combined the lists, reduced the values to a range, and refer to the full list in Table 1.

P360L15: 'Changes along the four canopy-structure axes yielded twenty permutation cases.' - The permutation gives 400 cases!

We added an explanation that: "Based on the available computing resources, we selected twenty combinations of the structural characteristics listed above."

2.5 Empirical determination of roughness parameters from simulations results

P360L20-23: split sentence in two or shorten to: 'To find this point we compiled a domain averaged wind-speed profile using Eq 2.'

Done

P360L22: 'h_r' → 'u_r'

Done

P360L22: 'vertical layer' - layer or column?

Layer. We revised the description of how we calculated u_r. See the revised section 2.5. We think it is clearer now.

P360L20-23: 'As RAFLES was able to estimate wind statistics across a large domain,' - delete *or explain the function within this sentence.*

We removed this sentence.

P360L24: 'we fit the wind profile in space' - delete 'in space'

Done

P360L26: How did you interpolate between the profile functions? Did you use linear interpolation, spline or … (that is important for the position of inflection point.

New explanation reads: "To find this point we compiled a domain-averaged wind-speed profile using Eq. 8. Then, we determined h_a as the location where the second derivative of the horizontal wind profile crosses zero. We approximated this location within the vertical grid resolution using linear interpolation. "

P360L27: 'found the height above the ground' → 'determined h_a'

Done

P361L6: 'vertical layer' - layer or column?

Layer. However, we call them vertical layers, and they are, in fact, horizontal layers. We revised throughout.

2.6 Surface roughness parameters: forest structure effects → Sources of variation of wind profile parameter

P361L6: 'LAD position' → 'LAD distribution'

P361L6: 'chaotic' → 'not explainable'

P361L17-20: These are results which do not belong in 'Materials and methods'

We removed this section

2.7 Testing empirical models linking roughness parameters to biometric measurements

Please write clearly and in an easy accessible manner which models and methods did you apply.

P361L25: How did you evaluate the potential improvement of the surface flux estimates? The '(a)' belongs presumably to a 'not easy to follow' list which is continued at P362L5. Please use a clearer structure. Reformulate and split the sentence. One cannot compare an improvement of something with a result of something.

P362L5-18: This part shout be shifted to subsection 'Theory'.

P363L0-10: Reformulate and split the sentences into shorter ones.

*P363L4: What are the "**four** other direct empirical methods"?*

P363L7: Describe the "Yearly Observed" method.

P363L11: Did you force the regression trough zero? Otherwise report the offset values.

P363L17: I assume you did apply the yearly parameterizations of the "Yearly Observed" on *the whole data set (all 10 years). Why did you use the parameters of structure-driven methods only for one year (P363L20)? This is inconsistent. Further, it would be interesting how long the parameters, which are gained from biometric measurements, can be used.*

P363L19: Delete 'Sect. 2.7,' as this is Section 2.7.

We completely rewrote this section and moved it to the results, section 3.4. We also revised table 3, and report the intercepts of the regression lines.

3 Results

3.1 Virtual experiment to explore canopy-roughness relationships

P364L9: write '$d = 0.69h_{max}$' in Equation stile, thus you can refer on this result later on.

Done

P364L12: 'There was little change to d with increasing gap fraction'? Table A1 showed an decrease of d of almost 30 % (from 20.1 m to 14.4 m) as a result of the 50 % increase of the gab fraction.

We revised to: "There was little change to *d* with increasing gap fraction, except with 50% gap fraction in the leaf-on simulations, which was significantly lower. "

3.2 Canopy-roughness improvements to surface flux models

What are 'Canopy-roughness improvements'?

Revised to "Improvements to estimates of friction velocity using canopy-structure-roughness relationships"

P365L7: 'fit' → 'fitted

Corrected throughout

P365L13: As far as I understood the setup, the h-h_{max} relationship is not 'found from the virtual experiment' it is given by the virtual canopies with h_{max} and GF.

We revised and cleaned up this section. It now reads: "We calculated a 'Biometric' h_a using the relationship we found in the virtual experiment between h_a and *LAI*, gap fraction and h_{max} (Eq. 12), we calculated the 'Biometric' h_a. To simulate the conditions in our site at US-UMB, we assumed a gap fraction of 5%, which was found by calculating the percent area within the NCALM lidar scan domain with vegetation less than 2 m. We used the peak growing season, site-level, mean *LAI* of 4.2 as measured from 2000-2011 (Maurer et al., 2013). A 'Biometric' *d* was then calculated using the Eq. 10. Finally, a 'Biometric' z_0 was calculated as:

$$z_0 = \lambda(h_a - d) \quad (13)$$

where λ = 0.34 was determined from Eq. 7 given the set of h_a, *d* and z_0 values from our simulations through the virtual sensitivity experiment.

