RC1

We sincerely thank the Editor, Associate Editor and Reviewers for handling and taking the time to read and review our manuscript. We greatly appreciate the reviewer's insightful and constructive remarks. They have helped us improve both the scientific rigor and the readability of the manuscript. Below we address each comment in turn. Line numbers refer to the clean revised version unless otherwise indicated.

The diurnal cycles of terrestrial ecosystem gas excanges determine the land-atmosphere interaction and land ecosystem function feedback to climate change. This study first incorporated 170 flux in-situ observations covering 11 ecosystem types to train and test the canopy irradiative tansfer model (SCOPE), Then latent heat flux, sensible heat flux, soil heat flux, GPP and SIF for each flux site generated by the SCOPE model. Finally, radom forest model machine learning method integrated with global gridded meteorology and remote sensing was applied to interpolate the site-level variables to global gridded hourly fluxes. This is an interesting study, and fill the scope of ESSD. And are attractive to the community.

However, I have some major concerns for its current version.

Comment 1: For the introduction, the author did not refer to the STEMMUS-SCOPE. I guess the authors try to use the SCOPE model to retrive SIF timeseries for each flux tower? Then I suggest to illustrate the significance of SIF in generating the global hourly gridded fluxes, since the authors have refered SCOPE model in the Abstract.

Response 1: We thank the reviewer for pointing out the need for clarification. In the revised introduction (lines 68-74), we have explicitly introduced STEMMUS-SCOPE model and clarified its role in our workflow. In our study, SIF is treated as an output variable rather than an input driver. Specifically, STEMMUS-SCOPE simulates hourly SIF alongside the five other flux variables at 170 PLUMBER2 sites. We then used simulated SIF time series as a target in our random forest upscaling. From this, the RF model generates a global 0.1° hourly SIF product at 685 and 740 nm (along with the other five variables) based on gridded remote-sensing and meteorological predictors.

The significance of SIF is briefly illustrated in lines 39-41, and we have added more sentences to illustrate its significance in lines 41-43. Including SIF explicitly as an output can reflect diurnal and stress-driven photosynthetic dynamics, thereby improving the physiological interpretability of our global hourly flux dataset.

For the result, site-level training and test.

Comment 2: 1.The author used site-level GPP and SIF to drive the RF interpplation(RF_OI)? Please show the accuracy comparison between GPP_scope and GPP_EC .

