First of all we want to thank the three reviewers for the helpful comments. We have already answered the main problems of the reviewers in a letter to the editor on January 24, which was distributed to all reviewers: https://www.biogeosciences-discuss.net/bg-2017-450/bg-2017-450-AC1-supplement.pdf. Therefore, in the following we will answer only the specific problems. In accordance with these general comments we have made large additions to the Introduction and deleted parts about the ACASA-model that were repetitions from earlier publications. Comments from referees are presented in black and these are followed by the authors' responses in blue.

Reviewer #1:

Specific comments:

P1L19: 30% of unclosure for which situation (mean on a lot of sites?)

P1L19: The paper is an overview paper.

P2L12: I'm confused by the use of the term "forest-clearing transition". Do you mean that your fluxes are both (MT and TM) affected by coherent structures because the two towers are close to the forest-clearing transition? After reading your paper, I rather had in mind that the forest tower fluxes were only weakly affected by the presence of the clearcut and that the clearcut fluxes were also only weakly affected by the presence of the forest. So rather than applying the model to a forest-clearing transition, you apply it for a forest and for a clearing.

P2L12: See general remarks.

P2L11: Group the two paragraphs, you are developing the same idea

P2L17-18: repetition. Already stated on line 6-16. You can delete this sentence and introduce the refs elsewhere.

P2L11, and L17-18: was done.

P2L19-20: "Additionally, it is evaluated whether the energy balance closure corrected flux measurements better fit the fluxes simulated by ACASA". This objective is embedded in the previous one so which are you using "additionally"?

P2L21: "Field measurements of the FLUXNET site 'Waldstein-Weidenbrunnen' (DE-Bay) were therefore complemented by additional measurements". Which ones? Be more precise.

P2L24: "to model the energy and CO2 exchanges of different vegetation types"

P2L19-20, L21, L24: Text was re-formulated and supplemented with additional text.

P2L28: "The experimental data for the initialization of the model and the comparison of the results" could be replaced by "The experimental data for the initialization of the model and the evaluation of its outputs"

P2L28: was done.

P3L13-14: "where z is the measurement height normalized by the stand height hc". You probably mean: "where z is the measurement height and hc is the stand height"

P3L13-14: was corrected.

P3L13-14: "The understory comprises two-thirds crinkled hairgrass (Deschampsia flexuosa) and moss (together LAI of 0.5m2 m-2 and less) and one third characterized by blueberry (Vaccinium myrtillus) and young Norway spruce (Picea abies, together PAI of 3.5m2 m-2)". Two thirds and one-third on which basis? And what means "and less"?

P3L13-14: was re-formulated.

P4L10: "In the majority of cases, high-frequency gas analyzers for carbon dioxide (cCO2) and water vapor (q) were installed in conjunction with sonic anemometers". Why "in the majority of cases"? Please rephrase

P4L10: was re-formulated.

P5L17-18: "This method allows the calculation of the temperatures of these components without also making substantial errors in the case of significant deviations from the ambient temperature". Hardly understandable.

P5L17-18: was re-formulated.

P5L23: "Bell-Berry stomatal conductance". I guess you mean "Ball-Berry"

P5L23: was corrected.

P6L3-4: R0 is defined as the respiration rate at 0 °C but Equ.1 will not give RT(Ts=273K)=R0. Probably Ts should be expressed in degree Celsius instead of Kelvin.

P6L3-4: This part was deleted because this was already published.

P7L22: "For the correction of the energy fluxes, the residual (Res) arises from the following assumption:". Equ. 3 that follows is the definition of the residuals, I don't see any assumption there.

P7L22: was re-formulated.

P7L25-27: not necessary to cite Haverd and Lindroth twice. Please reorganize

P7L25-27: was deleted.

P7L29: "with mf the biomass of the forest". You mean the above-ground biomass?

P7L29: was corrected.

P9L1-2: "This method is usually utilized for the correction of heat fluxes under the assumption of measuring errors, ...". Repetition from the previous sentence. Please rephrase.

P9L1-2: We do not see any repetition.

P9L9: "The discrepancy between measured and simulated NEE can be an effect of the unclosed energy balance on the CO2 fluxes". I understand what you mean but this is a complicated way of saying that if CO2 exchanges share the same transport processes than heat exchanges (scalar similarity), measured CO2 exchanges should be underestimated on the same level as heat fluxes. And this hypothesis being far from widely accepted, this point should be discussed further.

P9L9: We do not say that scalar similarity is fulfilled. We say that the methods can only be applied if the scalar similarity is fulfilled. Ruppert et al. (2006) have shown that this is not always the case. Therefore the agreement of measured (corrected) and modelled fluxes is not always fulfilled. We have added a short statement about the reference (Ruppert et al. 2006).

P9L20: "whereby a spectral method of the flux averaging of surface characteristics (roughness length) according to Hasager and Jensen (1999) is employed". I do not understand this part of the sentence. Please be more explicit.

P9L20: We have added some more details: Because of the non-linearity, you have to average the friction and not the roughness length.