P365L13: 'we empirically fit' → 'we calculated'

Done

P365L13: 'Eq. (11)' → 'Eq. (12)'

We revised equation references throughout.

P365L13-17: These lines are not comprehensible.
P365L20: Regard comment on P365L9, delete Eq. 14 and refer to 'd = 0.69hmax' as well as to Eq. 7 for z_0.
P365L21: How did you exactly determine λ.

See revised section above that addresses all these issues.

P366L3: SD is not introduced.

We removed this part and completely rewrote section 3.3

P366L9: 'Eqs. 11, …' →'Eqs. 12, …'. What influence has Eq. 13?

We revised equation references throughout.

4 Discussion

4.1 Response of roughness parameters to canopy structure change

P366L15-16: Emphasize that this are model results gained by the use of artificial canopies. Different relationships of h_a are possible for real canopies. Contrary to your statement, Table A1 shows that h_a is rarely sensitive to canopy structure, i.e. LAD profile variation (lower: h_a=20.7 m to upper: h_a=21.2 m).

We revised the discussion section thoroughly. Please check the revised version. It answers all of the comments and concerns indicated here. Specifically for the statement above, ha was sensitive to all canopy structure characteristics, other than LAD. We revised this sentence (now in the second paragraph of section 4.1 to read "By testing the independent effects of different characteristics of canopy structure through a set of controlled virtual experiment, we indeed found that different roughness parameters where sensitive to different structural characteristics. The aerodynamic canopy height (h_a) and eddy penetration depth (δ_e) where both sensitive to some of the characteristics of canopy structure and linearly scaled with leaf area, canopy height and gap fraction (figure 2,3). In contrast, *d* was only significantly sensitive to canopy height, while z_0 did not show any significant relationships with any single canopy structure characteristic. "

P367L10: d and z_0 are canopy parameters; they do not change with meteorological conditions, at least as long as the properties of the canopy are not influenced. However, the *estimation of d and z_0 might depend on the meteorological conditions.*

We agree and removed this paragraph.

P367L22: 'roughness-height'? Did you mean 'roughness-canopy height' (i.e. z_0-h)?

We revised to "As canopy height was the only canopy characteristic that varied among the 'canopy height variation' simulations (Table 1.c.), it is reasonable to assume…"

P367L28: How does the eddy-penetration depth influence the determination of d?

It brings fast eddies lower into the canopy. By definition, *d* is the height were mixing with the air above has only negligible effect on momentum flux.

P368L3-4: Table A1 shows a clear relationship between GF and d or d+z_0 (see comment on P364L12)

We restricted this claim to z_0 only, and the effect of GF is only apparent at the extreme point of the GF continuum (50%). No effect until then. We revised to: "However, the lack of any relationships between roughness length and gap fraction at all levels below 50% gap (*Table 1*) was surprising"

P368L15-18: Repetition of P367L1..

Fixed

P368L21: I cannot identify a 'weaker above-canopy turbulence and horizontal wind speed' within Fig. 5a and b. This might be caused by different wind speeds within the higher model layers.

We removed this statement

P368L24-27: This statement is very general, i.e. trivial.

We removed it.

P368L18-21: Please, reformulate this sentence.

Revised to: "For example, we found that increased gap fraction corresponded to increased momentum flux, turbulence, and horizontal wind speed inside the canopy (below 1*h*) (Figures 5, 6). This was likely due to the extension of turbulent eddy penetration deep into canopy gaps, indicated by elevated standard deviation of the vertical velocity, σ_w (a component of the turbulence kinetic energy) in canopy gaps (Figure 6a)."

P368L18-24: This is a conclusion, which is not so clearly stated within section 'conclusions' (shift it).

We removed it.

4.2 Integrating canopy-structure characteristics into flux models

P370L1-9: As the ''Yearly Observed' model' is not described these statements cannot be evaluated.

Section 3.2 now provides clear and detailed explanation of all parameterization approaches.

P370L12: What is meant by 'surface height, complexity, and density'(especially by the last to terms)

Revised to "complexity of organization, and density of roughness elements"

P370L10-19: What do you want to say exactly within this paragraph? Especially by the sentence: 'In their urban study of building heterogeneity, Grimmond and Oke (1999) suggested the method of Raupach (1994) for random building arrangements, which may provide insight towards its success in this study over our heterogeneous forest canopy.'

We removed this section.

P370L25: 'used' → 'taken'
P370L26: 'each' → 'any'

Fixed

5 Conclusions

P371L11-22: Those are general statements but not unique conclusions from your work.

The conclusions section was rewritten, in light of the analytic sensitivity analysis results, and the new approach for the discussion. It is now more interesting and meaningful.

Tables

Table 1 is not necessary, it is part of Table A1.

We removed table A1 and used it to replace table 1.

