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Discussions

Interactive comment on "Explaining the convector effect in canopy turbulence by means of large-eddy simulation" *by* Tirtha Banerjee et al.

Anonymous Referee #2

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General comments:

This manuscript is motivated by the work from Rotenberg and Yakir (2010), which observed a decrease in aerodynamic resistance to heat transfer (r_H) over a forest when compared to a shrubland region under similar conditions, an effect that is caused by an increase in surface roughness and is accompanied by an increase in atmospheric instability. This effect was called "canopy convector effect" (CCE) by Rotenberg and Yakir (2010). In this manuscript, the authors investigate the occurrence of CCE above the canopy using Large Eddy Simulation (LES). After observing a decrease in r_H with increase in unstable conditions in the simulations (used as evidence of CCE), the authors compare different models of r_H as a function of height (above the canopy) and atmospheric stability with the simulation results, and conclude that some models cannot capture the correct trend of CCE at all (because they present an increase in r_H with instability), and only two (out of eight) models display the signature of CCE (decrease in r_H with instability). The authors proposed an improved parameterization of one of the r_H models by using a value of momentum roughness length scale z_{0m} that vary with atmospheric stability, improving the agreement between model and LES results. The authors conclude that CCE is a generic feature of canopy turbulence.

Because the value of r_H is needed for a wide range of applications, the investigation of the behavior of different r_H parameterizations above the canopy is useful, and the use of LES for this purpose is appropriate, therefore this manuscript deals with an interesting topic. However, as described in more details below, I believe the manuscript needs an alternative motivation, better description of the simulations and models and better interpretation of the results.

Major specific comments:

1. I'm not sure I agree with the authors' interpretation of what CCE represents. The authors defined CCE as a decrease in aerodynamic resistance above the canopy, which can be accomplished by an increase in atmospheric instability. In my opinion, it is already well-accepted that there is an increase in turbulent transport (estimated by eddy diffusivity parameters, for example) and consequent decrease in r_H with increasing instability. This should be valid over a canopy and over bare soil. The differences between the canopy and the bare soil cases are the type of the turbulent flow and level of penetration of the transporting eddies across the heat source layer in the canopy case (compared to the no-penetration condition over bare soil), which makes the turbulent transport different in the canopy case compared to the bare soil case, even if all other factors are the same. In Equation (1) this difference is accounted by reducing the aerodynamic resistance in the canopy case, and I think this is what Rotenberg and Yakir (2010) meant in the

definition of CCE. Therefore, a study that wants to better describe the CCE phenomenon should focus on comparing turbulent transport characteristics across different canopies and bare soil, probably for different stabilities, but not only the stability difference in one canopy, as this difference is already expected. Therefore, I believe that the CCE should not be the motivation of this manuscript.

2. I'm surprised with the results of increased r_H with increase negative Ri_B (increase instability) for the non-MOST models. I believe all models try to replicate the overall idea that turbulent transport increases with instability, and after a quick look on the equations and original manuscripts, it seems to me that r_H should decrease with instability in all models, therefore I'm confused about the results shown in Figures 3 and 4.

3. The description of the temperature field simulated with LES is not complete. Dias-Junior et al. (2015) simulated only a near neutral case, and Patton et al. (2016) included a source profile term in the temperature equation which comes from the land-surface model, which is not present in the simulations presented here, therefore they cannot be used as references for some of the details of the simulations performed here. In Table 2, the value of $\overline{w'T'}_s$ is defined as being at the ground, where the same values used in Patton et al. (2016) were defined at canopy top. When looking into Figure 1, it is not clear where the imposed heat flux value is, as nowhere in the profile there is a match with the imposed values. If the heat source is applied in the SGS part of the model, and Figure 1(e) shows only the resolved part, maybe the resolved + SGS part of the heat flux should be presented instead. Also, the final profiles of temperature have a peak at canopy top, also different from the results obtained by Patton et al. (2016). Although this may not affect the final conclusions, the equations, sources and boundary conditions used in the temperature field of LES need to be clarified.

Minor specific comments:

- Introduction: include a paragraph describing why better estimations of r_H are needed, even though this is a parameter poorly defined for atmospheric transport. I believe the conclusion has some of the information that could be in the intro.
- Section 2.2: emphasize here that these models were developed for conditions different from canopy sublayer, again this is in the conclusion but should be discussed earlier in the manuscript. This can be a major cause of discrepancies between the models and the simulation, which could be tested by performing simulations without canopy and comparing with the models. After a quick look, I could not find such a test in the literature.
- Section 4.1: if possible, when describing the figures in the text, give some justification of the result encountered, for example, if the variations with instability observed makes physical sense.
- Page 7, line 21: mention how the eddy diffusivities were estimated.
- Page 10, line 6: describe how these profiles where estimated. Which values came from LES, which are constant, which are a function of height, for example?
- Page 11, line 5: not clear what "for weaker cases" mean. Do you mean for weaker instabilities?

Technical corrections and minor suggestions:

- Why "(in)stability"?

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Discussion paper

- Figure 3: I suggest to use z/z_i (where z_i is the top of the ABL) instead of z/h, emphasizing that the entire plot is above the canopy. It can help to discuss the region where MOST (and therefore some of the models) is valid (surface layer).
- Figure 4: I believe that the blue captions below the figures are wrong.

References

Dias-Junior, C. Q., Marques Filho, E. P., and Sá, L. D. A. (2015). A large eddy simulation model applied to analyze the turbulent flow above amazon forest. *Journal of Wind Engineering and Industrial Aerodynamics*, 147:143 – 153.

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