Manuscript number: acp-2017-498
Title: **The influence of idealized surface heterogeneity on virtual turbulent flux measurements**
Authors: F De Roo and M Mauder

Reviewer recommendation: *major revisions.*

**Summary:**

The authors investigate a long-standing problem: the non-closure of the energy balance observed in eddy-covariance measurements. The employ large-eddy simulations over heterogeneous terrain to perform measurements of the energy balance for virtual towers located in their domain, while varying the surface heterogeneity from case to case. In general I believe their results might be interesting to the research community, but I do have some critical comments that need to be addressed first.

First, their analysis is based on an energy balance formulation which is (atleast from dimensional point of view) not correct. Second, their results seem to indicate that advection due to surface heterogeneity plays a key role for the imbalance, but they do not compare their results against a homogeneous control run to prove that their results are not an artefact of insufficient averaging. Third, their analysis is limited to a height of 50 m above ground, which is way higher than the height of eddy covariance measurements. Fourth, the length of the paper (the number of figures) appears too long for the rather short message of it. To me, this research is better suited to be published as a research note (mainly because of my fourth point). And lastly, there are language issues, including wrong use of English language,ambiguous statements, and formatting issues. It makes the reading of the manuscript a bit hard so the authors should make some effort to formulate precise and correct sentences.

I hence recommend to reconsider this paper for publication after major revisions.

A list of more detailed comments are given below.

**Detailed comments by the reviewer:**

<u>Major comments:</u>

1. Energy balance (Eq. 3)
I am struggling with this equation due to several reasons. First, the dimensions do not fit. A flux should be given in K m/s. The storge term, however, is the integral of $d\theta/dt$ over all three dimensions in space so that the resulting dimension is K / s * m³ = K m/s * m², which is NOT a flux. The equation thus can't be correct (this applies to the original equation in Eder et al. 2015 as well). Second, the authors refer to the second right hand side term as the flux divergence. That is not correct. The divergence involves the derivation in space, but here only a sum (difference) is calculated. Using the divergence, the equation would start to make sense, as the dimension of a flux divergence is K/s, which fits to a storage term ($d\theta/dt$). Third, to me it would make more sense to put storage on the left hand side and the rest on the right hand side. Fourth, why is the storage term integrated over a volume? Which volume? Shouldn't ALL terms be integrated over the entire control volumes? Is it really an integral or an averaging? Moreover, you name the $\langle w\rangle\langle\theta\rangle$ terms "vertical advection" and so on. But a turbulent flux is also advective, so this is not correct language. You mean the advection by the mean flow, which the flux divergence represents the advection by the turbulent flow?! Lastly, The summation over the lateral fluxes seems to work, but I expect the same summation over the vertical fluxes, i.e., $w'\theta' + H$, but even in the equation it seems to be a difference and not a sum ( $w'\theta' – H$ if you put all terms on one side). In Summary, the equation is very difficult to understand, and reading the Eder et al. paper does not really contribute to the understanding. Finally, I am also not sure that you can apply your averaging procedure for such a large control volume. Temperature, e.g. has a logarithmic profile near the surface, so you must

be careful when integrating the storage term over all control volumes.

2. Homogeneous control run
From Fig. 2 it becomes obvious that the EC flux cannot make up for the available energy. I think that is the main point of the paper. It is shown nicely, that advection (of the mean flow) plays an important role, and also flux divergence. You relate this to the heterogeneous surface, but you do not prove it. What you need to do is to add results from control runs with a homogeneous surface. What do you expect to happen? At least advection should go to zero, because all velocity components should go to zero when sufficiently averaged.

3. Measurement height
I do understand why you did your virtual measurements at an elevated height because of a relatively coarse grid spacing and you need 5 grid volumens. However, I could not find the grid spacing in the text, so I had to infer it from the table.The main point, however, is – and you state that yourself – that EC measurements are performed much closer to the surface (2 m) so that it is completely unknown whether your results are transferable to 2 m height. Maronga and Raasch (2003) showed, e.g., that the effect of secondary circulations (heterogeneity effect) tends to zero close to the surface and is most pronounced at upper levels. To me it remains unclear what the benefit of your study is to the research community.

