Response to Interactive comment on "Stably stratified canopy flow in complex terrain" by X. Xu et al.

-------------------------- **Reviewer #2**----------------------------

Reviewer #2
This manuscript presents numerical simulations of stably stratified flow within and above a canopy over an isolated, idealized two-dimensional hill. The numerical model uses the Renormalized Group (RNG) k-epsilon turbulence model. The topic is an interesting and important one. Flow decoupling and drainage flows under stable conditions are important in controlling nighttime fluxes from forest canopies. While the simulations are potentially interesting, I found the discussion of them rather confusing and not particularly enlightening. I also have a number of questions about the model itself. This is compounded by the English language. A number of sentences just didn't make sense and I was unable to work out what you were trying to say. In addition to my scientific suggestions below, the language in the manuscript needs careful proof reading. Given my long list of questions I recommend major revisions for this manuscript.

Authors:
We appreciate referee #2 for the valuable comments. We have made revisions based on the comments.

Reviewer #2
Major comments
1) I'm not quite sure what the aim of the paper is. The abstract suggests that it is just showing that the model can successfully simulate stable canopy flows. It does simulate canopy flows which look reasonable and qualitatively reproduces some features seen in field observations, but since the simulations are very idealised there is no data to quantitative compare with and so it is impossible to be sure that the model really is "accurate".

Authors:
The canopy layer is an interface between land and atmosphere, which directly influences mass and energy exchange between vegetation and atmosphere. The complexity of nocturnal canopy flows in hilly terrain has been challenging the accurate measurement of mass and energy flux in FLUXNET community. The huge difficulty and problem (e.g. advection error) in eddy-flux measurements has been caused by stably-stratified canopy flow over complex terrain so that the measured data must be trashed under conditions with weak turbulence and very stably stratified flow (Goulden et al., 1996; Aubinet et al., 2003; Staebler and Fitzjarrald, 2004; Sun et al., 2007; Yi et al., 2008; Montagnani et al., 2009; Feigenwinter et al., 2010; Aubinet and Feigenwinter, 2010; Queck and Bernhofer, 2010; Siebicke et al., 2012). This is a well-known and long-standing problem in FLUXNET that consists of more than 500 towers around the world. We want to understand this difficult condition from modeling aspect. We agree with the reviewer that this is not accurate prediction since measurements that can be used to compare with modeling results are not available yet. However, we want to use CFD modeling to qualitatively understand the main features of stably-stratified canopy flow that have been captured by separate field observations (Alekseychik et al., 2013; van Gorsel et al., 2011; Jacob et al., 1992; Leclerc et al., 1990;Yi et

al., 2005; Zhang et al., 2010).

