

RC3: 'Comment on egusphere-2026-524, Anonymous Referee #3, 30 Apr 2026

The manuscript by Pery et al. presents an interesting approach to gap-fill CO₂ flux densities based on wind-sector partitioning, to account for field heterogeneity at the experimental site. This method employs the same set of predictors, while separating the input data to the model in wind sectors and gap-filling them separately. While the method is sound and it has potential to improve NEE estimates over heterogeneous sites, without the costs and logistic difficulties associated to the deployment of several EC stations, there are some hindrances in this approach and how it was validated and presented. Therefore, I would recommend major revisions before this manuscript is accepted for publication.

We thank Referee 3 for her/his appreciation and particularly for all her/his comments we address below and take into account for the coming revised manuscript that greatly strengthen the validation of the proposed approach.

Major comments

1.- Were meteorological data gap-filled? How did you deal with these data for the specific wind sectors and periods? Please provide all these details as they are very relevant for the RF parametrization.

We thank the Referee for this important clarification request. We agree that the treatment of meteorological data is relevant for the RF parametrization and we provide additional details below. It is important to note the data filtering procedures described in the manuscript apply only to CO₂ flux measurements. As a result, the reported 32% of missing data concerns flux data exclusively and does not affect the meteorological drivers used in the random forest models.

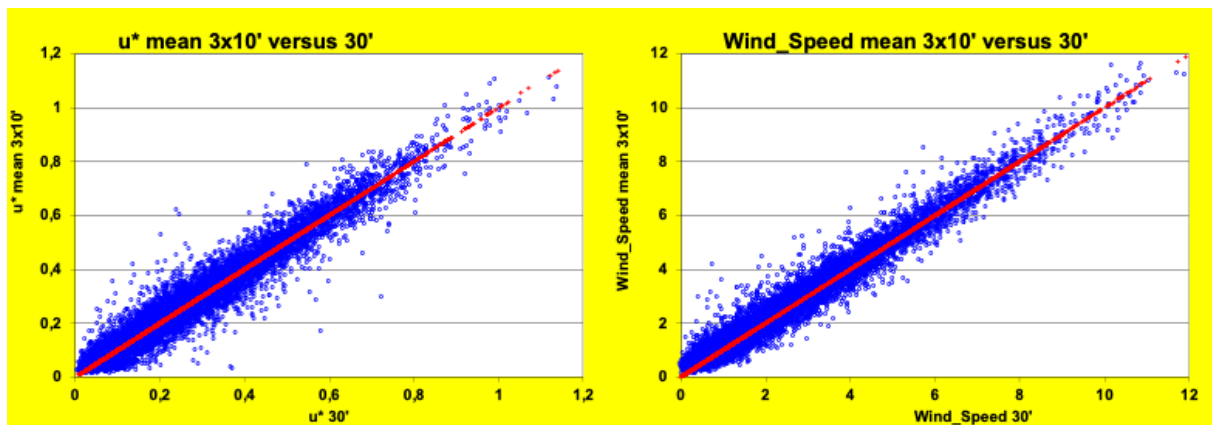
Regarding meteorological variables, a very small fraction of missing data remained after processed Eddy Pro output: approximately 0.03% for air temperature, soil temperature, and soil humidity, 0.005% for relative humidity, and 0.68% for wind direction, wind speed, and atmospheric pressure. These gaps were generally short, with a maximum duration of 7 consecutive missing values (i.e., 70 minutes).

Given the very limited extent and short duration of these gaps, we applied a simple mean-based gap-filling approach to ensure temporal continuity of the meteorological drivers used in the Random Forest models. This choice was made to avoid introducing unnecessary complexity while preserving the physical consistency of the predictors.

2.- Using 10-min resolution for processing high-frequency data would require stronger justification; heterogeneity itself could be one, as rapidly shifting wind conditions could drive measuring over different sectors and land covers, but this can come with hindrances that should be accounted for, unless it is adequately demonstrated.

We understand the Referee 3 (and other Referees as well) comment with regard to our 10 min. average flux window we now use since several years, as adapted for more coastal and dynamic systems such as wetlands (present study), salt marshes (i.e., Mayen et al., 2024, bg-21-993-2024) and tidal flats (i.e., Polsenaere et al., 2012, bg-9-249-2012), instead of the commonly used 30 min. averaged flux window for EC data processing over terrestrial ecosystems. The comment is fair since, our study site though being a wetland system, is surrounded by different crop and grassland fields and in any cases, we need, as for all atmospheric EC studies to be sure to measure and catch all eddy that contribute to the fluxes and be careful with the corresponding EC set-up in terms of measurement height and time averaging window for the chosen specific studied site.