4. Figures and length
I suggest to shorten the paper and stick to the main message. Not all figures are needed I think. Especially the "special plots" are difficult to read given the rather simple message you want to convey. By the way, figure 1 could be plotted nicer.

5. Language
You should be more careful with language editing before submission. I understand that correct English is an issue for non-native speakers, but please avoid wrong syntax and incorrect formatting. Examples: P1 L4: there is an extra "was", at several points you use the phrase "in function of", which to my knowledge simply does not exist, P7 L14: "Table table:2", wrong formatting. Referees spend their rare time for reading your manuscript without having any benefit from it. Beeing confronted with such carelessness suggests that the authors did not read their own manuscript carefully – and this does not really motivate to review your manuscript in-depth. See my detailed language corrections below.

<u>Minor comments:</u>

1. P2 L17: Why is there a correlation between the friction velocity and the energy balance closure? Also: there is nothing as a "good" correlation. It has a value, and can be possible regarded as "high".

2. P2 L19: You talk about secondary circulationy by self-organization, but I am not sure this contradicts the definition of a secondary circulation. Imagine a self-organized flow (give an example!), then what is the difference between primary and secondary circulation? I consider the hexagonal patterns for instance as the typical case of self-organization in the CBL, but that is what we call the primary circulation.

3. P2 L29: Secondary circulations do not "decay" to zero at the surface! They are simply not there.

4. P3 L5: Why is w=0 at first grid level?

5. P3 L19: The statement does not make sense. You can't say that a different partitioning of sensible and latent heat fluxes led to a higher Bown ratio, because that is simply the definition of the Bowen ratio!

6. P4 L19: What is "strong convection"? All your simulated cases are without mean wind, so you have free convective conditions in all your simulations.

7. P5 L16-31: You state that Neumann conditions are used at the lower boundary, which means you are prescribing surface fluxes. Then, how to you prescribe the surface momentum fluxes? With what values? Somehow you need to take into account the surface roughness, but for that you will need to use Monin-Obukhov Similarity Theory.

8. P6 L4: How can a tower be homogeneous? The surface around its base can, though.

9. P7 L14: There is no dataset in the table.

10. P7 L31: “worst imbalance of only 69%”. I guess you mean closure, not imbalance.

11. P8 L1: finally you mention that you are talking about the “mean advection”, but it should read “advection by the mean flow”.

12. P8 L25: You start the second sentence nearly exactly as you have finished the preceding one. That sounds odd.

13. P8: You discuss the difference between kilometer and hectometer scale heterogeneity; but from previous studies it is known that only those can trigger secondary circulations whose scale is in the order of magnitude of the boundary layer depth. Now I am wondering whether you took this into account or not. In the end, you do not show secondary circulations at all, so it remains a secret whether your findings are related to the scale of the heterogeneity of the ratio of the scale of the heterogeneity to the boundary layer depth. Or do you see local circulations that were not seen in previous studies? If yes, you should show them. You only provide very little evidence here.

14. P9 L13: Strictly, the friction velocity is zero in free convection as the mean wind is zero, so how can it increase for stronger circulation patterns? You must describe that you treat u* as a local quantity and that the primary circulation creates local wind shear near the surface (if that is what you mean).

<u>Language / typos (just an outline):</u>

1. P1 L1: “pending problem” - language.
2. P1 L4: extra “was”, “boundary-layer scale”
3. P1 L14: “order of magnitude” instead of “decade”
4. P2 L1: “Earth's”
5. P5 L3: influence on what?
6. P5 L18: (and throughout the text): “in function of” -> “as a function of”?
7. P6 L6: what is the “?” for?
8. P6 L27,29: Decide: “Hz” or “Hertz”
9. P7 L19: “time steps”
10. P7 L24: “we now plot” - rewrite.
11. Fig. 2: what does “in casu” mean?
12. Tab. 2: What does “1.4 ? 2.2” mean?
13. Tab. 2: “-36,1” should read “36.1”,
14. Tab. 2: I'd more like a long-list with all simulations
15. Tab. 2+3: What is the Boundary-layer height in “short” (1.4 – 2.2), while the Obukhov length is in “long” (From ... to ...)?