Table 3: Why has the coefficient of determination always the value 0.8?

Bug. We fixed it. See new table 3.

Figures

The axis labels of the kind 'd [m]' are common but mathematical incorrect. Please write 'd in m' or 'd /m' or 'd m^{-1}'. Variables should be written in the same style as in the text (kursiv, …)

We revised to the format: 'd in m'.

Fig. 1: Please add a 1:1 line

Fig. 2: Please add a 1:1 line in Fig. 2b
Done.

Fig. 3: y-axis label: 'd_e', use the Greek letter or write 'eddy-penetration depth', please add a 1:1 line in Fig. 3b
Done

Fig. 4: Use the same order of the colours in Fig. 4 and Fig. 5

Done

Fig. 5: Explain in the figure caption, what is exactly shown in 5c, d, e and f? If 5d shows the case with GF = 0 % then use the same colour as in 5a and 5b or better leave it out. SD is not introduced. The x-labels of 5b are not readable

Fig 5 was split to 2 figures and the 4-panel part was merged to 2.
Caption and labels were revised. We hope it's clearer now.

Fig. 6: Use the same colours like in Fig A1.

Done (all color schemes were made uniform)

Response to review by Reviewer #2

I. General Comments

Parameterization of the momentum exchange between plant canopies and the atmosphere is of great importance in regional and global weather, climate and ecosystem models. The authors used large-eddy simulation (LES) to investigate the effects of four axes of canopy structures on the estimates of roughness parameters. These axes are (1) leaf area index (LAI), (2) vertical profile of leaf area density, (3) canopy height, and (4) canopy gap fraction. Results were compared with existing empirical models and evaluated against observations. These results are interesting and constructive. However, the manuscript needs substantial revision for publication.

Firstly, the authors should be careful about theories and concepts. For example, some statements of MOST were misleading (P16351, L15-16). The canopy roughness sublayer and the layer directly above rough surface are not identical (P16351, L16-17). The use of higher-order closure model is not only caused by the failure of MOST (P16351, L19). The definition of h_a *does not agree with what we observe in Figs. 4, 5 and 6.*

Secondly, the authors should provide more in-depth discussion for scientific methods, especially those in subsection 2.6. What are the most critical differences among these methods? What are the pros and cons for each method? What do we expect to see for the results? I suggest create a Table to compare these methods. The linear fitting used for Fig. 1 also needs justification. For results shown in Figs. 4, 5 and 6, the reason why Reynolds stress at the canopy top varied by a factor of 2 should be examined and explained.

Thirdly, the manuscript needs to be restructured. For example, section 2 is very long and goes to a lot of directions. It should be divided into at least two sections, one for numerical simulation only and the other for observations and empirical models. Specifically, subsections 2.3 — 2.6 can form a section "Large eddy simulations". The contents for these subsections should be restructured as well. Subsection 2.3 is a mix of model description and model setup. Subsections 2.4 and 2.6 have repetitive contents. I suggest restructure the subsections as "Model description", "Simulation setup" and "Determination of roughness parameters from simulation results". Another important issue is the missing of highlights of important results in the conclusions.

II. Specific Comments and Technical Corrections Abstract

A. P16350, L1-9: There three sentences give motivation rather than an overview of a paper. They should be shortened to one sentence that occupies two lines at most.

Done as requested. The new section now reads: Surface roughness parameters, namely the roughness length and displacement height, are an integral input used to model surface fluxes.

However, most models assume these parameters to be a fixed property of plant functional type and disregard its structural heterogeneity and dynamics.

B. P16350, L20:

a. What does "our model-resolved parameters" mean?

We revised this statement to: "our model-driven 'biometric' parameterization approach".

b. "Frictional velocity" and "friction velocity" were used alternatively throughout the manuscript. Please use the standard term "friction velocity" consistently.

Changed to 'friction velocity' throughout the manuscript.

C. P16350, L21-22:

a. Does "it" mean "friction velocity" or "our model-resolved parameters"?

We revised this sentence to: "We compared the accuracy of these predictions with the friction-velocity predictions" (with the former "it" meaning the accuracy of the predictions of u*)

b. What are the most important differences between "our model-resolved parameters" and "three other semi-empirical models"?

We added "that utilize varying canopy-structure inputs,... from meteorological observations".

D. P16350, L23: Which models used "parameterizations with fixed representations of roughness"?

We revised that to: "We found that the classical representation of constant roughness parameters (in space and time) as a fraction of canopy height performed relatively well. "

E. P16350, L24-25:

a. What are "some empirical approaches"? What models used "some empirical approaches"?

b. In what aspect and to what extent did "some empirical approaches" performed better?