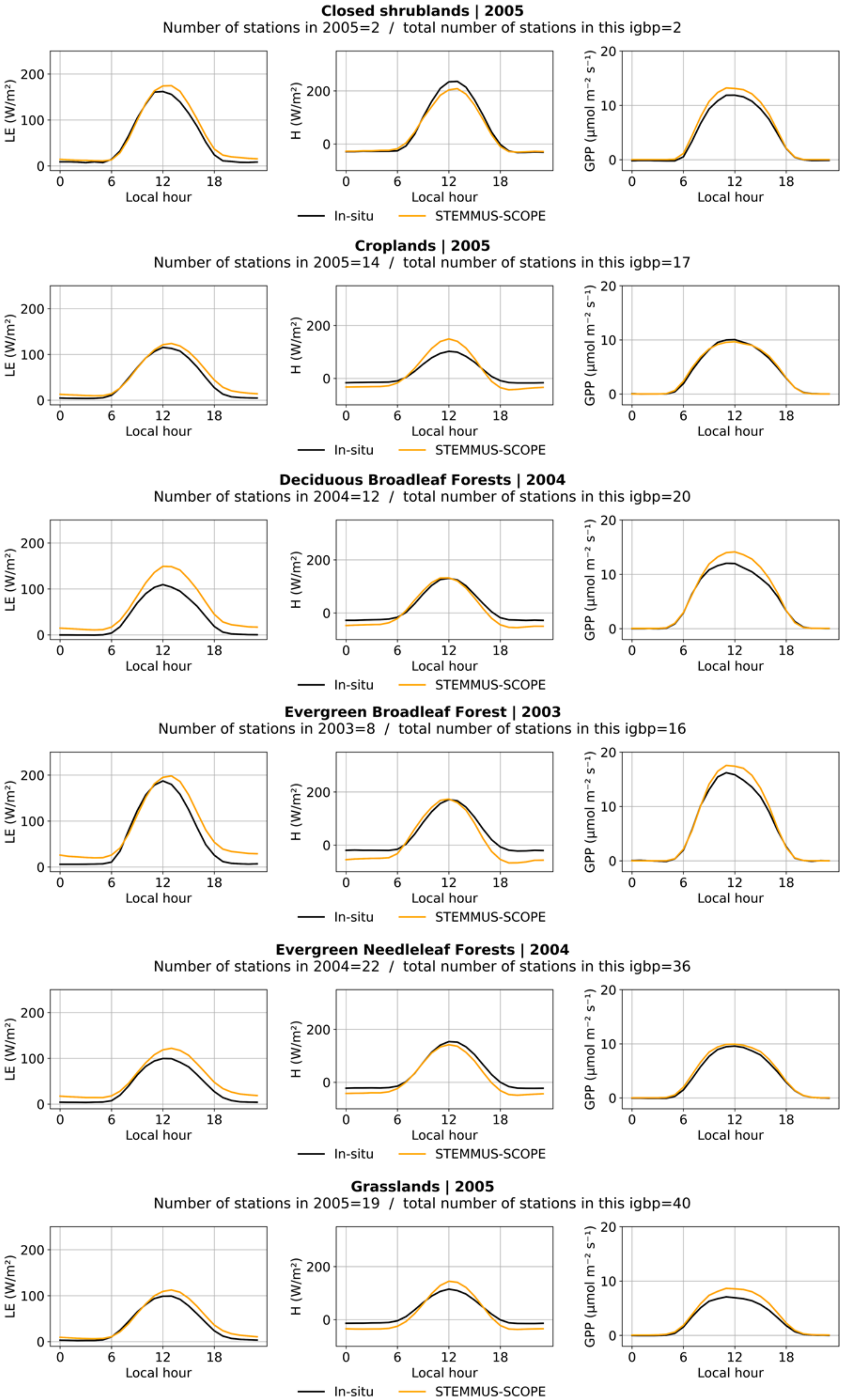
Response 2: Thank you for raising this important point. The comparison for GPP_STEMMUS_SCOPE and GPP_EC was already done in our another paper (Wang et al. 2025). https://www.nature.com/articles/s41597-025-05386-x

This is the text from the above mentioned paper: "The simulation of GPP is slightly more accurate than that of NEE. For GPP, the KGE ranged from -0.35 to 0.93 with a median value of 0.55 (median R^2, RMSE, rRMSE, and rSD values are 0.64, 3.79 μmol m−2 s−1, 6.15%, and 0.18, respectively)."