Reference:
Alekseychik, I. Mammarella, P., Launiainen, S., Rannik, Ü., Vesala, T.: Evolution of the nocturnal decoupled layer in a pine forest canopy. Agric. For. Meteor. 174-175, 15-27, 2013.
Aubinet, M., Feigenwinter, C.: Direct CO2 advection measurements and the night flux problem. Agric. For. Meteor. 150, 651-654, 2010.
Aubinet, M., Heinesch, B., Yernaux, M.: Horizontal and vertical CO2 advection in a sloping forest. Bound.-Layer Meteor. 108, 397–417, 2003.
Feigenwinter, C., Montagnani, L., Aubinet, M.: Plot-scale vertical and horizontal transport of CO2 modified by a persistent slope wind system in and above an alpine forest. Agric. For. Meteor. 150, 665–673, 2010.
van Gorsel, E., Harman, I. N., Finnigan, J. J., Leuning, R.: Decoupling of air flow above and in plant canopies and gravity waves affect micrometeorological estimates of net scalar exchange. Agric. For. Meteor. 151, 927-933, 2011.
Goulden, M. L., Daube, B. C., Fan, S., Sutton, D. J., Bazzaz, A., Munger, J. W., Wofsy, S. C.: Physiological response of a black spruce forest to weather. J. Geophys. Res. 102, 28987–28996, 1997.
Jacobs, A.F.G., van Boxel, J.H., Shaw, R.H.: The dependence of canopy layer turbulence on within-canopy thermal stratification. Agric. For. Meteorol. 58: 247-256,1992.
Leclerc, M. Y., Beissner, K. C., Shaw, R. H., den Hartog, G., Neumann, H. H.: The influence of atmospheric stability on the budgets of the reynolds stress and turbulent kinetic energy within and above a deciduous forest. J. Appl. Meteor. 29, 916–933, 1990.
Montagnani, L., Manca G., Canepa, E., Georgieva, E., Acosta, M., Feigenwinter. C., Janous, D., Kerschbaumer, G., Lindroth, A., Minach, L., Minerbi, S., Mölder, m., Pavelka, M., Seufert, G., Zeri, M., Ziegler, W.: A new mass conservation approach to the study of CO2 advection in an alpine forest. J. Geophys. Res. 114, D07306. doi:10.1029/2008JD010650, 2009.
Queck, R., Bernhofer, C.: Constructing wind profiles in forests from limited measurements of wind and vegetation structure. Agric. For. Meteor. 150, 724-735, 2010.
Sun, J., Burns, S. P., Delany, A. C., Oncley, S. P., Turnipseed, A. A., Stephens, B. B., Lenschow, D. H., LeMone, M. A., Monson, R. K., Anderson, D. E.: CO2 transport over complex terrain. Agric. For. Meteor. 145, 1–21, 2007.
Staebler, R. M., Fitzjarrald D. R.: Observing subcanopy CO2 advection. Agric. For. Meteor. 12, 139-156, 2004.
Siebicke, L., Hunner, M., Foken, T.: Aspects of CO2 advection measurements, Theor. Appl. Climatol. 109, 109–131, 2012.
Yi, C., Monsoon, R. K., Zhai, Z., Anderson, D. E., Lamb, B., Allwine, G., Turnipseed, A. A., Burns, S. P.: Modeling and measuring the nocturnal drainage flow in a high-elevation, subalpine forest with complex terrain. J. Geophys. Res. 110, D22303. doi:10.1029/2005JD006282, 2005.
Zhang, G., Leclerc, M. Y., Karipot, A.: Local flux-profile relationships of wind speed and temperature in a canopy layer in atmospheric stable conditions. Biogeosciences 7, 3625-3636, 2010.

Reviewer #2

2) Justification of the RNG closure. Two references are given to support the use of the RNG model over complex terrain. These are both for neutral flow. Only the previous paper by the authors has a canopy included (Xu and Yi, 2013), but this contains no validation of the model or comparison with observations. Has the RNG model been validated for canopy flows? Has it been validated for stable flows? The reader needs some evidence the model is correct before believing the results from this study.

Authors:

We have a detailed discussion of current models and their limitations applied in canopy flows in the section of 'Introduction'. RNG turbulence models have been applied to vegetation canopy flows on hilly terrain (Xu and Yi, 2013; Pattanapol et al., 2006) and urban canopy flows (Kim and Baik, 2004; Kim et al., 2012; Koutsourakis et al., 2012; Cheng et al., 2009). Pattanapol et al. (2006) compared the numerical results with measurements on hill slopes. The representation of source/sink terms of canopy (drag force, wake production in momentum and TKE budget) shows a more accurate prediction than those using roughness parameters. The RNG model is validated for urban canopy flows under neutral and unstable condition. Due to the difficulties in measuring night-time small scale turbulence and strong variability in topography and canopy structure, we can only have some qualitative comparison between our model prediction and field measurement. As we do this pioneer study of stably stratified canopy flow in hilly terrain, we expect more results from other model simulations, wind tunnel experiments and accurate quantification of the high resolution topography and vegetation morphology and intensive measurements of canopy flows to validate our modeling results.

Cheng, W. C., Liu, C. H., Leung, D. Y. C.: On the correlation of air and pollutant exchange for street canyons in combined wind-buoyancy-driven flow. 43, 3682-3690, 2009.
Kim, J. J., Baik, J. J.: A numerical study of the effects of ambient wind direction on flow and dispersion in urban street canyons using the RNG k-e turbulence. 38, 3039-3048, 2004.
Kim, M.J., Park, R. J., Kim, J. J.: Urban air quality modeling with full O3-NOx-VOC chemistry: Implications for O3 and PM air quality in a street canyon. Atmos. Environ. 47, 330-340, 2012.
Koutsourakis, N., Bartzis, J. G., Markatos, N. C.: Evaluation of Reynolds stress, k-e and RNG k-e turbulence models in street canyon flows using various experimental datasets. Environ. Fluid Mech. 12, 379-403, 2012.
Pattanapol, W., Wakes, S. J., Hilton, M. J., Dickinson, K. J. M.: Modeling of surface roughness for flow over a complex vegetated surface. **World Acad. Sci. Eng. Technol. 26, 271-291, 2006.**
Xu, X., Yi, C.: The influence of geometry on recirculation and CO_2 transport over forested hills. Meteorol. Atmos. Phys. 119, 187-196, 2013.

