Reply to the reviewers

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We thank the reviewers for their careful reading and constructive comments. We think that our implementation of the comments has improved the discussion paper and made it clearer for a wider audience.

Our responses are in italic, and our changes in the manuscript are straight. We have used a brown color for the part of the comments of reviewer 2 that we already incorporated in the original Discussion paper, and a blue response for the recent changes.

1 Reviewer 2

1.1 Major comments

1. Energy balance (Eq. 3)

   First of all, we made a typo when typing the storage term (not in our data processing) but other than that the main misunderstanding seems to come from our application of the divergence theorem. Applying Gauss-Ostrogradski (or Stokes-Cartan if need be) is a common procedure in a mathematical treatment of fluid dynamics and is textbook material, see e.g. "Fundamentals of Momentum, Heat and Mass Transfer" by Welty et al.

   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/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).

   Thanks for catching our typo in the equation, the dimensions of course work out as they should. We added the normalization factor for the volume averaging (so the integral turns into an average). The volume integral is normalized by the surface area, which is the same as taking the average line integral over z, the way it is presented now. We already corrected this in the discussion paper.

   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/dt).

That term is the average flux-divergence over the volume, it's just reformulated into a difference. The flux divergence at a point indeed has a spatial derivative, but after integration over the control volume (and normalization) the derivative can be removed.

Third, to me it would make more sense to put storage on the left hand side and the rest on the right hand side.

Whether it's a plus sign on the right or a minus sign on the left it's a matter of taste. The surface flux is our constraint, we put all other terms on the right where they retain a plus sign.

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?

Which volume: "the" volume of the control volume. All terms are integrated over the entire control volume, but the storage term is the only term that cannot be (analytically) reformulated into a surface term because it has no (spatial) divergence in front.

Moreover, you name the $< w >< \theta >$ 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?!

We replaced the shorthand "advection" by "advection by the main flow". Flux-divergence is a standard term so we keep it.

Lastly, The summation over the lateral fluxes seems to work, but I expect the same summation over the vertical fluxes, i.e., $\overline{w'\theta'} + H$, but even in the equation it seems to be a difference and not a sum ($\overline{w'\theta'} - H$ 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.

It has to be a difference in order to close the energy balance over the control volume. The convention is that a positive H is directed upward. Imagine that there were no advection and no storage, then the turbulent flux should equal the surface flux. Hence $\overline{w'\theta'} - H = 0$.

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.

While it is in principle true that we have to be careful when integrating a logarithmic profile by adding the interpolated values, in our convective simulations the log *profile is only in the first gridpoint (by applying MOST) and due to the smallness of the roughness length compared to the half-height of the gridsize the argument of the logarithm is very large, where the behaviour of the log can be well approximated by a linear function, leading to a good approximation of the integral by the sum of the gridpoint values. Therefore the error is quite small. Anyway, the storage term is small on its own, so this is only a small correction on a small term.*

In the manuscript:

Where possible, the Gauß-Ostrogradski theorem [1] has been used to reformulate a divergence within the control volume as a surface term. Due to the choice of a cuboid aligned with the coordinate system for the control volume, the control volume energy balance (4) simplifies further because only the velocity components perpendicular to the faces remain.

[1] The Gauß– Ostrogradski theorem or "divergence theorem" is a special case of the Stokes-Cartan theorem in differential geometry. For our purposes, we also restrict ourselves to three-dimensional space. We consider a compact volume V with a piecewise smooth boundary S. If $\mathbf{F}$ is a continuously differentiable vector field defined on a neighborhood of V, then:

$$\int_V (\nabla \cdot \mathbf{F})\ \mathrm{dV} = \oint_S \mathbf{F} \cdot \mathbf{dS}\,. \qquad (1)$$

The left side is a volume integral of the divergence of the vector field $\mathbf{F}$ over the volume V, with dV the volume element, and the right side is the surface integral over the boundary of the volume V. $\mathbf{dS}$ is the outward pointing unit normal field of the boundary $S = \partial V$ multiplied by the surface element. For our purposes we take $\mathbf{F} = \mathbf{v}\,\theta$ and V is the control volume described in the text.

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.

   We do have homogeneous control runs, more precisely the cases when the amplitude of the surface flux is zero ($A_x = A_y = 0$): then the terrain is homogeneous because in our setup the heterogeneity can only appear in the spatial variation of the surface flux. We would like to stress that also in homogeneous terrain the energy balance is not necessarily closed, see e.g. Kanda et al (2004), because turbulence structures can exist for longer than e.g. half an hour. We agree that when "sufficient averaging" is applied in homogeneous terrain (assuming stationarity of the turbulence) the EBR should be one.

