Response to Reviewer #1

The study evaluated CO_2 flux outputs from models at six sites and their relationship with climate in temperate forests of Western Europe. Overall, this manuscript addresses an important topic and presents some interesting findings. However, I am not entirely convinced that the results significantly advance our understanding of CO_2 fluxes at forest sites across different temporal resolutions (such as monthly and annual, as analyzed in this study).

We would like to thanks Reviewer 2 for their constructive feedback, which will greatly contributed to the improvement of the manuscript. In response to both reviewers' comments, we have decided to enhance the study by including approximately 17 additional ICOS sites, incorporating more models (specifically the TRENDY ensemble and FLUXCOM-X), and focusing exclusively on the monthly timescale. This adjustment will enable a more detailed exploration of the influence of climate on the temporal variability of CO_2 fluxes.

In the initial version of the manuscript, this influence was analyzed by considering all months together, which yielded results that closely resembled those at the annual timescale (e.g., see Figs. 6 and 8). In the revised version, we will examine the influence of climate on CO_2 fluxes for each month individually, as we anticipate that this influence will vary throughout the annual cycle. The inclusion of additional sites will also allow us to analyze the zonal and meridional variations in this influence across Europe.

General comments
1. The temporal variability in this study is not adequately addressed and may need reorganization. For example, there are ten figures illustrating monthly timescale results. However, these findings are neither included nor highlighted in the abstract or conclusion. Although there are similar patterns between the monthly and annual results, as shown in Figures 6 and 8, the monthly findings should be incorporated. Alternatively, I suggest removing or reducing the figures or text about the monthly results and focusing on annual and interannual scales.

The results are comparable at both the monthly (when all months are considered collectively) and annual timescales, which justifies the removal of all analyses conducted at the annual timescale. The results section will be reorganized into three parts: (1) mean annual cycle and interannual variability, influence of climate (2) regardless of the annual cycle (i.e., all months considered collectively) and (3) along the annual cycle (i.e. each month or season considered individually). The abstract and conclusion will be rewritten accordingly.

2. While I am not an expert in statistics, the R and R^2 results in this study (such as those in Figures 6 and 7) seem very close. Is it necessary to present Figure 7 in the main text? Regarding the long-term evolution, which method was used? Why do the authors state that it 'does not depict any trend'? Please provide this information in the Methods section.

R and R^2 provide complementary insights. R indicates the sign and strength of the relationship, while R^2 represents the proportion of variance in a given variable (e.g., NEE) that can be explained by another variable (e.g., 2 m temperature).

Although Figure 7 offers valuable information on temperature-induced threshold effects, it would occupy too much space with the inclusion of new sites and models. Therefore, we will remove Figure 7 in the revised manuscript.

We did not assess the statistical significance of the trends due to the limited temporal coverage of the data, particularly for observations and SMAP-L4C. As previously mentioned, we will eliminate all analyses conducted at the annual timescale, including the trend in the annual CO_2 flux budget.

3. The introduction of this study is not well-articulated. The discussion of the knowledge gap lacks a solid basis. Why is a finer spatial scale important? Why did the authors focus on monthly and annual timescales? The second objective, which involves the climate relationship, lacks motivation regarding the choice of variables in this study. Why were radiation or PPFD not considered?

The introduction will be revised to address the knowledge gap concerning the influence of climate on the interannual variability of CO_2 fluxes in European forests, as well as the need for high-resolution products. Additionally, we will provide a more robust justification for the selection of the variables included in the study.

4. It is not appropriate to assess flux trends using the FLUXCOM dataset, as FLUXCOM does not account for CO_2 fertilization effects. Please refer to the following paper: *Jung, M., Schwalm, C., Migliavacca, M., Walther, S., Camps-Valls, G., Koirala, S., ... & Reichstein, M. (2020). Scaling carbon fluxes from eddy covariance sites to globe: synthesis and evaluation of the FLUXCOM approach. Biogeosciences, 17(5), 1343-1365.*

Thank you for this remark. We believe that CO_2 fertilization effects are just one of many factors influencing long-term trends in the CO_2 flux budget (e.g., fire, timber extraction). Consequently, this analysis will be removed in the revised version of the manuscript.

Line-by-line comments:

Line 42: "..the best models…" sounds inaccurate. Perhaps "better models" would be more appropriate, as the comparison is only made with process-based models.

Changed as suggested.

LL 45-48: could you please explain why?

At the interannual timescale, the relationship between CO_2 flux and climate is weaker for NEE compared to GPP and RECO. Since NEE is defined as the difference between GPP and RECO, its interannual variability arises from various combinations of these two components. For instance, positive NEE anomalies (indicating reduced CO_2 uptake) can occur due to a greater increase in RECO compared to GPP, a larger decrease in GPP than in RECO, a decrease in GPP with no change in RECO, or an increase in RECO without any change in GPP. This complexity suggests that the influence of climate on NEE is less direct and likely more complex than its effects on GPP and RECO.

LL 45-47: The sentence contains a lot of information and lacks clarity. I suggest revising it for better readability.

We will revise this sentence for better readability.

Line 49: How long is the 'long-term'?

The term "long-term" is contingent on the length of data availability, which does not exceed 20 years. This is why we have placed the term in quotation marks. However, this remark will no longer apply in the revised manuscript, as analyses conducted at the annual timescale will be removed.

LL 50-53: The statement is too general and does not seem related to the main point of the study.

Thank you for this remark. This statement will be removed in the revised manuscript.

Line 61: 'At the global scale, forest ecosystems cover about 30% of landmasses' suggest adding a reference.

We will add this reference:
FAO Global Forest Resources Assessment 2020: Terms and Definitions. For. Resour. Assess. Work. Pap.2020, 32

LL 71-72: What is the meaning of this number here?

According to Chuine et al. (2023) timber-extraction and climate-related mortality have increased by 20% and 54% between 2005-2013 and 2012-2020, respectively.

Line 101: Suggest deleting 'when available'

Done as suggested.

LL 101-113: Suggest moving these sentences to another paragraph or placing them above the objectives.

Thank you for the suggestion. We will move these sentences above the objectives.

LL127-128: Redundace, and which varaible? all fluxes?

This sentence was unclear. It will be removed in the revised version.

Line 132: "Extreme" can refer to both high and low conditions. To specify, consider using "extreme high" or "extreme hot" for clarity.

Thank you for this remark. Done as suggested.

LL 132-133: Suggest revising the sentence.

The sentence has been revised. It now reads: "In addition, extreme high temperatures combined with soil water stress have been shown to significantly impact GPP and RECO".

LL147-149: Suggest deleting these sentences.

Done.

Figure 1: If the reader is not familiar with Europe, it may not be clear. Consider adding specific locations, such as "France".

Figure 1 will be modified to include the new sites.

Table 1: The table format needs adjustment. Please refer to the journal's guidelines, which typically suggest using horizontal lines only above and below the table, and as a separator between the table header and the main body.

Table 1 will be adjusted to follow the journal's guidelines.

Figure 9-11: Perhaps move to supplement.

Figures 9 to 11 will be moved to supplement.

LL555-556: Suggest deleting the sentence.

We believe it is important to retain this information in the figure caption to help readers understand why the results are similar between ENF and DBF.

Line 603: "qualitatively similar" might sound speculative?

You are correct. We will mention that the results obtained at the annual timescale are similar to those at the monthly timescale. A few results from the annual timescale will be included in the supplementary material to support this statement.

Line 724: Replace 'best' to 'better'.

Done.

Line 772: Should be 'Fig. 7' not 'Fig, 5'

You are correct.

Line 776: 'Fig. 7'

Done.