Reviewer #2
3) In the description of the RNG model you state that "Tp is calculated as the residual of all other terms". How can you do that as you don’t know what dk/dt is? Or do you assume dk/dt = 0 (implied later on in section 3.5)? In that case this is only a steady state turbulence closure model, but is applied to a time-varying model? Seems like a major limitation to me. Can you comment on this?

Authors:
In our simulation, steady state ($\partial k/\partial t = 0\ and\ \ \partial \varepsilon/\partial t = 0$) is assumed. We corrected the typos in the eq. (13) and (14), thank you! The condition of steady state flow under stable condition was proposed by Mahrt (1992). We evaluated our model setup in section 2.2 that meets the steady state assumption.

Reviewer #2
4) It is stated that the flow is sufficiently forced to ensure the flow remains turbulent. I find this a bit hard to believe with such stable layers. Accurately simulating stable flow is hard - and this comes back to my comments above about whether the model is really validated.

Authors:
We think that the simulation of stable flow on flat terrain is hard. But the buoyancy flow on slope can be simulated by CFD model. Luo and Li (2011) have simulated buoyancy driven slope flow and wall flow with even stronger cooling on slope surface (-100 Wm^{-2}) and wall surface (-100 Wm^{-2}) under calm conditions (no synoptic winds). The stronger cooling intensity leads to stronger down-slope wind, thus more down-slope air mixing.

Luo, Z., Li, Y.: Passive urban ventilation by combined buoyancy-driven slope flow and wall flow: Parametric CFD studies on idealized city models. Atmos. Environ. 45, 5946-5956, 2011.

Reviewer #2
5) Key to interpreting these idealised simulations seems to be the drawing down of air into

the canopy near the summit due to continuity. This is in part due to the idealised topography, and also the complete absence of any background flow. It would be interesting to know how more complex terrain and / or a weak (but non-zero) wind would modify the results. Is this something you have considered? It would at least be worth commenting on.

Authors:
In this study, our focus is on canopy flow driven purely by thermal and orographic forcing. The idealized simulations are better to interpret different mechanisms that regulate the small-scale turbulence in canopy layer. We also have the curiosity to see how synoptic weather condition (background wind) affects the local canopy flow. We have just finished one more manuscript in which we did three-dimensional simulation of canopy flows with real vegetation and terrain under three different synoptic weather conditions. The results are much different because of interactions between background wind and local slope wind, redistributed heat flux by heterogeneous vegetation and topographic effects.

Reviewer #2
6) These simulations should be entirely symmetric (in fact you state they are at the start of section 3), but the streamlines in figure 1 are not symmetric. Why? What breaks the symmetry?

Authors:
The asymmetry is very tiny. We believe that this is okay because any small numerical perturbation will cause the solution to instead go towards a stable asymmetric solution, so called the Coanda effect. The most important is that we have tested the grid-independence.

Reviewer #2
7) Section 3.2. The pooling of cold air at the bottom of the slopes seems to be important in decelerating the flow. How is this influenced by the model geometry? Would the results differ with a wider domain? Did you test sensitivity to this? How would this translate to the real world with 3-d valleys? (As an aside, in order to reach a steady state, the cold air must go somewhere. I assume that there is outflow from the lateral boundaries?)

Authors:
We agree with the reviewer that the pooling of cold air at the bottom of slopes decelerates the down-slope flow. We found that on the same elevation, the temperature inversion on steeper slope is slightly stronger than that on gentler slope. We didn’t make the experiments of wider domain. But we believe that the wider domain would not affect the cold air pool in our single hill case because of the long enough lateral domain and open lateral boundary. The 3-D valleys will lead to a different cold air pool because a closed valley would block the ventilation of cold air, which cannot be interpreted by our single hill modeling.