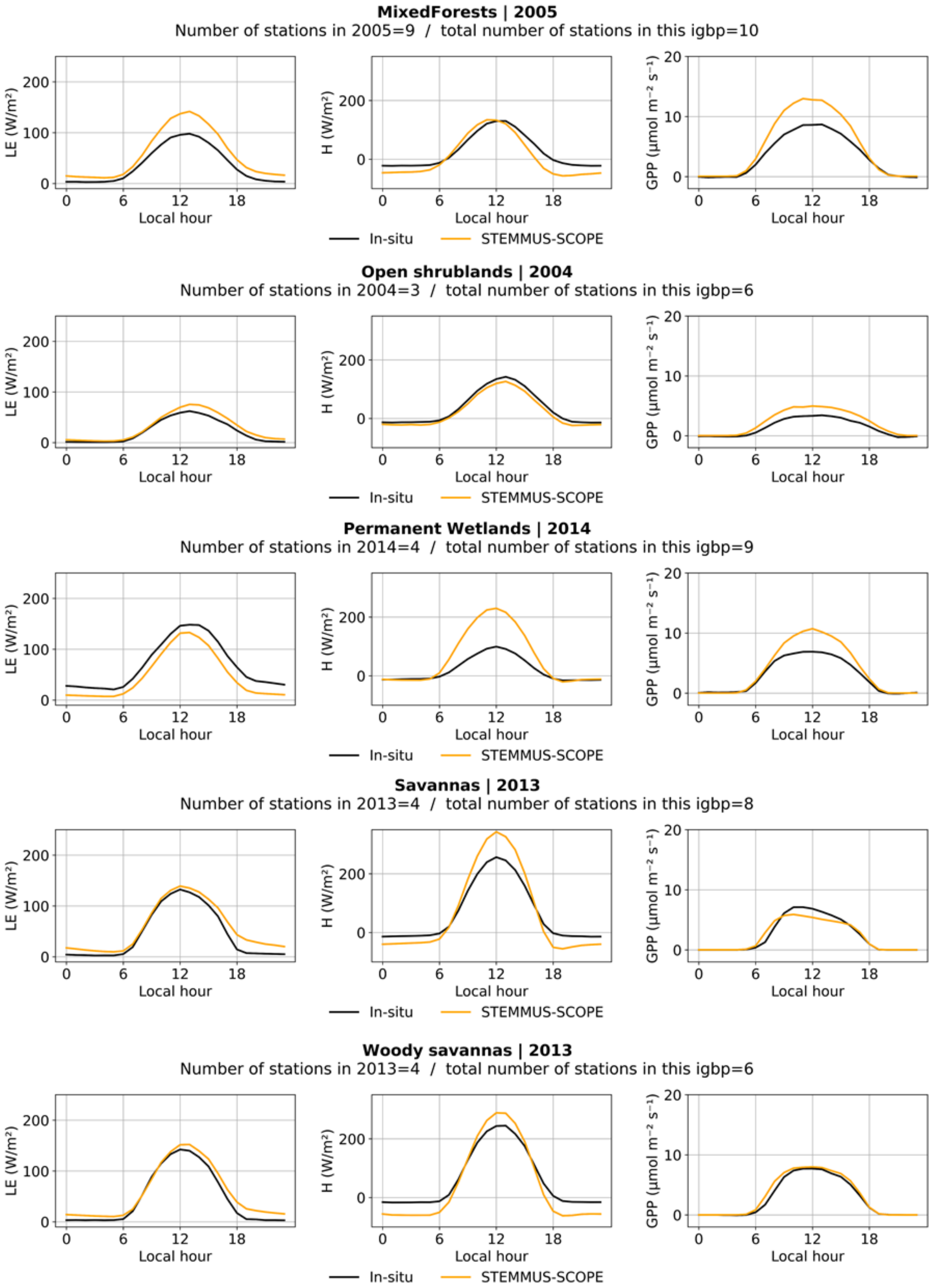
For SIF, we only used output from STEMMUS-SCOPE (due to the lack of in-situ data, but we compared our product with TROPOMISIF in Figure S9).

Comment 3: 2. Figure 3 and Figure 4 are good ways to show the technical issue of RF_OI. But I suggest to add analyze the diurnal variatons of GPP/LE/H for each IGBP class directly between the SCOPE output and EC tower. For example the the comparison (SCOPE v.s. EC tower) of mean diurnal cycle within one year for each IGBP class? within one season? This could tell the readers of message from IGBP classes.

Response 3: Thanks for pointing this out. We have made a new figure to compare GPP/LE/H for each IGBP class between STEMMUS-SCOPE output and EC tower observations. For each IGBP class, we first check which year most of the stations have data, then this year was chosen to make plot. For each IGBP class, we labelled the year we chose for plotting, and the number of stations in that year, and the total number of stations for each IGBP. This new figure is Figure S13 in Supplementary S5 (Section 5). The old S5 was changed to “S6”.



