Review of "Matching scales of eddy covariance measurements and process-based modeling - Assessing spatiotemporal dynamics of carbon and water fluxes in a mixed forest in Southern Germany" by Moutahir et al., submitted to EGUsphere, 2025.

(all line indications refer to the track-change version)

General comment

We are grateful to both reviewers for their specific comments and general feedback. We also acknowledge some deficits in the study which were pointed out by reviewer 2, and which we have tried to address appropriately. In addition, we've re-done the parameter calibration based on eddy-covariance measurements at all sites and for both species, leading to considerably better correlation results, particularly for Douglas fir. Thereby, we used only high-quality measurements and compared simulation results with measurements only (excluding the gap filled data). The new parameters are shown again in the supplement together with additional evaluation information (time series and statistics). Accordingly, we have re-done all simulations, which can be viewed now with more confidence than before.

As reviewers and editor recommended, we have also carried out some uncertainty analysis evaluating the influences of wind speed and direction or the assumption of displacement heights on the measurement results which, however, turned out to be not very sensitive. The impact of these considerations is also shown as additions to the supplement.

The analysis itself is emphasizing more on differences in seasonal developments and shows both transitional phases in the main document (while some longer periods are additional shown in the supplementary). Focusing on spring or on the non-growing period demonstrates that considering only the most abundant species (in this case beech) is not consistent with eddy covariance (EC) measurements from areas with deciduous as well as evergreen species. Focusing on Douglas fir only, however, clearly estimates NEE to be too negative in the non-growing period. Consequently, bottom-up ecosystem simulations also need to consider different species abundances if such EC measurements should be used for evaluation.

Still, we admit that the site is not ideal for demonstrating this result, which would be easier if species abundance would differ between main wind directions. However, as reviewers correctly pointed out, the species distribution is relatively homogenous and if some areas are significantly different from the mean, they only appear in very few footprints. Despite this limitation, we believe that by focusing on temporal rather than spatial differences, this article contains interesting findings that can build the basis for further analysis.

Point by point responses

Reviewer 1

General remarks:

The authors present a well-designed single-site study regarding footprint classified EC flux measurements to address tree species specific carbon and water budgets on different temporal scales. Overall, I only suggest minor changes.

The C budgets based on non-gap-filled and gap-filled fluxes differ considerably maybe due to more frequent gaps or fluxes with flag2 during winter. This is somewhat surprising to me because the use of the enclosed LI-7200 should prevent frequent gaps (by the way: Is the intake tube of the LI-7200 heated or not?).

Answer: Regarding the relatively high number of gaps, we admit that although the intake tube of the eddy covariance system is principally heated, this heating system was not working in autumn 2024 up until we exchanged the heated intake tube beginning of December 2024. Before this change, filters were often clogged after fog and rain events which was no problem anymore afterwards. The failure is now indicated in the description of the EC measurements (**chapter 2.2.1, first paragraph**).

Regarding model validation: Why only soil respiration and soil moisture were compared with in situ measurements? It would be nice to see the model performance also for NEE and ET.

Answer: The reason why we used soil moisture and respiration for evaluation was the availability of separate measurements for Douglas fir and beech dominated plots, respectively. This should indicate that the model is representing fluxes species-specifically in order to be sure that lumped up NEE and ET from eddy covariance measurements are correctly represented for the right reasons. The actual performance for NEE and ET is then presented in the scatterplots and the cumulative developments considering different options of gap-filling (regarding the measurements) and simulation setups (respectively different contributions of species according to the respective flux footprint).

The conclusions include the finding that the REddyProc gap-filling showed slightly higher correlation than process-based gap-filling approaches like LandscapeDNDC. So, the statement “the current analysis we could … demonstrate the suitability of process-based models for this task” should maybe complemented by the statement that the model doesn’t perform better than the usual gap-filling, in my point of view.

Answer: We don't want to give the impression to disqualify established approaches. Note that regarding this comparison, we also updated the results which still give similar values for both approaches (**see Fig. S8** of the supplementary). Accordingly, we added the demanded text as suggested (**L520**).