Reviewer #2
8) End of section 3.2. The effects of slope here controlling whether flow penetrates to the bottom of the canopy or not are interesting. You imply that this is due to the buoyancy force, which is in part true. I think there is more to it than that though. Even on a shallow slope there is a downslope drainage flow, and so by continuity some air must be drawn down deep into the canopy to compensate. I think this needs a more careful analysis to explain what is happening. It may also be amenable to some scaling analysis to show how the slope effect scales? Similarly I do not fully understand what causes the differences in the regions of baroclinicity at the bottom of the slope, and hence the differences in circulation. In particular

the upslope flow in the mid canopy over the gentle slope seems odd. Is there any observational evidence of this? How much is this controlled by the cooling of cold air? These are the kind of details which may be sensitive to the turbulence parametrisation - which again comes back to the question of how well validated the model is.

Authors:

We agree with reviewer that the downslope drainage flows even occur on shallow slope. The drainage flow is controlled by competition of hydrodynamic pressure gradient $F_{hd} = d\Delta p/dx \approx U^2 H/L^2$ and buoyancy force (or hydrostatic pressure gradient) $F_{hs} = g(\Delta\theta/\theta_0)\ sin\alpha \approx g(\Delta\theta/\theta_0)H/L$, *where* α *is the slope angle,* $\Delta\theta$ *is the potential temperature difference between the ambient air and the colder slope flow,* θ_0 *is the ambient potential temperature (Belcher et al., 2008).* Froude number Fr=U/NL=$\sqrt{F_{hd}/F_{hs}}$, where $N = (g/\theta_0)d\theta/dz$, L is the hill length scale. When Fr <<1, the hydrostatic pressure gradient is large enough to initialize drainage flow. We have two schematic figures below showing temperature and pressure field (Figure 1), which indicate the region of baroclinicity. The canopy winds in the broclinicity region shift direction form deep drainage flow to shallow drainage flow (Figure 2). For the concern of the upslope flow in the mid canopy, we could not find a quantitative analysis of it. However, we did a smoke experiment in 2011 and found this updraft motion (Figure 3). The link of our experiment video is https://www.youtube.com/watch?v=ljZ88QZWPw0.

Reference:
Belcher, S. E., Finnigan, J. J., Harman, I. N.: Flows through forest canopies in complex terrain. Ecol. Appl. 18, 1436-1453, 2008.

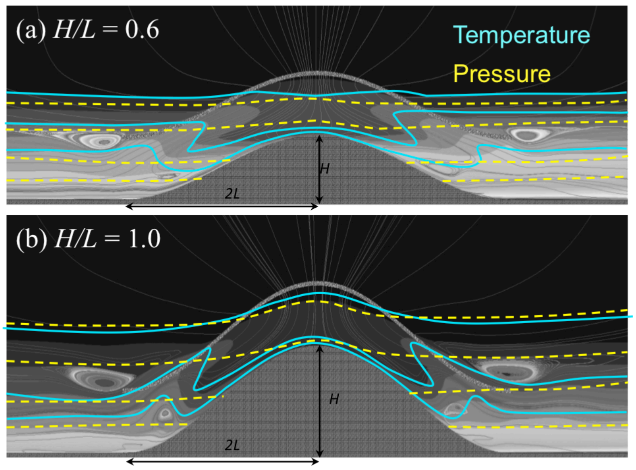

Figure 1 The temperaure and pressure fields indicating the baroclinicity regions.