To address this point and more strongly justify the 10 min. averaging time period chosen in the present study as rightly noticed, we did further computations comparing the friction velocity, wind speed and flux data between the mean of the three 10 min. values and the corresponding mean values obtained by reprocessing all data (February-September 2023 period) through EddyPro with an averaging time period of 30 min. (see attached graphs below). Although we can see some dispersion between both 10 min. and 30 min. mean values (u^* , wind speeds, mean fluxes), we also clearly notice that we did not have any low frequency losses on the 10 min. flux data.



Furthermore, we decided to choose a 10 min. averaging time period instead of 30 min. to lose less data due to non-stationary conditions and/or electronic noises leading to potential and more data suppression (with the 30 min. choice) according to our data quality check procedure adopted in the present study. The new computations and comparisons done between 10 min. and 30 min. data showed that, according to the land parcel, we would lose between 21% (wheat, sector 213°-283° with dominant wind directions) and 49% (wheat, sector 133°-173°) of data with a choice of 30 min. mean flux calculation instead of the 10 min. mean flux procedure used here.

All these considerations and new computations will be added to the revised version of the manuscript to specify and strengthen our choice.

3.- In the results section, the figures should be presented following a logical order and in a more complete but concise way. Currently, this section is a bit unorganized and mentions to Figures 5 or 6 are included before any explanation of Figure 4. Please try to re-write this to help readers to follow the story.

Thank you for this helpful comment. We agree that the current structure of the Results section may not be optimal. In the revised manuscript, we will reorganize the section to ensure a more logical flow of figures and their corresponding descriptions. This restructuring will be implemented in line with the other modifications made in response to the reviewers' comments, resulting in a clearer and more coherent presentation of the results.

4.- Perhaps the RF could benefit from adding time cycle variables, for day/night and for seasons. This was implemented e.g. in Vekuri et al. (2023), using cyclical indicators day-night and for months of the year, as well as a linear indicator of the day of year. Maybe this removes in an easier way the uncertainty in gap filling related to changes in the ecosystem properties due to management events such as harvest.

We thank Referee 3 for this suggestion. We agree that cyclical time variables can be useful in some modelling contexts. However, in our case we chose not to include explicit seasonal or monthly cyclical encodings because the main temporal dynamics are already well captured by PAR, day-of-year (DOY), and the day/night indicator. Adding additional cyclic variables would therefore provide largely redundant information. Moreover, we aimed to keep the model as simple and interpretable as possible, especially since Random Forests are already able to handle non-linear temporal patterns without requiring explicit seasonal transformations. We will clarify this point in the revised manuscript.

5.- The inclusion of some events, like the November storm, could be interesting if these specific phenomena would be studied. However, as this is not the scope of the text and this can bias the results of the modeling, I would suggest to either remove it, or to address the specific effect it may have on the gap-filled data. Furthermore, if the storm was relevant for CO₂, but no physiological activity was taking place at the moment, could it be an effect of a bad performance of the analyzer which then justifies to remove it from the time series? Or which explanation did you find for it?

In the revised manuscript, we applied additional quality-control procedures to the CO₂ flux time series to remove physically unrealistic values that were not adequately detected by the standard filtering approach. While the conventional filters based on the median absolute deviation (MAD) and the commonly used threshold of $u^* < 0.1$

m s^{-1} successfully removed a large fraction of low-quality measurements, several extreme flux values remained in the dataset, particularly during periods of strong meteorological disturbances.

To address this issue, we introduced hard limits based on the observed range of realistic ecosystem fluxes. Before harvest, NEE values were constrained between $+20$ and $-50 \mu\text{mol m}^{-2} \text{s}^{-1}$, whereas after harvest, when ecosystem activity was substantially reduced, a range of $+10$ to $-10 \mu\text{mol m}^{-2} \text{s}^{-1}$ was applied. These limits removed a small number of spurious observations, including the most extreme values associated with the November storm period (see Fig. 3).