   In manuscript: 3.1 Circulation patterns in heterogeneous terrain

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.

   We added the grid spacing and the other important dials of Table 1 in the main text (see also the first comment of reviewer 1).

   In the methods: Relevant parameters of the simulation setup are summarized in Table 1, the grid spacing is 10 m in all three dimensions and the domain size is 6×6 square kilometers in the horizontal, and 2.4 km in the vertical. The boundary conditions of the simulations are periodic in the lateral dimensions.

   While we strive towards simulating real EC measurements performed at 2 m height (which is extremely costly computationally because it requires less than 0.5 m resolution, without suitable model manipulations that we are working on) these simulations are a first step towards that goal, at the time we used the computing time available to us to produce the 288 runs. This article provides a method that can be repeated for higher grid resolution. Though the domain averaged energy balance of Maronga and Raasch (2013) tends to zero close to the surface, we do not think it means that it's absent (in fact, in reality the non-closure is there at 2 m) and we are still investigating this. There are some issues related to high-resolution LES near the surface, not only related to the computational demand, so we think this problem merits additional research. One of the major benefits of our approach is that we can study the energy balance closure locally in 2D complex terrain.

   Added in the conclusions: By means of a control volume approach, we decomposed the modeled surface energy budget to highlight its partitioning, and we have shown that the modeled energy balance ratio exhibits values that are found in field experiments. In addition, this approach allows us to investigate the energy balance closure in two-dimensional complex terrain.

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.

   We combined plots 2 and 3 into plot 2 (new) and 5 and 6 into plot 4 (new). We inserted different markers for each tower in figure 1.

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.

We apologize for the typos that we didn't catch beforehand and thank you for your time and efforts. We already incorporated these corrections in the Discussion paper.

1.2 Minor comments

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.

   Corrected: good → high

   In the results: In the literature (e.g. Stoy et al.,2013; Eder et al., 2015b) a correlation between friction velocity and energy balance closure has been found: a high friction velocity leads to a smaller residual. Typically, a higher friction velocity is correlated to smaller atmospheric instability, and hence roll-like convection instead of cellular convection. Maronga and Raasch (2013) found that boundary-layer rolls "smear out" the surface heterogeneity, leading to an effective surface that looks less heterogeneous, which has been related to a higher EBR (Mauder et al., 2007; Stoy et al., 2013). Therefore a possible cause for the present low correlation of u^* with the EBR, could be our range of the stability parameter. For the free convective cases considered here the stability parameter lies below the range where the friction velocity has a high correlation with EBR.

2. P2 L19: You talk about secondary circulations 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.

   We agree that the hexagonal patters should be called primary circulation in the homogeneous case. The secondary circulation is the additional circulation due to the presence of the heterogeneity (insofar it is a small effects that can be superimposed upon the primary circulation, as the NS equations are nonlinear). However, in our case the hexagonal patterns arising from the convective conditions are preferentially tied to the landscape heterogeneity in the case of O(km) heterogeneity, so the primary and secondary circulation become indistinguishable.

   In the introduction: In the case of cellular convection in heterogeneous terrain the distinction between the primary and the secondary circulation becomes blurred, when the convection cells are tied to the landscape heterogeneity.

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

   Rewritten: "they are not present at the ground level".

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

   The lowest level (I would prefer to call it level zero) is the level where the boundary condition is applied. By constructing in the LES, there is no vertical velocity at the ground level which is supposed to be a hard boundary, not a movable or permeable membrane. We summarize this as a "rigid no-slip boundary condition" (added in the text) which implies zero values for the horizontal and vertical wind components.

   In manuscript: Firstly, from continuity we indeed expect no vertical meso-scale transport by advection with the mean flow at the lowest grid point representing the lower surface, since w=0 due to the rigid no-slip boundary, but horizontal flux-divergence plays a role, too.

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 Bowen ratio, because that is simply the definition of the Bowen ratio!

   We didn't intend to say this. The first subclause states the finding (Bowen ratio depends on the scale of the heterogeneity), the second makes it more precise (Bowen ratio is largest for intermediate scales).

   Rewritten: Brunsell et al. (2011) found that the partitioning between latent and sensible heat was affected by the scale of heterogeneity. More precisely, the intermediate scales yielded a higher Bowen ratio.

   This is one of the results of their article, and we don't want it to be understood as a definition.

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.