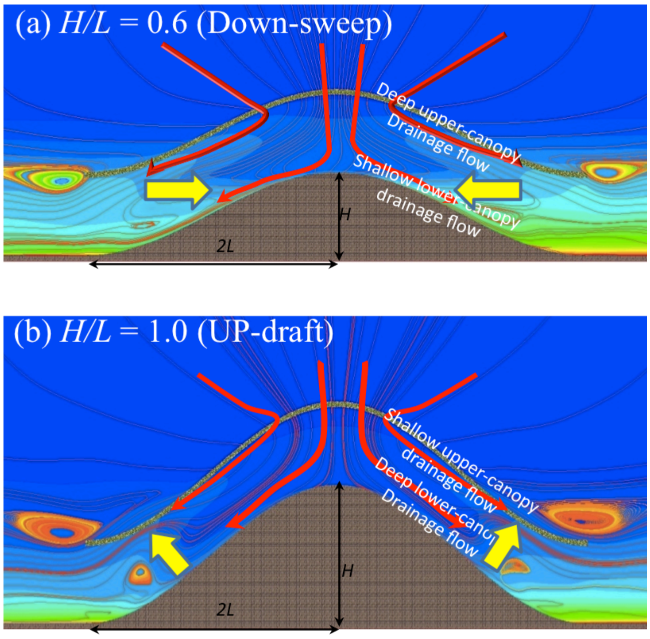

Figure 2 The drainage flow pattern and the direction of wind shifting in canopy

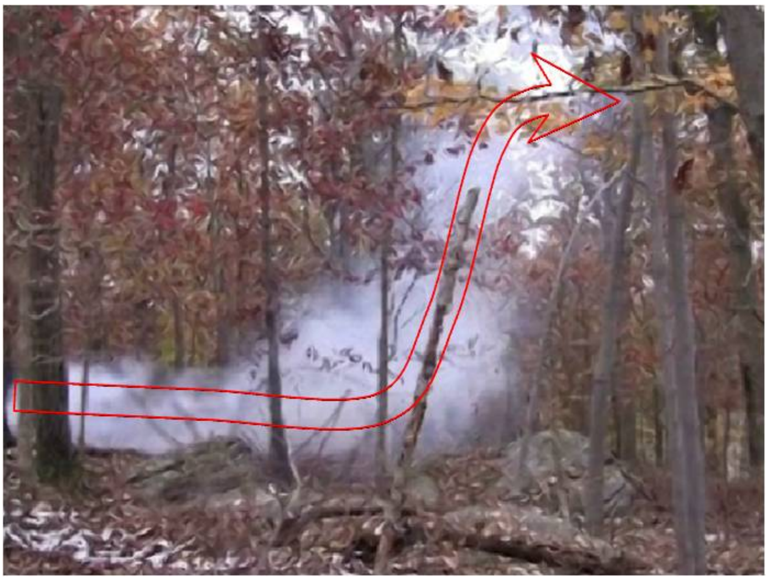

Figure 3 Updraft motion detected in forest canopy in the early morning by smoke experiment.

Reviewer #2

9) I wasn't entirely clear from the text whether the canopy is only on the slope and not on the flat ground. This seems to be implied by Figure 2. This may have a significant effect in controlling what happens at the bottom of the slope and is an added complication. In particular, I wonder if this controls the vortices seen near the bottom the slopes. Did you try experiments with a fully forested domain?

Authors:

In this simulation, canopy is covering the slope. We have the description in section 2.1. The vortices near the bottom of slopes are caused by the edge of canopy. Strong shear and large TKE are shown in vortex regions. We did not make experiments with fully forested domain.

Reviewer #2

10) Section 3.3 I found to be rather unsurprising. The results seem entirely consistent with the observed mean flow and much of the section is just repeating other studies.

Authors:
The major purpose of this study does not focus on turbulence in vary stably stratification but complexity of local flows resulting from interactions between canopy, terrain, and cooling effect. We believe that these simulations for difficult conditions in eddy-flux measurements will be useful for FLUXNET community to understand their advection problems and issues. So we are happy that we can reproduce the shear stress and turbulent heat flux profiles in agreement with measurements.

Reviewer #2
11) Section 3.4 is potentially interesting, but given the questions raised above about the RNG scheme and how well it has been validated in stable / canopy flows it is hard to have too much faith in the conclusions, particularly about the importance of the pressure term. Other observational studies do seem to suggest this is important though and it would be interesting to pin this down.

Authors:
We agree with the reviewer that pressure perturbation is uncertain to TKE budget. We have added the discussion of the uncertainties in calculating and measuring pressure perturbation in 3.4.

Reviewer #2
12) In the concluding remarks you say that no comparison with field observations is possible. There may not be detailed measurements of all the relevant terms in the TKE budget, but there are (limited) measurements of mean flow and turbulent fluxes from multi-tower, multi-level experiments as stated. The model could, and should, be compared with these to validate it. The other source of data is wind-tunnel experiments which are generally better observed and more controlled. There have been recent experiments at the CSIRO Pye Lab wind tunnel with stable canopy flows. These are not yet published, but if and when the data is published this would be another valuable source of validation data.

Authors:
We agree with the reviewer that the numerical model should be validated. However, we still cannot get enough information from the limited measurement data. Current analysis of the stable canopy flow lacks detailed description of either canopy structure, topography, or atmospheric stability. Wind tunnel experiments would be a good way to validate our idealized model due to controlled setting. We know that there are research groups that made wind tunnel experiments of canopy flow. But these experiments are under neutral or unstable condition (Endalew et al., 2009; Segalini et al., 2013; Poggi et al., 2004). We are happy to hear that CSIRO Pye Lab has done wind tunnel experiments of canopy flow under stable condition. We look forward to seeing these data published and compared with our model results.