To answer both points above, we clarify this sentence to "Nonetheless, of the approaches we tested, most of the empirical approaches that incorporate seasonal and inter-annual variation of roughness length and displacement height as a function of the dynamics of canopy structure produced more precise and less biased estimates for friction velocity than models with temporally invariable parameters." There is no space in the abstract to list and explain the different methods, we explain them in the results section.

1 Introduction

A. P16351, L15-16:

c. "MOST is expected to be accurate in the inertial sublayer": This statement is wrong, because MOST is an approximation based on certain assumptions.

d. "High above the ground surface in the inertial sublayer": This statement is misleading. ISL does not include all regions "high above the ground surface".

e. "The viscous effects of the rough underlying surface may be neglected" (in the ISL): This statement is wrong, because ISL is defined for wall-boundary-layer flow, where the flow is strongly affected by the wall.

f. "The vertical flux of momentum is constant" (in the ISL): This statement is wrong. The vertical flux of momentum is not constant.

B. P16351, L17-21:

a. "The rough surface" usually indicates a rough wall. Is the layer directly above the rough wall identical to the layer "near forest canopies"?

b. Please clarify the reason why MOST is inapplicable to the canopy roughness sublayer.

c. Please clarify the reason why higher-order closure models were used for the canopy roughness sublayer.

We removed the entire section referred to in comments A.c-f and B.a-c above. These statements relate to the particulars of the formulation used to determine the roughness parameters from observations of wind speed in the forest site and in the model. It is better explained around eq. 5 in the methods section, where the RSL correction term is described.

C. P16354, L4-8:

a. "Unlike most LES": This statement is misleading, as if there were very few existing LES studies resolving the canopy layer.

b. "RAFLES does not use a prescribed 2-D roughness length": This statement is misleading, as if models using roughness-length parameterization and models resolving the canopy layer were designed for the same research objectives.

c. "RAFLES incorporates leaf-level drag heterogeneously in 3-D and dynamically in time": This statement is misleading, as if RAFLES were resolving leaves and having these leaves interact with the flow dynamically.

We could argue that there are only a handful of LES that prescribe 3-D vegetation domains and do not use a vegetation-roughness surface boundary condition. But we would rather not argue about that and have removed that statement. The 'dynamic' part relates to the drag force. With the leaf surface area and stem surface area in each voxel are constant, the drag force is a dynamic function of wind speed and surface area roughness. The new sentence does not state anything about other models, and better explains how vegetation interacts with the flow in RAFLES. It now reads: "RAFLES incorporates a prescribed 3-D domain that includes the vegetation leaf density and stem diameters and dynamically calculates the change to wind velocity as a function of leaf and stem surface drag in each voxel".

2 Materials and methods

2.1 Theory

A. P16355, L1: Why is there a second title "Parameterization of aerodynamic canopy properties" for this subsection? Should it be combines with "Theory"?

Corrected as suggested.

B. P16355, L2-15:"MOST describes the relationships between the mean horizontal wind speed and the friction velocity at all heights within the atmospheric surface layer": This statement is misleading, because MOST was originally developed for inertial sublayer only.

We revised "all heights" to "the inertial sublayer".

a. "The friction velocity is a property of the turbulence of the flow": This statement is vague. What specific characteristic of turbulence does friction velocity measure?

We removed this statement. Friction velocity is a basic property and does not need to be explained. In any case, the exact formulation of frictional velocity is listed in equation 3.

b. "MOST relates surface stress to d and z0": This statement is vague. What does it mean by "relates"?

c. Eq. (1): This is not the original MOST, because MOST was developed for inertial sublayer only. Please clarify that this is a modified version that accounts for the canopy roughness sublayer as well. What are the assumptions associated with modifying MOST to account for the canopy roughness sublayer? How accurate is this equation?

To address comment (c) and to acknowledge that we used a modified version of MOST, we revised this sentence to "MOST describes the functional relationship between surface stress to *d* and z_0 and wind speed using a logarithmic function. The original MOST formulation was expanded to include the effects of thermal instability and the flow regimen in the roughness sub-layer (RSL). The formulation we used was:"

A reference to each correction term is provided next to the equation that describes it (Eq. 4-5). The evaluation of accuracy of the particular formulation was provided with these original papers that introduced these correction terms. These are, however, standard and very commonly used formulations, and we do not think that it is within the scope of this manuscript to further validate them. In theory, one would need an indented method (that does not rely on relating z_0 or *d* to wind speed) to measure roughness length and displacement height in order to provide a true validation for any of these formulations. As far as we know, this is not technically feasible in forest environments. But again, this is way beyond the scope of this manuscript.

C. P16356, L8-9:

a. "When conditions are neutrally buoyant": This statement is potentially confusing, because "neutrally buoyant" is also used to describe materials having the same density as the carrying fluid.

b. "When conditions are neutrally buoyant, $\Psi_m(x)$ becomes negligible": This statement is misleading, because by definition $\Psi_m(x)$ is zero for neutral conditions.