Detailed remarks:

L55: regarding footprint calculation references you should mention Kljun et al. already here: Kljun, N., P. Calanca, M. W. Rotach, and H. P. Schmid. 2004. A simple parameterisation for flux footprint predictions. Boundary-Layer Meteorology, 112: 503-523.

Answer: The reference has been included as suggested (**L58**).

L94: 7.8782°E instead of 7.8782°W

Answer: Corrected as suggested (**L102**).

L180: European instead of Europe

Answer: Corrected as suggested (**L213**).

Fig. 4: measured soil moisture instead of measured flux

Answer: Corrected as suggested.

Fig. 7: Regarding hourly resolution of NEE I would prefer the unit µmol m-2 s-1 instead of kgC ha-1 hr-1

Answer: We are sorry not to comply to this suggestion but because of consistency with actual measurements and simulation timesteps we would like to keep the unit as it is.

Tab. 3: mm hr-1 instead of kgC ha-1 hr-1

Answer: Table 3 is deleted because other comments suggested to put the statistics directly into the respected figures.

Fig. S8: during the non-growing season instead of during the growing season

Answer: Corrected as suggested (now Figure S7).

Reviewer 2

General remarks:

This is an interesting paper about our capability of measuring and modelling carbon and water fluxes from biodiverse forests in contrast to monocultures. The authors argue that biodiverse forests are said to be more resilient to climatic, environmental and biotic stressors and that hence it is important to distinguish exchange between monocultures and biodiverse forests as well as to identify the contribution of individual species to the total. This is indeed a relevant and timely topic in both measuring and modelling.

The authors use eddy covariance measurements from monocultures to train and evaluate an ecosystem model. Then they apply the model to a mixed forest (63% Beech, 27% Doulas Fir) and confront the quality of the results with EC measurements over that forest. In order to quantify the relative contribution of the two species, the authors use short-term flux footprint predictions (FFPs) overlain on a detailed species map.

The objectives, as stated in the introduction, are: 1) to assess the influence of the different species composition on LE and NEE in the mixed forest and 2) to analyze the uncertainty resulting from the assumption of a homogeneous footprint for mixed forest.

This suggests that the paper reports about a methodological question: To what level can we use EC measurements over a mixed forest in combination with footprint analysis to identify the individual contributions of species in a mixed forest. The conclusions, in contrast, focus on the quality of the model effort and less about the capability to measure the contribution of species to the whole.

In principle, I am positive to the aim of the paper and about the setup of the research. It addresses an important question and the methods are carefully chosen. Unfortunately, however, there are some limitations to this study too.

Answer: Thanks' a lot for pointing out this mismatch between objectives and conclusions. Along with some clarification in the abstract (**L23-29, L37-40**) We have modified and further explained the objectives in the end of the introduction (**L80-88**) accordingly to 1. assess the uncertainty when using eddy-covariance measurements for evaluation of vegetation models that are initialized at a specific footprint, and 2. to evaluate the flux contribution of two tree species and isolate their environmental responses. We think that this is more in line with the conclusions that have been formulated more cautiously considering the deficits of the study area.

The first is: the paper does not really answer the question how well, in principle, the combination of EC flux measurements and footprint distribution can be attributed to contribution of the two species. Both EC measurements and footprint models come with uncertainties. Suppose the footprint model has an uncertainty in footprint distance of, let's say, 15%. How will the distribution of 'detected' species change? Given such an uncertainty, is it feasible to actually quantify the contribution of species accurately enough? The answer obviously depends on the way the species are mixed. If the species are normally distributed, it will be difficult to separate the effects, whereas it would be much easier of the two species are somewhat clustered (but not too much, for it wouldn't be a biodiverse forest anymore). In my opinion, the authors should consider the impact of footprint accuracy and species distribution before even trying to answer the research questions.