Endalew, A. M., Hertog, M., Delele, M. A., Baetens, K., Persoons, T., Baelmans, M., Ramon, H., Nicolai, B. M., Berboven, P.: CDF modeling and wind tunnel validation of airflow through plant canopies using 3D canopy architecture. INT. J. *HEAT. FLUID* FL. 30, 356-368, 2009.
Segalini, A., Fransson, J. H. M., Alfredsson, P. H.: Scaling laws in canopy flows: A wind-tunnel analysis. Bound.-Layer Meteorol. 148, 269-283, 2013.
Poggi, D., Porporato, A., Ridolfi, L., Albertson, J.D., Katul, G. G.: The effect of vegetation density on canopy

sub-layer turbulence. 111, 565-587, 2004.

Reviewer #2

Minor comments

1) p28488, line 14. You state that the benefit of the RNG model is the lack of any tuneable empirical parameters. This is not true as the model contains 7 empirical constants (see p28493, lines 14-15).

Authors:

The empirical parameters are derived from Renormalized group method. The parameters are not experimentally adjustable (Yakhot and Orszag, 1986 ;Yakhot, 1988).

Yakhot, V. and 5 Orszag, S. A.: Renormalization group analysis of turbulence, Phys. Rev. Lett., 57, 1722–1724, 1986.
Yakhot, V.: Propagation Velocity of Premixed Turbulent Flames, Combustion Science and Technology, 60:1-3, 191-214, 1988.

Reviewer #2

2) p28490, line 9. What is $\theta\infty$? How does this differ from θ_{00} defined on the previous page?

Authors:

$\theta_0(z) = \theta_{00} + \gamma z$ defines as the ambient temperature, where θ_{00} is surface temperature and γ *is* positive temperature gradient . θ_∞ is buoyancy reference temperature. It is specified as the mean temperature of the domain. The buoyancy force in eq. 3 is a linear function of fluid thermal expansion and the local temperature difference with Buoyancy reference temperature.

Reviewer #2

3) Eqs 7-9. Why is this form of the drag force taken rather than the more usual F = CDau|U|?How does this compare?

Authors:

In numerical modeling of canopy flow, the drag coefficient Cd is usually set as a constant value for uniform canopy (Patton et al., 2003; Dupont et al., 2008; Katul et al., 2006). We applied the leaf area density profile from a real forest without a direct measurement of drag coefficient. Thus, we use the empirical relationship between leaf area density and resistance coefficient (eq. 8 and 9).

Reference:
Katul, G. G., Finnigan, J. J., Poggi, D., Leuning,R., Belcher, S. E.: The influence of hilly terrain on canopy–atmosphere carbon dioxide exchange. Boundary-Layer Meteorology 118, 189–216.
Dupont, S., Brunet, Y., Finnigan, J.: Large-eddy simulation of turbulent flowover a forested hill: validation and coherent structure identification. Q .J. Roy. Meteorol. Soc. 134, 1911–1929, 2008.
Patton, E.G., Sullivan, P.P., Davis, K.J.: The influence of a forest canopy on top=down and bottom-up diffusion in the planetary boundary layer. 129, 1415-1434, 2003.

Reviewer #2

4) Section 2.3. What is the Prandtl number taken as? No value is given in the text.

Authors:

The value of Prandtl number is given in section 2.2 (line 10-11 page 28495).

Reviewer #2
5) p28495, line 19. Should be "The Richardson number..."

Authors:
We corrected.

Reviewer #2
6) p28496, line 1. Should be "with" not "With" at the start of the line.

Authors:
We corrected. Thanks.

Reviewer #2
7) p28496. There are several references to subfigures 4a - 4f. Figure only contains 4 subfigures though, and these are not actually labelled. Do you mean the profiles a-f in the figures? If so, this is a very confusing notation. Please change.

Authors:
We made two corrections (Fig. 4 locations a-f) and (Fig. 4 locations e and f). Thanks.

Reviewer #2
8) p28496, line 23. Why is the depth of the secondary super stable layer "due to strong temperature inversion"? Is the strong inversion not just part of the super stable layer? I found this sentence confusing.

Authors:
We wanted to emphasize the super stable layer is deep on the lower slope consistent with deep and strong inversion layer. We rephrased this sentence. Thanks.

Reviewer #2
9) p28497, lines 1-2. Do you really mean stronger entrainment at the summit? Why just there? I interpret entrainment to be mixing due to turbulence. Is it not the mean flow, i.e. air being drawn down into the canopy over the summit to balance the downslope flow which is suppressing the secondary super-stable layer?