P10L5-6: "However, it has been found that the energy balance closure for the sensible heat, the latent heat, and the NEE was better for the buoyancy flux correction, but the results are partly inconclusive". Be more precise, what mean "partly inconclusive"?

P10L5-6: This part was deleted.

P10L15-19: This § should be reorganized to avoid repetitions.

P10L15-19: P10L1-13 were deleted.

P10L23: Remove "thus", there is no causality link with the previous sentence. You just start the explanation of the previous sentence.

P10L23: was deleted.

P10L26-28: You probably switched westerly and easterly!

P10L26-28: was corrected.

P10L29-30: Why didn't you use only the common dataset at the two heights? It would avoid this problem.

P10L29-30: We have deleted the sentence because we cannot see a significant difference between the effect of the different heights and the slightly different numbers of data points.

P11L4-5: Use or at least recall the acronyms defined in section 2.4 (EBC-Bo, EBC-HB), it will be more explicit than "the sensible heat flux corrected with the buoyancy flux".

P11L4-5: We have added the acronyms.

P12L2-3: "Obviously, the integrated fluxes of the tile approaches for the whole clearing and the footprint of the turbulence mast for 5.5m height do not differ significantly.". I understand what you mean but literally you compare apples and pears. Please rephrase.

P12L2-3: was corrected.

P14L24-26: "Due to Bo > 1, the buoyancy correction overestimates the effect of thermal convection on the energy balance closure and the true correction might lie between both correction methods". I do not understand this sentence. Why EBC-HB should overestimate H when H is dominant? Is the explanation linked to the following sentence?

P14L24-26: The sentence is linked to the sentence before and therefore to the latent heat flux.

Fig. 5: Would be convenient if all the y-axis scales would be identical for the forest and the clearing. Would also be convenient if the colors/ markers used would be compliant with those udes in fig. 3 and 4.

Fig. 5: The scaling has been corrected. The markers/colors of Fig. 5 cannot be directly compared to Fig. 3/4, which includes two weighting methods plus the corrected fluxes (4 markers), where Fig. 5 compares the footprint-weighted tile-approach with the uncorrected, the buoyancy corrected, and the Bowen-ratio corrected fluxes. The colors used in Fig. 5 are compliant with the colors utilized in Fig. 6.

P17L9: "This could also be an overestimation by the measured fluxes due to the turbulence and the forest structure, discussed by Foken (2017b)". Please explain what you mean, in order to have this manuscript self-standing. It's a bit more explicit in the conclusion but should be moved here.

P17L9: Additional sentence was included.

P18L2: "According to the findings in Sect. 3.2.2 that a large contribution of the unclosed energy balance is a missing sensible heat flux, we used the modeled data as a reference for the validation of the correction methods". I guess that the first part of the sentence should be deduced by the reader based on fig. 5. It would be better to comment this feature already in sect. 3.2.2. Also, I do not understand the link between the first part of the sentence and the second part. Whether is measured H or measured LE the main responsible for the non-closure of the energy balance, you can decide to use the modeled values as the reference. Please clarify.

P18L2: We deleted the link to Sect. 3.2.2 and hope it is now clear.

P18L15-15: "Additionally, this indicates that the Bowen-ratio correction is not a method that is applicable for the correction of the measurement errors that occur". Confusing since the reader does not know if this assertion holds for a given range of Bo or for the whole range.

P18L15: We have re-formulated.

P18L17-18: "Due to the assumption of a similarity between the water and carbon dioxide fluxes (Ruppert et al., 2006), neither the NEE flux nor the latent heat flux were corrected for high Bowen ratios". Not necessary to cite again Ruppert et al. at this stage. Also, this sentence is not well integrated in the discussion and is ambiguous. I would prefer something like: "In all cases, attribution of residual energy to latent heat flux is low for high bowen ratios. Therefore, due to the assumption of a similarity between the water and carbon dioxide fluxes, the NEE flux was only marginally corrected in these conditions".

P18L17-18: We have followed your suggestion.

P19L4-5: "Due to high Bowen ratios and large underestimation by the model, the buoyancy corrected fluxes show better results in comparison with the model". This sentence brings nothing in this discussion, I would simply delete it. If you think it brings necessary information, please improve it to be more explicit.

P19L4-5: was deleted.

P20L21-23: "The better correction...could be a reason". Not understandable for the reader. Please rephrase/improve. And consider revisiting the writing style of the whole conclusion to avoid this "telegraphic style".

P20L21-23: was deleted and a new sentence was added.

Technical corrections:

P1L16: replace "stratification" by "atmospheric stratification"

P1L16: was added.

P10L15: "of the" appears twice

P10L15: was done.

Fig 2: Distance units are missing

Fig. 2: was done.

Fig. 4: you can simplify the legend by "same as fig 3 but for the second GDP"

Fig 4: was done.

Reference:

Ruppert J, Thomas C and Foken T (2006) Scalar similarity for relaxed eddy accumulation methods. Boundary-Layer Meteorol. 120:39-63.