Answer: It is certainly true that the question of species contribution to a footprint under field conditions can only be answered with considerable uncertainty, which we now acknowledge in the second paragraph of the discussion (**L471-498**). Following Kljun et al. (2015) we argue that any footprint model is most accurate for flat and uniform surfaces and introduce errors in areas with variable topography, non-uniform canopy structures, or rapidly changing meteorological conditions, because it assumes stationary and horizontal homogeneity of the flow over the eddy-covariance integration period.

Other uncertainties such as the 80% limit of cumulative source contribution in the 2 x 2 km domain, have already been mentioned in the methodology section (**chapter 2.4,** first paragraph) arguing that this limit can be assumed sufficient to estimate the main impact areas of the flux measurements.

The second is: the relative contribution of the two species in the mixed forest is nearly the same in all wind directions. Of course, this is beyond control of the authors, but it severely limits the applicability of the methods to answer the research questions. The authors do not really go into the variability of the relative contribution with stability and wind speed, perhaps the actual distribution over species is more variable than Fig. 6b suggests.

Answer: We agree that the relative contribution of the two species in the mixed forest is similar in all wind directions, limiting the possibilities to determine the influence of different species to flux composition. As suggested, we have therefore further investigated the impact of atmospheric stability and wind speed on the tree species contributions of the flux footprints. The results indicate that stability and wind speed have only a minor impact, which is now stated in **chapter 3.2** of the result section and is illustrated in the **new Fig. S5** in the supplement.

The third is: The NEE and LE of Beech, Fir and mixed forest do not seem to differ a lot. As a result, the model-observation comparison all tend to yield similar results (Figs. 7-9 and tables 2, 3 and 4). The authors claim that the model with static or dynamic footprint perform better than the pure Beech or pure Fir models. However, I do not really see a major difference. The r2, slope and NSE of the mixed models are indeed somewhat better, but overall, the r2 and NSE are rather small. In fact, the performance of the model to monocultures of Beech and Fir (Figs. S1 and S2) are really poor, particularly for Fir (r2 = -0.26 and 0.24 for the two sites).

Answer: Due to an admittingly bad evaluation in the previous manuscript version, particularly for the Douglas fir sites, we got quite similar flux intensities in beech and Douglas fir. This poor performance was at least partly the result of a very high variability of measurements, and also (in case of the Speulderbos site) due to periods where climate input could only be estimated. We have therefore redone the calibration/evaluations, focusing on high quality measurements using only reliable climate drivers, and could considerably improve the results (**Fig. S3 in the supplement**). For example, the single site R^2 at Campbell and Speulderbos for NEE are now 0.42 and 0.25, the joint evaluation resulting in an R^2 of 0.43 (R^2 at beech sites are always larger than 0.5). In addition, we not only show figures with 1:1 regression but also demonstrate the model performance throughout the year for each site (**Figures S1 and S2**).

The improved calibration yielded slightly better results than before for the overall NEE and ET evaluation at the ECOSENSE site. Still the regression plots, i.e. that for NEE throughout the season (**now Fig. 8**) are quite similar for beech, Douglas fir, and mixed forest simulations in comparison to measured EC fluxes. Therefore, we added a seasonally differentiated evaluation to highlight the importance of specific periods for cumulative annual fluxes (**new Fig. 9 and Table 2**). The additional analysis shows that considering dynamic footprints may be particular important in spring during the flushing phase, which is stated in a new paragraph to section 3.4 in the results (**L390-406**).

So, in principle, this is a great research topic. However, the authors seem to be more focused on applying a designed methodology than to actually answer the research question – if EC measurements in combination with footprint analysis could be a way to identify the contribution of individual species. Additionally, the measurement and model results for both tree species tend to be rather similar, which makes it difficult to tell if the species attribution method actually works or not. So, the conclusions should be negative: we cannot assess the influence of the different species and the uncertainty of resulting from the assumption of homogeneous footprint cannot be identified with the current methods.

Concludingly, although the research question and the methods are interesting and promising, I advise against publication in EGUspheres. Several elements of the study could be publishable though, for example, an analysis of the feasibility of the footprint

analysis to attribute to individual species, or the model results after better validation and perhaps going more into differences in fluxes.