Authors:
We corrected the word ‘entrainment’ and add a reference.

Reviewer #2
10) p28497, lines 8-9. I don’t see the point of this sentence. Previous studies have already observed the stable canopy layer and linked it to decoupling. How does this clarify that?

Authors:
We replace ‘clarify‘ with ‘explain’.

Reviewer #2
11) p28497, line 10. "van Gorsel" not "Gorsel".

Authors:
We corrected. Thanks.

Reviewer #2
12) p28497, line 21. "from the terrestrial ecosystem."

Authors:
We corrected. Thanks.

Reviewer #2
13) p28497, lines 25-29. I found these sentences rather unclear. The phrases "under- goes direction shift within canopy." is odd. The English needs improving here to make the meaning clearer.

Authors:
We rephrased this sentence.

Reviewer #2
14) p28498, line 10. What do you mean by "lateral sides"? This sentence is unclear.

Authors:
We rephrased this sentence.

Reviewer #2
15) p28498, lines 10-11. This sentence is also very unclear. Why is the sinking motion diverted? What do you mean by top canopy layer?

Authors:
Here we mean that the sinking motion changed its direction from descending into the canopy to following the shape of canopy. We rephrased this sentence.

Reviewer #2
16) p28498 and figure 5. Again confusing whether Fig 5.d is referring to a subfigure or the location of a particular profile. Figure 5 seems to contain two subfigures labelled a), b) etc. I would suggest using a different naming convention for the profile locations to avoid confusion.

Authors:
We have two panels of subfigures in Figure 5, clarified as streamwise wind velocity and vertical wind velocity.
We corrected the captions of the figures to indicate the labeled (a)-(f) and their locations on the slope.

Reviewer #2
17) p28498, line 19 and figure 5. How can the velocity maximum be below the lower stable layer? The model description implies a no-slip lower boundary (the roughness length is given), but Figure 5 seems to show a non-zero velocity at the surface, in fact a velocity maximum occurs there. How can this be? Is the lower boundary actually free slip? Please explain, and if free slip then justify this choice.

Authors:
Our model is set with no-slip boundary. The wind velocity is not zero at the surface due to the centers of bottom grid cells are not exactly at the surface. We added this note to figure caption.

Reviewer #2
18) p28498, lines 24-25. This sentence doesn't make sense. What do you mean by "...determines the shift direction within canopy."

Authors:
We rephrased this sentence. Thanks.

Reviewer #2
19) p28505, lines 3-4. ".. with additional strong non-linear terms". What additional terms? Do you mean the RNG turbulence closure? I don't see the point of this sen- tence anyway.

Authors:
We clarified the nonlinear term in sentence.

Reviewer #2
20) p28505, lines 13-14. "... at the ultra-short wave scale in the whole spectrum of atmospheric turbulence study." This sentence doesn't make sense. Do you mean you are looking at very small-scale flows?

Authors:
We mean that our numerical simulations have examined leaves to micrometeorological scale processes in the whole atmospheric turbulence study. We rephrased this sentence.

Reviewer #2
21) Figure 1. Can you mark the canopy on this figure? Figure is not very good quality, and is difficult to read when printed.

Authors:
We updated figure 1. Thanks.

Reviewer #2
22) Figure 2. Caption mentions green dashed lines, but lines appear to be white to me?

Authors:
The white dashed line is the top of canopy. The cyan blue lines highlight the 'fish-head' temperature distribution. We corrected the caption. Thanks.

Reviewer #2
23) Figure 3. The second sentence in the caption is very poorly phrased. When you say "... which is normalized by the half length scale L" you presumably mean the locations. I would split this sentence and say "The locations of the size sections are labelled as (a-f). Horizontal distances are normalized by the half length, L, of the hill." or something similar. The caption mentions a green curve. It looks more like light blue to me?

Authors:

We corrected the caption. Cyan blue is more appropriate to name the color in figure 3. Thanks.

Reviewer #2
24) Figure 4. Resolution is not sufficiently good when printed. Are these bitmaps rather than vector graphics?

Authors:
We updated Figure 4. Thanks.

Reviewer #2
25) Figure 9 and 10. Plot the y-axis on the edge of the figures, not on the x = 0 line for clarity.

Authors:
We updated Figures 9 and 10. Thanks.