   We rephrased it as "strongly convective conditions". Taking the Obukhov length as the criterion for the strength of the instability of the CBL we have cases that fit the tag "strongly unstable" of Patton et al (2016) for which they take $L \approx -10\,\mathrm{m}$. For purely free convection we should at least have $|L| < 1$ which is not satisfied for all cases. Even though we do not have a synoptic background wind, the circulation between the patches generates a nonzero u^* that increases L. When we determine the instability from surface variables (u^* and $\overline{w'T'}$) we cannot call all our cases free convection (in reality we would determine it from the EC tower, so we only have knowledge about the surface variables).

In introduction: To disentangle the influence of the surface heterogeneity from that of the meteorology, we will focus on a set-up of free convection without a synoptic wind (which will effectively lead to strongly to freely convective conditions diagnosed by the virtual towers).

In the introduction: In addition, as both the lack of closure and the strength of the circulations are most pronounced for strongly convective conditions, we will likewise focus on (effectively) strongly unstable conditions to free convection, we will likewise focus on strongly unstable conditions to free convection, with the unstability parameter $-z/L$ ranging from 1 to 5000. The $-z/L$ is different from ∞ because the convective conditions lead to cellular circulation patterns, which locally induce a friction velocity at the surface, and due to its positiveness, there will also be a horizontally averaged u^* different from zero. From the perspective of the tower measurement, by eddy-covariance measurements alone it cannot be distinguished if a measured u^* follows from the wind aloft or locally from the convection-driven circulation. In addition, the circulation locally leads to advective terms that can influence the energy balance closure: e.g. near an updraft there will be horizontal convergence in the flow field. Even in homogeneous terrain these advective terms can lead to a non-closure of the surface energy budget (e.g. Kanda et al 2004).

We have also added an analysis of the dependence of the (horizontally averaged) u^* on the amplitude of the surface heterogeneity. In homogeneous terrain a stronger surface heating leads to stronger circulation patterns and therefore the dependence could be complicated. However, it turns out that the domain-averaged u^* drops monotonically with stronger amplitudes of the surface heterogeneity.

In the results: On the topic of circulations driven by a surface conditions that are by design freely convective, we investigate how the domain average of u^* is influenced by the surface heterogeneity. The ratio between the surface flux at the hottest patch and the surface flux at the coolest patch is given by:

$$r = (1 + A_x + A_y + A_x \cdot A_y) \times (1 - A_x - A_y + A_x \cdot A_y)^{-1} \ . \tag{2}$$

The horizontal mean of the friction velocity scales very well with the natural logarithm of this ratio,

$$u^* = -0.046 \ln(r) + 0.384 \ , \ R^2 = 0.85 \tag{3}$$

The remaining spread in u^* does not result from the time stamp or the heterogeneity length scale. The monotonous decrease of u^* in function of the heterogeneity ratio shows that for more homogeneous terrain we will obtain a slightly larger domain averaged u^*.

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.

For the velocity components, Dirichlet conditions are applied. The Neumann conditions are for scalars like potential temperature. There is a roughness length (indicated in table 1) and the first gridpoint is parameterized with MOST. We added a clarification of the model, as demanded by reviewer 1, including a more thorough discussion of our boundary conditions.

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

   It was shorthand for a tower situated in locally homogeneous terrain. We rephrased this.

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

   The table summarizes the simulation parameters within that suite. We clarified this in the text and in the table captions.

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

    Indeed, we meant lowest mean closure, corrected.

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

    We replaced "mean advection" everywhere by "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.

    We removed that sentence.

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.

    The references show that also landscape heterogeneity of scale smaller than the boundary layer depth has a signature on the spectrum of the heat flux (e.g. Brunsell et al 2011) and can affect the EBR (Schmid et al 1990). We improved our choice of words and replaced "secondary circulations" by "secondary circulations at the landscape up to the boundary-layer scale". We would expect a dimensionless parameter such as the scale of heterogeneity divided by either L or by z_i (these are the most obvious choices, *the former for local circulations, the latter for secondary circulations spanning the entire boundary-layer). Because our heterogeneity length is limited to four values (after all our domain has to be periodic) this is not enough to do the correlation analysis. Nevertheless it is an interesting idea to simulate different heterogeneity lengths (and a larger range of u*) to investigate correlation with L_{het}/L or L/z_i but this study focuses on 288 simulations of strongly to freely convective conditions. We added a subsection on the local circulations.*