In the context of boundary layer meteorology and large eddy simulations, 'neutrally buoyant' is a well-defined and commonly used term. In any case, we changed it to 'boundary layer conditions are near neutral'. Adding 'near neutral' rather than neutral balances the 'approaches 0' which is now a correct statement.

D. P16356, L22: "h_a calculated from the horizontal wind profile": This statement is vague. The determination of h_a was restated on P16360, L18-19. This is difficult for readers to follow. See Comment #13 for additional issues associated with h_a.

This was a typo, and we mixed 'horizontal' with 'vertical'. We fixed that, and further clarified this sentence to read: "In simulation results, where the detailed 3-D wind field is known, we can use Eq. 7 to calculate a simulation-specific λ values using h_a, which can be calculated from the vertical profile of horizontal wind speed and the empirically fitted d and z_0."

E. P16357, L4-6: It sounds like that u_r' is calculated as $u_r' = u_r - \langle u_r \rangle$, where $u_r = (u^2+v^2)^{0.5}$. If this is not the case, please clarify mathematically how u_r' was calculated. If this is the case, please explain the physical meaning of u_r'.

This is not exactly the case. u_r is the downstream velocity on a streamwise-rotated coordinate system such that:

$u_r' = u_r - \langle u_r \rangle$

and $u_r = u\cos(\theta) + v\sin(\theta)$

where $\theta = \mathrm{atan2}(v,u)$; and $\langle u_r \rangle = (u^2+v^2)^{0.5}$

This is a common practice in micrometeorology, but not often done in LES (though it should be done in order to compare LES results to observations if the mean flow is not perfectly towards the same direction at all levels).

We added the following explanation (in section 2.5 of the revised manuscript):

"To calculate flux and wind statistics, we first calculated the mean value of each model variable at each vertical model level over the entire horizontal domain at that height level, and over all 300 time snapshots. We then rotated the horizontal wind coordinates of each vertical level toward the downstream direction, such that the resulting mean rotated downstream velocity is

$$\langle u_r \rangle_{xyt} = \left(\langle u \rangle_{xyt}^2 + \langle v \rangle_{xyt}^2 \right)^{1/2} \qquad (8)$$

where $\langle \ \rangle_{xyt}$ marks an average of the simulation results over all voxels in the x (eastward) y (northward) and t (temporal, 300 snapshots) dimensions. Though the wind forcing aloft is eastward a rotation develops, following the Ekman spiral and further amplified by random x-y asymmetries in the simulation domain. The rotation for the horizontal coordinate system of each horizontal layer is necessary to maintain a consistent downstream axis needed for the data analysis. After this rotation, we calculated the instantaneous perturbation of the velocity components from the xyt average for each voxel in space and time along each horizontal layer, such that

$$u_r' = u_r - \langle u_r \rangle_{xyt} \quad (9)$$

where the prime marks an instantaneous perturbation from the mean value, in this example of the u_r (downstream) velocity component. Similar formulation applies to the vertical (w) and cross-stream (v_r) velocity components. Momentum flux at the down-steam direction was calculated as

$$\langle u_r' w' \rangle_{xyt} = \left\langle \left(u_r - \langle u_r \rangle_{xyt}\right)\left(w - \langle w \rangle_{xyt}\right) \right\rangle_{xyt} \quad (10)$$

See Bohrer et al. (2009) for more details on the calculation of wind statistics and momentum fluxes from RAFLES output."

2.2 Site description

2.3 Large eddy simulations

A. P16358, L6-8:

a. "RAFLES resolves the canopy as a 3-D heterogeneous domain where the leaves interact with the flow": Same issue as 1C.

Revised to: "RAFLES (Bohrer et al., 2009) uses a 3-D heterogeneous canopy domain where leaf and stem areas are prescribed within each voxel. The leaf area density and the wind speed within the voxel determine the drag force that is applied to wind flow through that grid cell within each time step. "

b. "The canopy is represented as leaf density and volume restriction terms": Here comes the true description of RAFLES. Please clarify the physical meaning of this type of canopy representation. What specific effects of canopy on the flow were accounted for by RAFLES? What are the other potentially important effects of canopy on the flow that have not been accounted for by RAFLES?

I can spend days and fill volumes providing the most accurate description and justification for the LES model, its numerical approach and all of its parameterizations. I did all of that in my thesis (Bohrer 2007), and in a much more concise way, the model is described in Bohrer et al (2009) and Chatziefstratiou et al (2014), both are referenced in this section. We have to strike a balance between providing enough detail to understand what we did, and not overburdening the readers with too much detail about the inner guts of the model. RAFLES is well established and interested readers can find the details in the listed references that describe the model.