A somewhat larger question would be how the authors want to identify the effect of species living together in an ecosystem. If mixed forests are more resilient than monocultures, this must somehow show as a non-linear response to mixing monocultures.

Answer: We acknowledge the criticism expressed and are thankful for the warning not to overstate the results and the advice to appropriately address limitations of the study. The critical issues mostly originate from an uncertain evaluation regarding Douglas fir monocultures and from the similar contribution of both investigated species within the investigated flux footprints. This has been addressed with an improved calibration/evaluation and a differentiation of periods showing that the species separation in modelling improves the alignment with measurements at least in periods with considerable different foliage activities. In addition, we have also modified the objectives to better fit to the results and to scale down expectations.

We also agree that a larger research question would be to investigate flux responses in differently mixed forests in order to see if responses differ from those of different monocultures within a flux footprint. However, this is beyond the current scope and abilities of the authors.

Specific remarks:

Line 83: do you have references to these model studies?

Answer: In the current exercise, additional data from new beech- and Douglas fir sites for which long-term EC data and good boundary information are available, have been used for calibration. Although the calibration exercise as such is unpublished, general references to the sites are given in the method section but have now been added already here. **(L97-98)**

Fig. 1: Could you apply the same color codes in Fig 1 a and b? E.g. Beech is purple in the left figure and blue in the right one.

Answer: We have happily followed this suggestion.

Line 106: Could you please go into the way that the tree species are clustered? And does the clustering have effect on the canopy height distribution? Does this affect the eddy covariance measurements?

Answer: The tree species are generally clustered in groups although single tree intermixture occasionally occurs. In the surrounding of the investigation center, Douglas fir and beech stands occur site by site in a group mixture with only marginally different heights of the dominant trees. This description has been added to the text **(L104-107)**.

The impact of these difference in heights is assumed to be small, which is also indicated by sensitivity tests, varying the zero-plane displacement by -25 to +20 %. Now we mention the uncertainty related to such parameters and argue that it only plays a minor role **(L145-150**). In addition, we are presenting some sensitivity tests in **Table S2** of the supplements.

Line 133: which data quality filter did you apply in the obs-model comparison in Figs 7-9 and tables 2 -4?

Answer: In this study, only CO_2 fluxes and ET data with quality flags 0 or 1 were used for further analysis, while data with quality flag 2 were discarded. The REddyProc software was used for further processing of the EC data (Wutzler et al., 2018), including u*-filtering according to Papale et al. (2006). For the comparison with model derived cumulative values, we used gap filling by marginal distribution sampling (MDS) according to Reichstein et al. (2005). This information has already been given in section 2.2.1 where Eddy covariance measurements are explained (**L132-136**).

Line 157: The method of measuring soil respiration is not clear to me. Did you rotate between plots? And how long did you measure per plot?

Answer: A bias during the day was prevented by varying the measurement order of the plots randomly. The measurement time depended on the season and was within 90 – 140 seconds. We added the requested information along with some more specifications (**L172-179**).

Line 160: the model description is not clear to me. Is the model based on an ecosystem or individual trees? Does the model include carbon stocks in stems, branches, leaves, roots and soil? Does the model include allocation to these stocks? And phenology? Does leaf fall contribute to soil carbon stocks?

Answer: We are sorry for the missing information and improved the description accordingly. The vegetation module of the model framework is based on trees which represent cohorts of individuals with equal dimensions and homogeneous spatial distribution. The carbon balance in each cohort (in a monoculture there is only one) is based on uptake (photosynthesis), allocation (into leaves, sapwood, fine roots, and structural reserves), and loss (respiration and senescence) processes. All processes are calculated in dependence on environmental conditions with explicit consideration of

phenology (based on cumulated temperature sums and chilling requirements). Vegetation and soil modules are in close interrelation with litterfall adding to the soil carbon pools and explicit calculation of decomposition processes supplying heterotrophic respiration. This information is added to the first and second paragraph of chapter 2.3.1 (**L182-208**).

Line 181: You describe that you studied model performance, but you do not describe how well the model performed. Could you please?