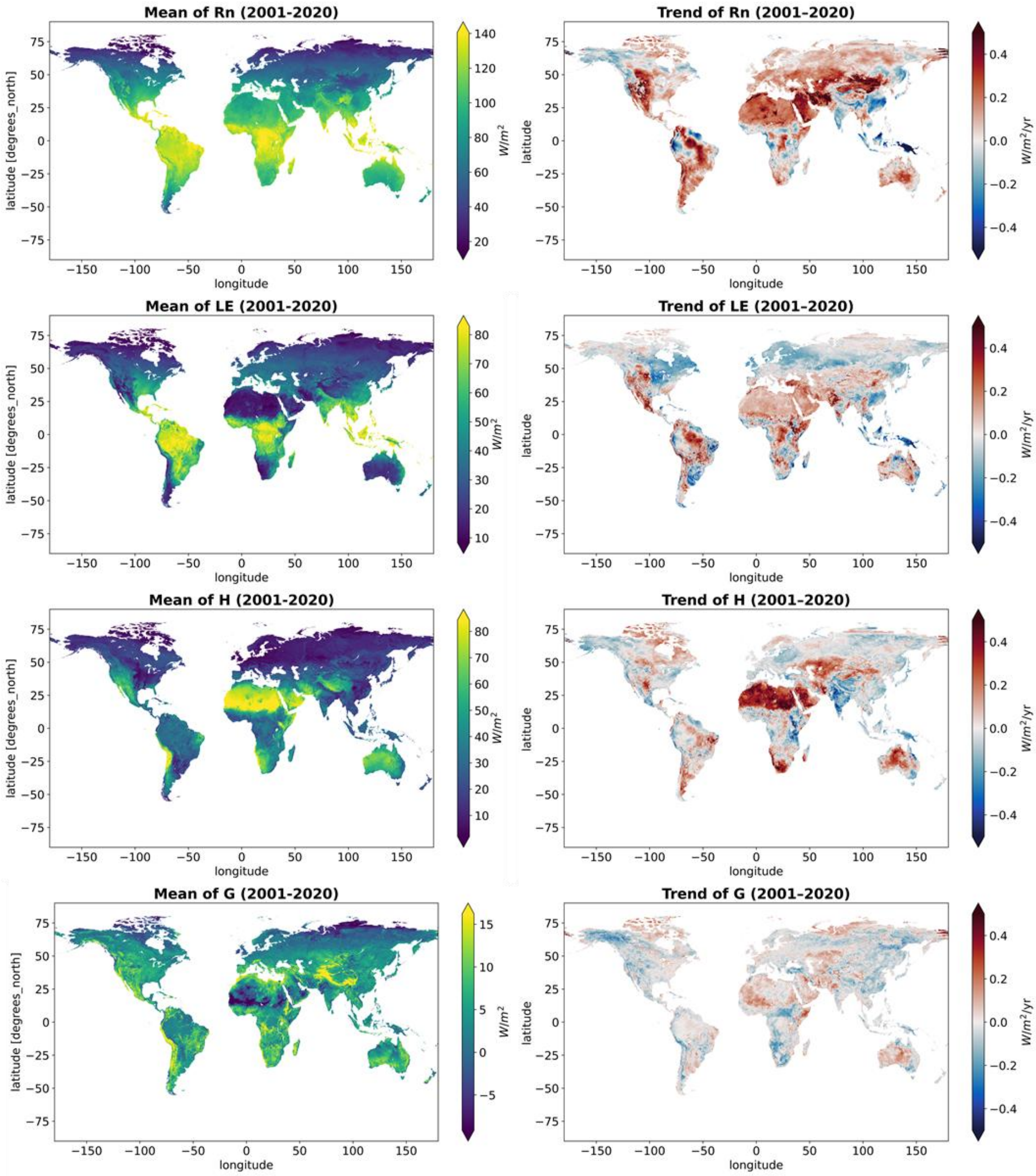
Closed shrublands | 2005
Number of stations in 2005=2 / total number of stations in this igbp=2
Croplands | 2005
Number of stations in 2005=14 / total number of stations in this igbp=17
Deciduous Broadleaf Forests | 2004
Number of stations in 2004=12 / total number of stations in this igbp=20
Evergreen Broadleaf Forest | 2003
Number of stations in 2003=8 / total number of stations in this igbp=16
Evergreen Needleleaf Forests | 2004
Number of stations in 2004=22 / total number of stations in this igbp=36
Grasslands | 2005
Number of stations in 2005=19 / total number of stations in this igbp=40
LE (W/m²)
H (W/m²)
GPP (μmol m⁻² s⁻¹)
Local hour
In-situ
STEMMUS-SCOPE