We did add some more details, particularly for items that are related to the effects of the canopy.

"The leaf area density and the instantaneous wind speed within the voxel determine the drag force that is applied to wind flow through that grid cell within each time step. Common to the approach used in most LES, it assumes the leaf area is composed of flat surfaces oriented downstream and neglects higher-order effects of leaf and stem shapes and sub-grid-scale wake generation (shown to be a small effect, Shaw and Patton, 2003). It is combined with radiation attenuation (given the leaf densities in the grid cells above) to determine the sensible and latent heat fluxes emitted in each grid cell. The model uses the finite volume approach for discretization of the simulation domain. It resolves the effects of volume restriction due to the volume of the vegetation (stems, branches) by reducing the aperture areas available for flux exchange between each pair of neighboring grid cells and by reducing the volume that is available for flow within each grid cell according to the volume of the vegetation present within the grid cell (Chatziefstratiou et al., 2014). It resolves sub-grid-scale turbulence using the Deardorff (1978) scheme, and includes a parameterization for sub-grid-scale turbulence dissipation due to leaf drag (Shaw and Patton, 2003)."

B. P16358, L25-27:

a. Were subgrid-scale fluxes and statistics available in the snapshots? If so, were they used in the calculation of fluxes and statistics? At least subgrid-scale fluxes of momentum and scalars should be included in the analysis.

Only sub-grid-scale turbulence (SGS-TKE) is available in the model output, and it is a directionless scalar, and thus, cannot be directly added to the resolved fluxes. The model dynamically calculates the components of the momentum equation that are driven by the sub-grid-scale stresses, as parameterized by SGS-TKE, but this calculation is done at 40 Hz and the statistics of the sub-components of the momentum equation are not recorded as state variables at the 2-second snapshots. In any case, due to the fine resolution of the model, these terms are relatively small in the regions that are used to calculate the roughness length, above 1.5*h* (at that height, SGS-TKE is less than 1% of resolved kinetic energy).

b. The description of how fluxes and statistics were calculated is unclear. Take $<u'w'>$ (Reynolds stress) for example, it sounds like that it was calculated as $<u'w'> = <(u'w'-<u'w'>_{xy})>_{xyt}$, where subscript xy indicates average over the horizontal plane, and subscript xyt indicates average over the horizontal plane and time. This is not the definition of Reynolds stress. Please clarify the definition of Reynolds stress from Reynolds decomposition. What would be the physical meaning for Reynolds decomposition using mean quantities determined by time, spatial and ensemble averaging, respectively? Which averaging is most appropriate for the analysis performed here?

We added a detailed description and formulation. See the new explanation and formulation in our response to comment E above. We use a consistent Reynolds decomposition, applied in space and in time. We were wrong to relate to $<w'u_r'>$ as 'Reynolds Stress', though it is commonly done. There is a factor of air density between the downstream components of Reynolds Stress and $<w'u_r'>$. We now use the term streamwise momentum flux instead of Reynolds Stress throughout the manuscript.

C. P16359, L10: What is the mathematical expression for "a reflective top boundary"? Please explain the reason why a reflective boundary condition was chosen.

We replaced the term 'reflective top' with 'no-flux' boundary condition. They are the same thing, but 'no-flux boundary condition' is the proper term to describe it. It is a very standard LES approach and we see no reason to explain it further, especially since the top boundary (at 1 km) has little to no influence on the results at the sub-domain we used for data analysis (at 40-100 m).

D. P16359, L13-14:

a. *"Surface boundary layer height": This is not a defined term. Is it "surface layer height" or "atmospheric boundary layer height"?*
b. *"Surface boundary layer height was prescribed by the shape of the potential temperature profile": This statement is vague. Please clarify how "surface boundary layer height" was prescribed.*
c. *The word "prescribed" leads to an impression that the "surface boundary layer height" does not evolve in time. Is it true? If so, please explain why a fix "surface boundary layer height" was used. If not, please replace the word "prescribed" with a more proper word.*

For comments D.a-c, we clarify this sentence to: "The prescribed initial vertical profile of the potential temperature described a well-mixed atmospheric boundary layer and was constant from 50 m to the height of the capping inversion, and increased with height above that level. "

2.4 Virtual experiment setup

2.5 Empirical determination of roughness parameters from simulation results

A. P16360, L18-19: Here comes the determination of h_a. What is the corresponding mathematical expression?

We mixed up two different terms in this section. We apologize for that. We cleaned up this section. It now reads: "We determined the effective aerodynamic canopy height, h_a, by identifying the height at which 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, 2007). To find this point we compiled a domain-averaged wind-speed profile using Eq. 8. Then, we determined h_a as the location where the second derivative of the horizontal wind profile crosses zero."

B. P16361, L2: What does "the lateral forcing of wind speed was effective" mean?

We removed that statement.