Answer: It is true that we kept the description of evaluations quite short in order not to overstretch this part. We have now more specifically indicated for which species EC fluxes or other data were compared with simulation results and that the model evaluation was successful. We separately address the performance in multi-model comparisons and only mention briefly applications to specific sites (**L209-213**).

Line 184: 1.5 years for soil spin up sounds extremely short, particularly for the more stable pools. Please explain.

Answer: In contrast to models that use pre-runs to simulate site conditions that are in equilibrium with the environment, the LDNDC model relies mostly on explicitly measured conditions. A short spin-up period is nevertheless necessary because the initialization of recalcitrant, intermediate and labile carbon pools in the soil (incl. litter), which can only be estimated from the available soil carbon and nitrogen information, is inevitably uncertain. The actual period depends on the climate and how close the automated estimates are to the equilibrium conditions, but test runs have ensured that the applied spin-up is sufficient for the investigated site. The latter information is additionally added to the text (**L217**).

Line 195; 'The two initializations … (Fig1, right)' it is not clear what you mean here. Could you clarify?

Answer: This refers to the site conditions and tree dimensions initialized for Douglas fir and Beech simulations, respectively. Each site was initialized with tree size and stand density that are representative for the surrounding of the EC flux tower. We use these initializations for all possible footprints which is another source of uncertainty to be discussed in the revision. The text was modified for better understanding (**L227-229**)

Line 208: I do not know these forests too well, but I thought Speulderbos was not a Fir monoculture.

Answer: The forest in Speulderbos, also sometimes called Speulder forest, is indeed a pure 2.5 ha sized Douglas fir forest, planted in 1962 (according to Su et al., 2009). Surrounding forests consist of various tree species but are generally thought to be of minor importance (van Wijk et al. 2001).

Line 210: Why do you only show daily numbers, not hourly ones, or diurnal cycles? The daily statistics of the different species are quite similar and may hide responses hidden in photosynthetical / stomatal / light responses of the species. A half hourly time-scale would be more appropriate from this perspective, while the footprint is variable at that time-scale too.

Answer: The main reason the evaluations are done on daily resolution is that the model (climatic) input at the investigated sites were in daily resolution as well. Although the model internally represents diurnal cycles for gas exchange in hourly resolution, patterns that are related to sub-daily cloudiness or precipitation pattern can thus not be reproduced. We have added this information into the text (**L243**). In addition, the daily evaluation steps are chosen for easier perception for these time lines that are running for more than 10 years.

The comparison of simulations and measurements at the ECOSENSE forest, however, have all been done based on hourly data (Fig. 7-11).

Line 214: 'reasonably well' - can you quantify?

& Line 215, Figs S1 and S2: I do not consider comparisons with r2 - -0.26 and 0.24 'reasonably well', this is an uncorrelated point cloud. Before applying the model to mixed forests, the model should be applicable to monocultures first.

& Lines 216-218: Nice that you analyzed the model performance. Could you go into the results?

Answer: The model has now been re-evaluated and basic statistics of the calibration as well as a short indication of reasons for deviations are indicated in the text of the main document (**L247-252**). We think that given the large range of different conditions at the selected sites, R^{2}'s of around 0.6 (for beech) and 0.4 (for Douglas fir) can be considered quite good. In addition, we put more statistics as well as seasonal developments into the supplement (**Fig. S3**).

Section 3.2: Overall, fig 6c suggests that the relative contribution of Beech and Fir are quite similar in all wind directions. In this situation, how can you distinguish between species? Don't you need contrasting contributions to single out the contributions of the species?

Answer: It is true that it would have been easier to compare flux footprints with significantly different species contributions in different wind directions (done e.g. in Kutsch et al. 2005). Since this is not really the case here, we are relying on the site-evaluated model simulations which distinguish species contribution. However, we have instead added an analysis of relation of species abundance and flux contribution for different seasons (**L389-406, new Fig. 9, new Table 2**). This shows that at least in spring (flushing phase), when the flux contribution of beeches is generally small, neither the assumption pure beech nor pure Douglas fir can represent the measured findings well, while considering a dynamic footprint performs considerably better.

