

Interactive comment on “Application of the ACASA model for a spruce forest and a nearby patchy clearing” by Kathrin Gatzsche et al.

Anonymous Referee #1

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GENERAL COMMENTS:

In this paper, Gatzsche et al. test a third-order closure one-dimensional SVAT model (ACASA) for the prediction of heat, water and CO₂ exchanges over a heterogeneous clear-cut and a nearby spruce forest. They compare the modeled fluxes to turbulent flux measurements on a dozen days obtained in the middle of the vegetation season (July). Their specific aims are (i) to test the implementation of a so-called tile approach on the clearcut (weighting the modeled flux contributions of individual land-use according to different schemes to obtain the total modeled flux to be compared to the measured flux) and (ii) to test different scaling of measured turbulent fluxes to correct for underestimation of these fluxes diagnosed through a deficit in the energy balance closure. This scaling is not only applied to heat fluxes but also to CO₂ flux, postulating

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scalar similarity between heat and CO₂ transport. For this second objective, both the forest and the clearcut datasets are used.

This work is a follow-up of recently published research by Falge et al. (2017) on the same site and using the same datasets. More specifically Falge et al. propose in their section 16.3.4 ACASA simulations for the forest and the clearing. They show that ACASA is performing well for predicting heat, water and CO₂ exchanges for the forest and the clearing as well, using the “coarser” tile approach for the clearing, i.e. weighting the vegetation specific modeled fluxes by the percentage of land cover for the individual vegetation type within the whole clearing (with the eddy-covariance dataset at 2.25 m). They also test the two scaling proposed in the present paper (EBC-Bo and EBC-HB), already stating that the EBC-HB gives better results than the EBC-Bo.

Therefore I’m wondering what are the real additional and useful finding proposed in the present paper. Regarding objective (i) a finer weighting is proposed, using the respective footprint contribution of each individual vegetation type rather than its percentage of land-cover in the whole clearing. However, this finer approach did not give significantly different results than the “coarser” one because “the footprint model is probably not accurate enough in the location of the effect levels of the footprint, considering the small-scale heterogeneities of the clearing in comparison to the size of the footprint area.” (P20L8-9). Looking at the land-cover map provided in Fig 2, this result was very predictable to my opinion. Regarding objective (ii), the present paper investigates more deeply the conditions under which the EBC-Bo and EBC-HB seems to perform well (fig. 6). This goes beyond Falge et al. 2017. These points are explained in section 2.6 and previous papers on which the research is relying are duly cited but I had a hard time disentangling new findings from already published ones. Previous findings and precise objectives should be crystal clear already after reading the introduction which is not the case in the submitted manuscript. This flaw is also reflected in the manuscript title which is vague. Also, the added value of analyzing in the same manuscript the forest and the clearing should be explained in the introduction.

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I have also some concern on the quality of the writing. Repetitions, lack of precision in some places, some cumbersome within-paragraph structure. See the numerous specific comments for details. I also found that the manuscript was not enough self-standing, being too much elliptic on important concepts (e.g. the “tile” approach is not defined and the reader is referred to Molders 2012 which is a full book; see also specific comments).

Also, what is the interest of introducing the turbulent flux measurements in the clearing at 2.25 m? They had almost nothing to the story. Cannot it be removed from the manuscript? Same question for the turbulence tower (TT). It is not clear where these measurements have been used (P5L1 does not mention it but in the conclusion, it's mentioned).

Finally, section 3.2.4. is an evidence and I suggest to remove it. The fact that combining a robust model with the tile approach to simulate fluxes having large-scale heterogeneous land-cover within the footprint is quite obvious and does not deserve a fig. and this subsection. In addition, illustration of the different NEE for forest and clearing has already been presented in Falge et al., 2017 (fig. 16.15).

Only after these comments have been taken into account, it will be feasible to estimate whether a “critical mass” is reached to justify a full paper.

SPECIFIC COMMENTS:

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

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

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it for a forest and for a clearing.

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.

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”

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”.

P3L13-14: “where z is the measurement height normalized by the stand height h_c ”. You probably mean: “where z is the measurement height and h_c is the stand height”.

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”?

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.

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

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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.

P6L3-4: R_0 is defined as the respiration rate at 0°C but Equ. 1 will not give $R_T(T_s=273\text{K})=R_0$. Probably T_s should be expressed in degree Celsius instead of Kelvin.

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.

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

P7L29: “with m_f the biomass of the forest”. You mean the above-ground biomass?

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.

P9L9: “The discrepancy between measured and simulated NEE can be an effect of the unclosed energy balance on the CO_2 fluxes”. I understand what you mean but this is a complicated way of saying that if CO_2 exchanges share the same transport processes than heat exchanges (scalar similarity), measured CO_2 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.

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.

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”?

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See my general comment on this point.

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

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

P10L26-28: You probably switched westerly and easterly!

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

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”.

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.

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?

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 used in fig. 3 and 4.

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.

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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.

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.

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”.

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.

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”.

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TECHNICAL CORRECTIONS:

P1L16: replace “stratification” by “atmospheric stratification”.

P10L15: “of the” appears twice.

Fig 2: Distance units are missing.

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

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-450>, 2017.

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