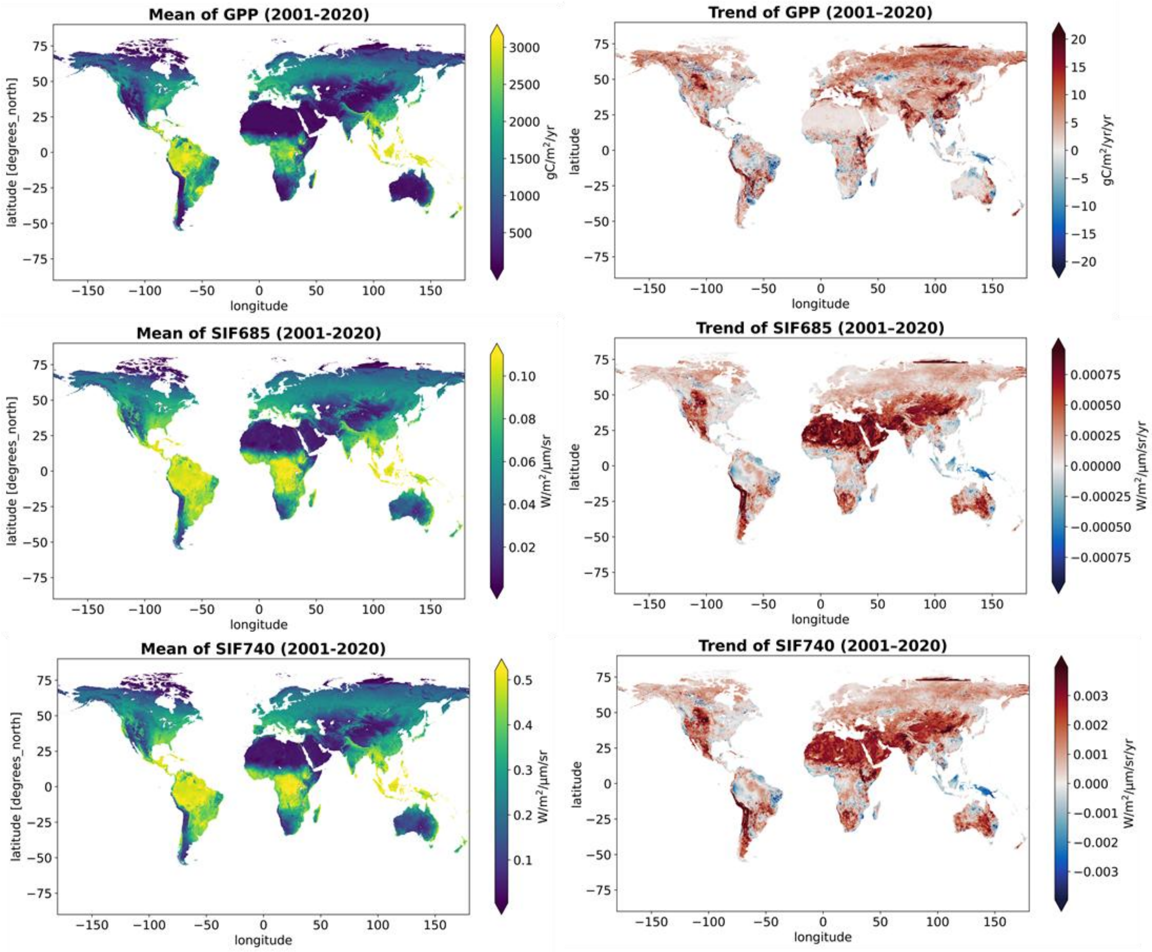


For the global gridded fluxes,

Comment 4: 1. If I do not miss the global patterns of hourly products, I did not find the global mean (magnitude) and trends map for each three wate-carbon-energy flux.