Figs. 7-9 and tables 2-3: Would it be an idea to include the statistics information in the figure panels for easier dissemination?

Answer: Thanks for the suggestion. We have provided this information into the **Figures 7 and 8** and have deleted the former Tables 2 and 3 accordingly. For the new Fig. 9, we provide the statistics in a new Table 2, since we think that the information would be overwhelming for a 9 panel Figure.

Section 3.3: I would say all obs-model comparison perform similarly. The statistics of the fixed and dynamically weighted model is not considerably better.

Answer: The differences between the R^2 and bias of the various model settings are indeed small. However, we now have tested various other statistical measures which show that the representation of flux footprints is considerably better when considering both species compared with the assumption of monocultures (see also answer to the previous question (**L389-406, new Fig. 9, new Table 2**). This performance difference is particularly apparent in the spring periods (April), while it cannot be seen in summer (due to the similarity of flux activities of the two species) nor in Autumn (probably due to a relatively long senescence period originating from the calibration sites). In addition, we have toned done the enthusiasm for getting this result due to the still relatively small differences.

Line 401: 'Overall, … shows the closest agreement' - That depends on which criteria you apply. I would say all model approaches perform similarly well considering annual NEE and timing, except perhaps the Fir monoculture simulation.

Answer: Thanks for the suggestion. We are now addressing gap filling results obtained with the statistical approach and those derived from the model as of similar quality (**L472-477** and **509**). We also enlarge the discussion about both approaches with respect to seasonal differences (**L510-514**).

Line 440: '…clearly superior…': The improvement is minor. Please refrain from overselling your results.

& Line 517: 'successfully reproduced key …': yes for soil moisture and soil respiration, but not for NEE and LE (Supplementary information).

Answer: Thanks for the reminder. We are now refraining to use words that could give the impression of overselling the results (**L471-523**).

References: all DOI's miss a colon in the hyperlink, please change 'https//' to 'https://'

Answer: Thanks for the hint. This is now changed in the template for generating references.

Editor comments:

Thank you very much for your very timely comments to the two reviews, which is much appreciated as it fully supports the motivation behind the open review and discussion process that our journal pursues.

Both referees mentioned the high relevance of your approach and see a potential that your study can be further developed and become a good and novel contribution to our science.

I am impressed by the high quality of the two reviews, and I believe that especially referee #2 had some very important critical remarks. Your responses made it clear that you have understood well the situation and provided some promising ideas on how to improve the manuscript by openly addressing these weaker parts of your study - considering weaknesses can make your study stronger.

The uncertainties in flux data and footprint estimates and the, for this study somehow limiting relatively uniform species distributions around the tower are real world situations and you will certainly address what that means for the significance of the analysis.

Finally, of course, we need significant and clear results and examining certain seasons for contrasting different plant functional types can be expected to lead to higher statistical species-specific contrasts in the flux data (see e.g. Fig. 6 in Aubinet, M., B. Heinesch and B. Longdoz (2002). "Estimation of the carbon sequestration by a heterogeneous forest: night flux corrections, heterogeneity of the site and inter-annual variability." Global Change Biology 8(11): 1053-1071.)

If in these seasons bare deciduous and evergreen crowns form a common canopy, the zero-plane displacement height might be difficult to obtain, which might then add systematic bias to the footprint estimations. A hypothesis would be that z-d will be larger with higher fraction of leafless trees in the canopy. You might want to try optimizing the choice of this parameter, or, at least, include it in a sensitivity analysis.

Good success with the further analyses and revisions!

We are grateful to the editor for his evaluation and suggestions. Regarding the possible influence of the zero-plane displacement height we have added a sensitivity analyses (**L145-150, Table S2**) which, however, resulted in the finding that changing the parameter within a -25/+20% range is not significantly influencing the species contributions.

We have also incorporated the indicated paper into the discussion for highlighting possible differences between site investigation and the uncertainty related to studies that are only running over one year and thus depend on specific environmental conditions (**L493-498**).

Mentioned references

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