Dear Dr. Ibrom,

Thank you very much for your comment and thanks to the reviewers for their comments. In response to the opinions that the final results of the reviewers are different from the main revisions (Rev. #1), the model is not up to date (Rev. #2), Biogeosciences is the wrong journal (Rev. #3) and the paper is similar to Falge et al. (2017), we are providing you and the three reviewers with some answers to the comments. Most of the specific points can be easily corrected or explained without significant consequences for the manuscript.

1. We agree that the paper looks quite similar to the publication by Falge. The Falge paper is a summary of the results of all model studies made in the last nearly 20 years at the Waldstein-Weidenbrunnen site, without any discussion of specific problems. However, reviewer #1 has selected the two points that are new in the present manuscript: (i) a more precise concept of the comparison of flux measurements and their footprints and 1D modeling with a tile approach; (ii) a detailed discussion of the effects of different energy balance correction schemata. We agree that we should highlight both points more concretely in the first two paragraphs of the introduction (P1L13 – P2L5 of the submitted manuscript).

2. As was mainly stated by **reviewer #2,** the 1D model ACASA is very old and not appropriate to our measuring site. **Here we disagree**. It is right that the basics of this model – mainly the higher order closure concept – were developed more than 30 years ago (Meyers and Paw U, 1986, 1987). The model was significantly updated by the University of Davis (Pyles et al., 2000; Pyles et al., 2003) and it is still in use (Falk et al., 2014). About ten years ago we started to work with ACASA. The first issue was to use a sensitivity analysis to check whether the model can be applied for a Central European spruce forest (Staudt et al., 2010). We also found some model-specific problems (Staudt, 2010), and the model was again updated by the University of Davis. Using the updated model, a first study for the Waldstein-Weidenbrunnen site was published (Staudt et al., 2011). This study showed that the ACASA model is more accurate (in comparison to a K-approach model) when coherent structures dominate at night. We have not highlighted coherent structures (see reviewer #1, P2L12), because a paper providing an overview of about 15 years research on this topic is available. We will include the reference (Thomas et al., 2017). The model was changed by Staudt et al. (2011) for the spruce-specific parameterizations, and Falge et al. (2017) determined several parameters of the plants of the clearing (Table 16.1). This history of the ACASA model is partly described in the first part of Section 2.3 of our paper. In the second part we have listed all changes made in the model according to Staudt et al. (2011) and Falge et al. (2017) for the clearing. We believe that this second part of Section 2.3 is necessary to ensure that the reader does not assume that we only used a version of the model developed by the University of Davis without site-specific parameterizations. From the given references in Section 2.3 it should be clear that we used a well-developed model and our paper is not a modeling study. In this point we **disagree with the position of reviewer #3** that the paper is not appropriate for Biogeosciences. We copied Table 16.1 from Falge et al. (2017) in our paper (Table 3) because we believe that these data are relevant for the understanding of the tile approach and that the reader should not need a book chapter for a complete understanding of our results; however, Table 3 may not be necessary.

3. According to the comments of all three reviewers, we found that we have not explained our measurement and model comparison concept precisely enough, which had only been done

with some references, e.g. the definition of the tile approach (e.g. reviewer #1): The fluxes measured with the eddy covariance technique are related to all surfaces on the up-wind side of the measurements, and the influence of each surface on the measured data is given by the footprint function. For further details see Leclerc and Foken (2014). If you want to compare these flux measurements with 1D models, this can be done over a homogeneous surface easily or over a heterogeneous surface if the model is parameterized for all surface characteristics and averaged according to the percentage of each surface in the footprint (tile approach). The footprint is due to changing wind velocity and atmospheric stratification that are different for each measurement (about 30 minutes), and the tile approach is also different for each measurement. The deficit of the Falge et al. (2017) paper was that all measurements were compared with the same tile approach for the clearing. This problem was solved in the presented paper. This concept works quite well for only two significantly different surfaces in the footprint (Foken and Leclerc, 2004; Göckede et al., 2005; Biermann et al., 2014), but in our case, with multiple surface characteristics, it partly fails. We will discuss this issue in more detail (now already P20L7-12) in comparison with the sensitivity study for footprint models (Markkanen et al., 2009).

4. The term "forest-clearing interaction" (P2L12) probably confused all reviewers and they assumed an advection study. We will delete this term, because the tile approach is not an advection study (for forest clearing interaction see e.g. Foken et al. (2017)). Reviewer #1 has indicated that the paper is not a "forest-clearing interaction" and proposed the deletion of Section 3.2.4. We would not delete it, but we have to give some more explanations of why it should be used and provide Fig. 7, which is only for modeled NEE similar to Fig. 16.15 of Falge et al. (2017). The NEE data measured at Waldstein-Weidenbrunnen site are used in many modeling studies within FLUXNET. For surface characteristics these often use MODIS data with a grid size of 500 m or 250 m. Since 2007, the satellite sees about 50 % forest and 50 % clearing in the relevant grid, and the authors compare these data with measured fluxes that have nearly 100 % forest in the footprint. We will highlight the problem and reformulate Section 3.2.4. Reviewer #2 and #3 propose the use of a 3D model rather than a 1D model. However, this would also need a different measurement strategy to directly measure advection. Up to now, experimental advection studies with an acceptable number of measurement points failed (Aubinet et al., 2010; Aubinet et al., 2012). Furthermore, the resolution of available 3D models (Sogachev et al., 2002; Sogachev and Lloyd, 2004) is too large (25–50 m) for the small scale heterogeneities in the clearing (5–10 m). By the way, a 3D LES study (model PALM) with 1 m grid resolution was made for the site, but only for a short time series (Kanani-Sühring et al., 2017); see also P20L14–16. Because our study is a comparison of experimental data and modeled data, both concepts must agree (see point 3). Furthermore, ACASA and PALM are available for free while most of the 3D models are commercial models. On the basis of these points **we disagree with reviewers #2 and #3** that the ACASA and footprint modeling should be replaced by 3D modeling. Footprint and 1D models (SVAT) are still widely used.

5. Besides the comparison of the footprint – tile approach for the clearing, the energy balance closure study was done in much more detail than was the case by Falge et al. (2017). In particular, the conclusion made by Falge et al. (2017) for both correction methods was strengthened. New is the proposed correction for NEE. Therefore, the scalar analysis by Ruppert et al. (2006) is necessary. We are sorry, but the title of this paper is confusing. By the

way, Foken (2008) is an overview paper about the energy balance closure problem and not a special field study.

We hope we have been able to explain why we disagree with the conclusions of reviewers # 2 and #3. If you agree, we would like to follow the proposed revision of reviewer #1 and apply the specific comments by the other reviewers. In this case, we would like to submit a revised paper within one month.

Sincerely,

Kathrin Gatzsche and Thomas Foken for all authors

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