Added in the discussion: We start our analysis with a discussion of the location of the updrafts and downdrafts in heterogeneous terrain. For this purpose, we concentrate on a few specific cases, more precisely $A_x = A_y = 0.3$ and all four heterogeneity lengths (with $L_x = L_y$). We will take the mean vertical velocity as the simplest proxy for circulation patterns in the boundary layer. In Fig. 2 we plot the time-averaged vertical velocity at the height of the control volumes (50 m). We stress that the structures at 50 m extend into the mixed layer above where the absolute velocities become larger (not shown). The reason for the additional time average (over the complete virtual measurement interval of 4 hours) of the already hourly mean data is to remove the drift of the turbulent structures. We notice that for the heterogeneity lengths of O(km), the motions within the mixed-layer clearly reflect the surface pattern, with updrafts concentrated above the hotter patches and downdrafts above the lower patches in the 3-km heterogeneity length, and a little offset in case of the 1.5-km heterogeneity length. However, the structure of the convective turbulence for both kilometer scale are clearly different from homogeneous control run, where typical cellular convection patterns arise (Schmidt and Schumann 1989), though the hectometer scales are qualitatively rather similar to the homogeneous run. The latter could be a consequence of the blending height. Investigating the heterogeneity lengths of O(hm) with more horizontal detail for the time-averaged w, we do not see clear updrafts or downdrafts tied to the surface heterogeneity. However, in this respect it could be interesting to note that some of the hourly mean vertical velocity (without additional time-average) for the O(hm) appear better related the surface structure. Similar results appear for weaker amplitudes and also when A_x is different from A_y, in which case the dominant pattern is visible along the direction with the larger amplitude (not shown). We can conclude that circulations are tied to the landscape heterogeneity when it is of O(km). For the O(hm) such a correspondence is unclear. However, the latter could be related to the "coarse" grid resolution and the distance from the ground. Indeed, Mauder et al (2010) found persistent updraft and downdraft regions during the 2008 Ottawa field campaign.

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).

    Yes, we clarified our description. Nevertheless, the circulation pattern that arises *from the surface patches leads to a mean nonzero u^* as well, because u^* is always positive. (This does not contradict the mean zero mean wind because that has a direction so the average becomes zero. Hence, locally the flux-gradient assumption can still hold, but it doesn't hold for the area-average of the simulation domain.)*

In the methods: In addition, as both the lack of closure and the strength of the circulations are most pronounced for strongly convective conditions, we will likewise focus on strongly unstable conditions to free convection, we will likewise focus on (effectively) strongly unstable conditions to free convection, with the unstability parameter $-z/L$ ranging from 1 to 5000. The $-z/L$ is different from ∞ because the convective conditions lead to cellular circulation patterns, which locally induce a friction velocity at the surface, and due to its positiveness, there will also be a horizontally averaged u^* different from zero. From the perspective of the tower measurement, by eddy-covariance measurements alone it cannot be distinguished if a measured u^* follows from the wind aloft or locally from the convection-driven circulation. In addition, the circulation locally leads to advective terms that can influence the energy balance closure: e.g. near an updraft there will be horizontal convergence in the flow field. Even in homogeneous terrain these advective terms can lead to a non-closure of the surface energy budget (e.g. Kanda et al 2004).

1.3 Language

1. P1 L1: pending problem - language.

   Corrected: unsolved problem

2. P1 L4: extra was, boundary-layer scale

   Corrected

3. P1 L14: order of magnitude instead of decade

   Corrected

4. P2 L1: Earth's

   Corrected

5. P5 L3: influence on what?

   On virtual flux measurements. Rewritten.

6. P5 L18: (and throughout the text): "in function of" → "as a function of"?

   We have removed all occurrences of "in function of".

7. P6 L6: what is the "?" for?

   Missing latex reference. Corrected.

8. P6 L27,29: Decide: "Hz" or "Hertz"

   We confused it with the writing of e.g. "1-Hertz sampling" Corrected.

9. P7 L19: "time steps"

   Corrected

10. P7 L24: "we now plot" — rewrite.

    Rewritten, it's in the same plot as well because we condensed the figures.

11. Fig. 2: what does "in casu" mean?

    "In casu" means "in this case". We removed it.

12. Tab. 2: What does "1.4 ? 2.2" mean?

    Should have been a dash. Corrected.

13. Tab. 2: -36,1 should read 36.1,

    Corrected.

14. Tab. 2: I'd more like a long-list with all simulations

    This would yield two tables with 144 rows (6x6x2x2 cases) and 7 columns, which is basically the information in tables 2 and 3.

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 ...)?

    The Obukhov length is negative for unstable conditions and therefore the dash distracted. Both are now in the long format without a dash.