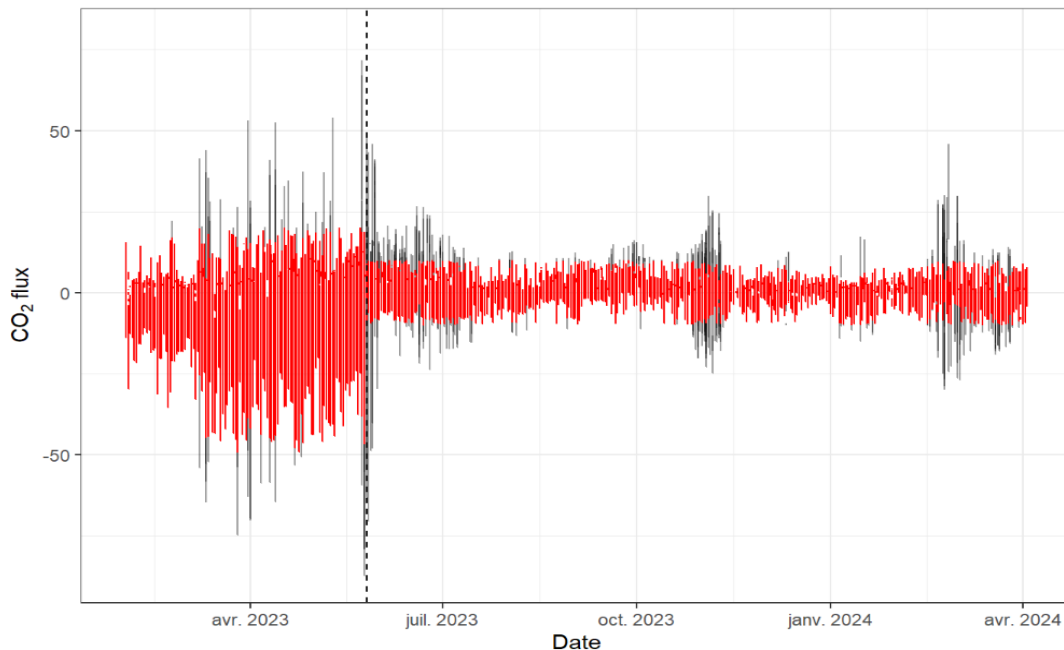


Figure 3. CO_2 fluxes presented in the submitted manuscript (black) and CO_2 with hard limits filtration (red) introduced in the revised manuscript.

The effectiveness of this additional filtering is particularly evident for the wheat sector after harvest, which was the most strongly affected by these extreme values. In the original analysis, the RF model achieved an R^2 of 0.44 for this period. After applying the hard limits, the model performance increased to an R^2 of 0.97, indicating that a limited number of unrealistic observations were disproportionately degrading the model calibration.

6.- The manuscript could benefit from adding the partitioning of NEE into GPP and RECO using either Reichstein et al. (2005) or Lasslop et al. (2010), or both. This would allow to skip the separation into nighttime and daytime NEE. These models could be applied to the filled data for the whole dataset and for the different wind sectors and periods. Please also be careful with naming daytime NEE as productivity or production, because this is the net CO_2 flux density and not photosynthesis.

We thank Referee 3 for encouraging us to carry out the partitioning of simulated NEE into GPP and Reco, which we regret not having done in the submitted MS. We've now done it for each of the 8 sectors shown in Fig. 2a as well as for the entire wheat sector, using the method from Kowalski et al. (2003, 2004).

The current Figure 9 will therefore be replaced in the revised MS by a new figure showing the cumulative GPP, Reco, and NEE for all the sectors considered. As a result, sections 3.3, 3.4, and 4 will be largely rewritten accordingly.

Kowalski, A. S., Loustau, D., Berbigier, P., Manca, G., Tedeschi, V., Borghetti, M., Valentini, R., Kolari, P., Berniger, F., Rannik, U., Hari, P., Rayment, M., Mencuccini, M., Moncrieff, J., and Grace, J.: Paired comparison of carbon exchange between undisturbed and regenerating stands in four managed forests in Europe, *Global. Change. Biol.*, 10, 1707–1723, 2004.

Kowalski, S., Sartore, M., Burlett, R., Berbigier, P., and Loustau, D.: The annual carbon budget of a French pine forest (*Pinus pinaster*) following harvest, *Global. Change. Biol.*, 9, 1051–1065, 2003.

7.- Carbon budgets usually account for terms like carbon export through harvest, carbon input through organic fertilizer, etc. The most important are typically NEE and carbon export. Please avoid the term "carbon budget" throughout the text if no other lateral C transport is considered. Additionally, if there are data on C export through harvest, they should be presented in the Results section, together with annual NEE sums, and then discussed appropriately later on. If no other terms are considered, then also the title of the manuscript should change to avoid confusion.