C. P16361, L6-8: The mean u_* and Obukhov length were used, implying u_* and Obukhov length varied with height. What are the degree of variations? What caused the variation? What averaging method was used to obtain the mean? Please also justify the physical meaning of the averaging method.

Obukhov length is a direct function of elevation as it is dependent on the virtual temperature, which varies with height. In the MOST formulation it is used as a ratio z/L and at each z there is a corresponding L. $u*$ is invariable over a range far from the ground. In the model there are very small (<0.1%) variations in $u*$ within the range 3.5-4.5h, and we used the average value of $u*$ from this range.
When interpreting tower observations, z is used as the tower height and both L and u* are calculated at the tower height.

"We calculated the characteristic domain-averaged u_* for each simulation case by calculating the horizontal-temporal average u_* for each for each horizontal plane of grid cells within the 3-D virtual domain and further averaging these vertically over the range from 3.5-4.5*h* (u_* values are nearly invariable with height in that range). Obukhov length was calculated for each horizontal plane of grid cells within the 3-D virtual domain as a function of the characteristic u_*, surface heat flux (which was prescribed) and the mean potential virtual temperature at each horizontal plane of grid cells. Next, the vertical profile of horizontal mean wind speed from vertical layers above 1.5h_a and below 4.5h (95 m) above ground was fitted to Eq. 1 to determine d and z_0 using the characteristic friction velocity and the Obukhov length. "

2.6 Surface roughness parameters:forest structure effects

A. P16361, L14-17: This sentence provides the same information as P16360, L3-6.

We removed the entire 2.6 section.

2.7 Testing empirical models linking roughness parameters to biometric measurements

A. P16361-16362: Please clarify the difference among the three methods: (a) "Biometric", (b) "Raupach (1994)" and (c) Nakai et al. (2008a). Why are they chosen as representatives for the determination of roughness parameters? What performance do we expect from a theoretical perspective?

B. P16363, L7-8: Please clarify the approach of "Yearly Observed".

A-B: We rewrote this section and moved it to the 'Results' (following a request by Reviewer #1). The methods are now explained in a clearer way. The difference between the approaches are clearly provided by the detailed descriptions of the methods. We added a discussion of the differences in the type of input needed for each method and the significance of that for potential application of each model in the discussion section. Please see revised sections 3.1 and 3.2 for explanation of the different approaches, and revised section 4.2 for discussion of differences between them.

3 Results

3.1 Virtual experiment to explore canopy-roughness relationships

3.2 Canopy-roughness improvements to surface flux models

4 Discussion

4.1 Response of roughness parameters to canopy structure change

A. P16367, L2-3: What does "the ability for eddies to transport momentum" mean? Does it precisely describe the physical meaning of the eddy penetration depth shown in Fig. 3a? What does the comparison "weakened" point to? Is it a comparison within a specific type of eddies or for the distribution of eddies on some characteristics scales?

We completely rewrote this section and this sentence is no longer included in the revised version.

B. *P16367, L4: What does "sub-canopy turbulence" mean? Is it well characterized by vertical momentum flux only?*

We meant "turbulence kinetic energy inside the canopy domain." We revised it accordingly.

C. *P16367, L16-17: Please specify "environmental forcing". Through what mechanism does "environment forcing" affect the values of roughness parameters?*

This statement is no longer included in the revised section.

4.2 Integrating canopy-structure characteristics into flux models

5 Conclusions

A. *P16371, L4-22: I do not see highlights of the most important results from this paper.*

We added them. We prefer to limit the conclusions section to concluding remarks that integrate what we have found in the study and put it in a broader context, and not to act as a second abstract. We did however revise the conclusions section, and we hope that you find the revised version improved relative to the previous one. The revised conclusions section:

"In this study we used an LES, long-term meteorological observations, and remote sensing of the canopy to explore the effects of canopy structure on surface roughness parameters in a forest site. We performed a virtual experiment to test the sensitivity of roughness parameters with respect to four axes of variation in canopy structure: (1) leaf area index, (2) the mode of the vertical profile of LAD, (3) canopy height, and (4) gap fraction. We found consistent relationships between the aerodynamic canopy height and *LAI*, maximum height, and gap fraction and between d and maximal canopy height. We found that the predicted values of friction velocity are not sensitive to roughness length. As a result, most of the roughness-based approaches we tested for simulating friction velocity performed similarly well. This is despite having very different approaches for determining the values of z_0 and d, and having large differences in the range of z_0 and d values. This is good news for modelers, because it limits the error from using the current approaches that do not vary in time and do not incorporate canopy structure.

Nonetheless, most of the approaches we tested which used annually variable z_0 and d and that incorporated canopy structure provided better approximation for friction velocity than the 'Classical', time-invariable method. Many easily obtainable metrics of canopy-structure characteristics are available through a suite of measurements, such as on-site meteorological and biometric observations or satellite-derived site characteristics. Additionally, many ecosystem models and ecosystem modules within earth system models resolve the growth of the forest and accurately predict canopy height and *LAI*. Some models, such as the Ecosystem Demography model (Medvigy et al., 2009) even resolve the distribution of stem sizes. Such demographic models could readily incorporate the approach by Raupach (1994) for a significant improvement in surface roughness parameterization. For other models that resolve, or are forced by observed leaf area and vegetation height, our LES-derived 'Biometric' approach could offer an easy way to dynamically affect the roughness-length parameterization. This could provide an improvement of surface flux modeling, especially when canopy structure variations are large. Due to limited spatial coverage by direct meteorological measurements, remote sensed structure statistics, and stand inventories, we suggest utilizing site- and time-specific biometric measurements of canopy structure to estimate site-level *d* and z_0. The effectivity of these model improvements will, of course, be dependent upon the quality, quantity, and resolution of the datasets available at the forest of interest."

References

A. *P16377, L4: "Reynolds" should be capitalized.*

Corrected.

Tables

Table 3

A. *The value of r^2 is 0.80 for all cases. If it was calculated, please explain the reason why the same value was found for all cases. If it was prescribed, please clarify and explain why it was not calculated.*

We redid this analysis and expanded the period over which we model u* values. We provide the r^2 values of the revised analysis to the 3rd significant figure, and they are not all identical.

B. *How do Biometric estimates of d and z_0 compare with observations and other models? Please discuss the discrepancies among d and z_0 and the effects of these discrepancies on the estimates of u_*.*

d estimates from the 'Biometric' approach are within the range of the inter-annual variation of the observed values. z_0 values are about twice as high, and at the highest range among all approaches. However there is very little agreement between any of the methods about the values of z_0. We added the following section to the end of section 3.2 (results):
"The values of d and z_0 as determined by each of the parameterization approaches are listed in Table 3. The range for yearly observed mean *d* values was 18.3-26.0 m and for z_0 0.99-1.99 m. The 'Classical' approximation based on *h* resulted in a significantly lower *d* = 14.0 m (outside the range of the inter-annual variability over 12 years), and a slightly above-range z_0 = 2.10 m. The 'Explicit-LES' resulted in a very similar *d* to the 'Classical' approach. The 'Biometric' approach predicted high but within-range *d* values (24.0-25.0 m) but extreme z_0 values (3.64-3.82 m). There was nearly no overlap between the values of z_0 prom each of the approaches, indicating poor agreement between approaches for this parameter."

Fig. 1

A. *How do LES results of d and z_0 compare with other models? Please discuss the discrepancies.*

We added this discussion to the results. See response above.

B. *The definition of "leaf-off" and "leaf-on" were given in the figure caption. They should* *also be stated in the text where these terms were first mentioned.*

We added this at section 2.4 of the methods.

Fig. 2

A. *Figs. 2a and 2c: The variation of h_a is within 0.3 m for the range of LAI (2a) and within 0.2 m for the range of gap fraction (for a fixed value of LAI in 2c). The dependence of h_a on LAI and gap fraction would only be convincing if the variation of ha is much larger than the uncertainty associated with the determination of h_a. What are the uncertainties associated with the estimates of h_a?*

The statistics of these relationships are provided in Table 2. The relationship between h_a and gap fraction, and *LAI* are strongly significant.

Fig. 4 — 6

1. *Figs. 4a, 5a and 6a: It was stated on P16360, L18-19 that h_a was determined as the height of inflection point. However, in the figures the inflection points were visually below the corresponding values of h_a provided in Table A1. Please explain.*

We made a half-pixel shift of the y-axis labels used in the figure. We thank the reviewer for catching this error and we fixed the figures to list the correct heights.

2. *Fig. 4b, 5b and 6b: What caused Reynolds stress at the canopy top vary by a factor of two? Were all these simulations forced by the same geostrophic wind? To what degree did the atmospheric boundary height vary from one simulation to another?*

All simulations are forced by the same wind speed aloft, initialized by the same vertical profile of potential temperature (which determines the atmospheric boundary layer height) and have the same surface heat flux, so the growth rate of the ABL should be similar in all simulations, and the wind profiles directly comparable. Reynolds stress has a sharp gradient and therefore, when showing it up to an arbitrary height near the canopy top, differences in *d* lead to differences in the value at that particular arbitrary height. We revised the figure to extend to a higher elevations (3*h*), and further normalize the momentum flux by u_*^2 and the results align across all simulations.

3. *"Reynold's stress" used in the figure captions is incorrect. See P16377, L4, "Reynolds stress" is the correct term.*

This was a typo, but we changed the term to Momentum flux, following other comments.