We thank Referee 3 for this important comment. We agree that the term 'carbon budget' may be misleading in the absence of a full accounting of all carbon lateral fluxes (e.g., harvest export, organic amendments). In the revised manuscript, we will therefore avoid using this term and instead refer more specifically to NEE-based carbon dynamics. We will also clarify in the Methods and Results sections what components are included in our carbon accounting.

8.- The results and method described in the paper could be put in context with more literature describing how to address heterogeneity effects over EC sites. There are plenty of studies using either footprint modeling (e.g. Chen et al., 2011) or multiple towers (e.g. Oren et al., 2006), or a distinct approach (e.g. Wang et al., 2016). Furthermore, related to a comment in the conclusions, (line 632), if the focus is the cost of the EC stations, there have been in recent years developments of lower-cost eddy covariance systems; some studies validated the systems (Hill et al., 2017; van Ramshorst et al., 2024; Rannik et al., 2026) and some focused on their application to improve spatial representativity of measurements (Cunliffe et al., 2022; Callejas-Rodelas et al., 2025). The text could put this also in context.

We thank Referee 3 for this helpful suggestion. We agree that the manuscript would benefit from better positioning within the existing literature on addressing spatial heterogeneity in EC measurements (e.g., footprint modelling, multi-tower approaches, and alternative methods), as well as recent developments in lower-cost EC systems. We will revise the Introduction and Discussion sections of the revised MS accordingly to better contextualize our approach within these studies.

8.- The manuscript would benefit from re-wording many sentences and sections for technical completeness and coherence, particularly in the Methods section. Some of these comments are included in what follows, but they do not cover the whole text.

We will take into account Referee 3 minor comments below in the revised version of the manuscript being more clear, complete and coherent in the different sections especially in the M&M as mentioned too in a previous comment (3.) above.

Minor comments

1.- Section 2.2.2 would benefit from a more clear structure and presenting more details about the EC data processing; particularly, as this is a paper presenting a novel method, all important details in data processing are necessary. For instance, which method did you apply for high- and low-frequency spectral losses?

We believe this 2.2.2 EC data processing and filtering method section is already clear and detailed in the submitted manuscript as based on previous published papers we did with an associated validated method now also used in the present study (see Polsenae et al., 2012, bg-9-249-2012; Mayen et al., bg-21-993-2024; Mayen et al., bg-22-5387-2025). We agree that our present study shows a novel method with modelling RF approaches but our EC data processing/filtering procedure remains the same as before; in our EC studies, we always justify our procedure that remains specific to the corresponding study site. Our EC data are always carefully checked with regards to the 10 min. averaging time period choice (see comment 2 response) that can potentially lead to (low) frequency losses indeed. According to Referees comment and as requested, we will add these information and further computations (comment 2 response) in the revised manuscript to more strongly specify and justify from this point (flux losses) our EC data processing adapted to the present studied site.

2.- Line 154: Please try to write the description of EC method in a more correct way, such as "This method measured vertical wind speed and molar density or fraction of the gas of interest at high frequency. The flux density is then derived by calculating the covariance between w and s , and multiplied by air density".

We thank Referee 3 for this precision, we will modify the sentence as proposed in the revised manuscript.

3.- Line 161: Please rewrite - Fluxes are not measured directly, but calculated from high-frequency measurements.

We will modify accordingly this sentence in the revised manuscript as: "In this study, calculated fluxes from high-frequency measurements were averaged every 10 minutes to maximize the number of observations".

4.- Line 183: "to ensure accurate CO₂ flux measurements" - this sentence is not necessary. Other software (EddySoft, EddyUH, etc.) also produce standardized and similarly accepted calculations; furthermore, the fluxes are not measurements, but calculations from raw measurements.

We will modify accordingly this sentence suppressing this part.

5.- Line 194: "calculated using the linear detrending method" - this is not a method for calculating fluxes, but just one of the steps along the processing routine. Please rewrite this.

We will rewrite accordingly this sentence as "Linear detrending was used to calculate fluctuations as the deviations from a linear trend on a chosen 10-minute averaging time period.

6.- Line 196 to line 204: Please try to re-organize this section; it is not conceptually wrong, but maybe it would help for clarity to organize the different filters that were applied. "After flux processing, data were filtered to remove outliers using MAD (explain), to exclude periods with weak or non-existent turbulence (ustar filter, explain) and to ()".

We thank Referee 3 for her/his help clarifying this part and modify it in the revised manuscript as proposed and as also explained in your comment 5 above.

7.- Section 2.3.1: Please include information on the footprint model. How were footprints calculated? Did you parametrize something specific, like aerodynamic canopy height (as in Chu et al., 2018)?

We apologize for not mentioning any information on our footprint calculation in the submitted manuscript. The footprint was estimated with the Kljun et al. (2015) model and calculated using the constant measurement height ($Z_m=3\text{m}$), the constant displacement height ($d = 0.1 \text{ m}$), mean wind velocities (u_{mean} , m s^{-1}), standard deviations of the lateral velocity fluctuations after rotation (σ_v , m s^{-1}), the Obukhov length (L), friction velocities (u_{star} , m s^{-1}) and wind directions ($^\circ$), measured by the station from February, 1st 2023 to April, 3rd 2024. We did not parametrize something specific like aerodynamic canopy height though being definitely a good approach Referee 3 mentioned and as done in Chu et al., 2018. In the present study, we only present an averaged footprint computed on the whole time period as mentioned above that was enough adapted to our modelling approach we propose in the present study. All these information according to footprint calculation will be added in the revised manuscript.

8.- Line 227: Do you have an explanation why this happens? Is this the case for all these anemometers? Please justify it.

It is not surprising as due to the sonic anemometer position, actually the whole IRGASON sensor (CSAT3A and EC150 Campbell IRGA) with regard to wind directions coming right behind the sensor in particular in the opposite direction (i.e., Southern directions) of the sensor transducer head (133° - 283°) (see attached photo below). It could be specific to the IRGASON Campbell sensor (EC150 parallel arms very close to the CSAT3A transducers) though this effect could also exist for other high frequency IRGA/sonic set-up according to associated sensor geometry. Very few studies tested such potential bias through u^* versus wind direction plots as we did in the present study, so it is complicated to know if it is the case for all EC sensor set-up and/or more pronounced for some models than others. It is then something that could be checked in EC studies; again, this effect has no

influence on CO₂ flux calculations directly but needs to be taken into account carefully when u* filtering procedure are done on post-processing EC data.



(April, 9th 2026)

9.- Lines 248 to 252: this should be discussed in Discussion; other methods could complement this, like footprint modeling with land use/cover attribution.

It will be discussed in the Discussion section of the revised MS along with other methods as suggested.

10.- Lines 254-257: this paragraph belongs to discussion or conclusions.

This paragraph will be displaced in the right (discussion/conclusion) section in the revised MS.

11.- Line 266: "key temporal subdivision" - do you mean the separate models were applied to wind sector and specific periods? Please make this more clear in this sentence.

The sentence will be rephrased accordingly in the revised MS.

12.- Line 267: "Specifically, for each crop parcel ..." -- this should be commented in Discussion in the context of whether this is accurate also for several years and how the transition from senescence or dormancy to growing season... by daily sum of NEE, by meteorological inputs, by phenology, etc. Generally, crops' carbon sink during growing season starts to decline before harvest, as crop ripens, therefore it could have an impact on when and how the growing season is defined.

It will be commented in the Discussion section of the revised MS in that context as rightly suggested by Referee 3.

13.- Line 280: How was the qualitative day-night indicator defined? And why was it qualitative? Couldn't it be better to define day or night based on, e.g., radiation? Please clarify this.

The qualitative day-night indicator distinguishes periods of daytime and nighttime activity by assigning "day" to time steps with $PAR > 10 \mu\text{mol m}^{-2} \text{s}^{-1}$ and "night" to those with $PAR \leq 10 \mu\text{mol m}^{-2} \text{s}^{-1}$. We will add this clarification in the revised MS.

14.- Line 306: "This evaluation framework..." - this sentence is used in similar forms in other subsections in Methods; maybe it could be avoided or just write it once in discussion to summarize why this approach was adopted.

It is true and will be modified accordingly in the revised MS.

15.- Line 310: "Predictions were made ..." - this is not necessary, as it was clear from before that the time resolution is 10 min, and the units of CO₂ flux densities.

In the revised manuscript, this sentence will be removed as this information was already given in the manuscript.

16.- Line 311: The factor 0.0072 comes from the number of seconds in a year, multiplied by molar mass of carbon and the conversion from umol to mol. However, either this should be clarified for unexperienced readers or be left out as it is common knowledge in EC studies and it is just a simple calculation.

"using a factor of 0.0072" will be removed in the revised manuscript as obvious/common calculation in carbon flux studies.

17.- Line 314: Please rewrite the whole paragraph for clarity.

Since we carried out the partitioning of simulated NEE into GPP and Reco, this paragraph will be completely rewritten in the revised MS.

18.- Line 326-329: This paragraph could be in Methods.

This information is already given in the M&M section but we would like to keep it here at the beginning of the results section for a reminder and a better understanding of the coming results and analysis next.

19.- Line 342: particularly at small flux values.

We will modify the sentence accordingly in the revised manuscript.

20.- Line 343: what is meant by unmodeled management practices? Please clarify. As I understand it, no specific management was introduced in the RF models, just the predictor variables and the classification in wind sectors and periods.

This will be clarified in the revised MS.

21.- Line 368: This is only a particular 30-min period, so it shouldn't be taken as the example; furthermore, a flux density of -74 umol m⁻² s⁻¹ seems too large. Looking at the Figure, aren't these values occurring in the hybrid parcel? Maybe it is my mistake, but I do not see these values happening in the wheat parcel, but in the hybrid.

It is true and we thank Referee 3 for her/his comment; please see again our response to your comment 5- above to get modifications done and presented in the revised version of the MS.

22.- Line 504: "This lower performance...". Why is this happening? It shouldn't be the case.

Please see our response to your comment 5- and other Referees according to our modelling approach performance and associated modifications done in these revisions and that will be presented in the revised version of the MS.

23.- Line 512: if there was noise introduced by some fluxes that were not filtered, why not trying a different filtering approach, maybe more strict, or using e.g. hard limits?

In the present study (as our previous ones), we do our best to qualify all our EC data set, filtering bad data that do not fulfilled EC theory condition (stationary test) or bad weather conditions (stormy events, etc.) without being too abusive on the other hand removing potentially good data. After applying all our processing and filtering data

procedure, we already removed more than 30% of data (32%) (l. 204 in the submitted manuscript). For this particular stormy event in autumn 2023, we particularly checked corresponding EC data we kept based on our flux quality check procedure described in the submitted manuscript (M&M section) though a higher flux magnitude was logically recorded during these stormy conditions under high wind speeds in particular. However, owing to our further computations particularly described in our response to your comment 5- with regards to our flux data filtering, we will modify accordingly this part of the discussion in the revised version of the manuscript allowing to be more accurate in the description and discussion of the measured flux data in comparison with the modelling approaches we present.

24.- Line 516: from this sentence it is not clear whether the spurious values were included or not in the dataset that was used to train the model. Please clarify.

It will be clarified accordingly in the revised MS.

25.- Line 576: The annual NEE sums (not carbon budgets unless carbon export was considered, as commented previously) should be reported in results.

Referee 3 is right and NEE sums instead of carbon budgets will be reported in the Results section of the revised MS.

26.- Line 514: maybe this table could go in Appendix, as it is just a reference for literature values.

We would like to keep this table in the main text (discussion section) as it stands, instead of going in Appendix; indeed, this later gives relevant information on other EC fluxes in term of crops, methodology and flux values (range) allowing consistent comparisons with results we obtained and discussed in the present study.

27.- Generally, the past tense is used for Methods and Results, and partially on Discussion when referring to results. Although this is an election from the author, I would recommend to check this for consistency across the manuscript.

It is right and we will be more careful according to tenses used in the whole revised manuscript and corresponding sections.

Technical corrections

We thank Referee 3 for all her/his technical corrections we will address in the revised version of the manuscript.

Line 37: remove "as".

Line 42: suggestion to re-write as "as both sink or sources of atmospheric CO₂".

Line 78: meteorological.

Line 93: write "a RF-based" instead of "an RF-based" as RF is read as "random forest".

Line 154: These eddies occur/are located.

Line 172: ... includes the following sensors positioned at the top of a 3-m tower mast (Fig. 2b): an ultrasonic anemometer...

Line 175: The station also included additional sensors...

Line 261: The first strategy (Scenario 1) involved...

Line 265: In the second strategy (Scenario 2) separate RF models were trained for...

Line 354: Box in red represents November's storm.

Line 400: Please avoid statements like "clear".

REFERENCES

We thank Referee 3 for references below we did browse and take into account in our revisions and the corresponding revised version of the manuscript.

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