Response 4: This is a good point and we agree that the global mean and trends map are important. We have calculated 2001-2020 mean (because 2000 miss data for Jan and Feb, so it is excluded) and trend maps for 2001-2020 annual mean. This new figure is Figure S14 in Supplementary S5 (Section 5).





Comment 5: 2. And then inter-comparison of the mapping pattern between RF_OI and existing hourly products such as FLUXCOM. Currently, the author only show the intercomparison map of LE (Figure 6)?

Response 5: Thanks for noticing this. Figure S9 shows the inter-comparison of Rn, H, GPP and SIF740. In lines 249-276, we described that “Figures 6 and S9 illustrates the spatial distribution of Rn, LE, H, GPP and SIF740 across different datasets.” with detailed descriptions.

In line 251, we modified from “Figures 7 and S8” to “Figure 6 and S9”. This was an error.

In line 272, we modified from “Figure S8” to “Figure S9”. This was an error.

Comment 6: 2. Please explain the rational for the slected 8 regions to decompose the global product. Why not select the global plant function type classes or K-G climate classes.

Response 6: Actually the selected 8 regions were based on K-G climate classes, in lines 283-286. These 8 regions cover five main climate zones, and the K-G climate classes each region covers is listed in Table 3.

Comment 7: 3.Also, the author showed the diurnal cycle of global LE and GPP hourly fluxes for the 8 regions, respectively, but we did not find the 8 regions analyze for global hourly fluxes.

Response 7: It is described in lines 289-306, not only hourly LE and GPP, but also Rn, H, and G which are corresponding to Figures S10-S12.

RC2

We sincerely thank the Editor, Associate Editor and Reviewers for handling and taking the time to read and review our manuscript. We greatly appreciate the reviewer's insightful and constructive remarks. They have helped us improve both the scientific rigor and the readability of the manuscript. Below we address each comment in turn. Line numbers refer to the clean revised version unless otherwise indicated.

Land surface water-energy-carbon fluxes are key for understanding Earth's climate system. However, high-resolution data on water-energy-carbon fluxes at finer temporal scales remain limited. This study produced a new data that estimates these exchanges hourly during 2000-2020 by using STEMMUS-SCOPE model, field measurements, and machine learning with satellite and meteorological data. I believe this dataset could provide valuable insights into diurnal variability and finer-scale land-air processes. I recommend that this paper be accepted for publication after addressing the following comments.

Comment 1: 1. Note that estimating the water-energy-carbon fluxes at regional to global scales depend on interpolation processes. The authors applied the optimal interpolation to merge Rn, LE, and H from STEMMUS-SCOPE simulations with eddy covariance observations. Can the authors provide a reason or explanation to why this interpolation is reasonable or why this method can reduce the interpolation errors in the best possible way?

Response 1: Thank you for this insightful comment. We agree that fusing in-situ measurements with model simulations requires a principled and robust approach. In this study, Optimal Interpolation is a variance based data assimilation approach, to combine Rn, LE, and H from STEMMUS-SCOPE simulations and eddy covariance observations at flux tower sites. Optimal Interpolation assumes that both model outputs and observations contain errors characterized by their error variances and covariances. The method provides the best linear unbiased estimate (BLUE) of the target variables by minimizing the expected mean-square error, which is mathematically equivalent to a weighted least squares solution::

$$\mathrm{x} = \frac{var_{model}}{var_{obs} + var_{model}} * x_{obs} + \frac{var_{obs}}{var_{obs} + var_{model}} * x_{model}$$

In this formulation, daily variances for each data source were calculated based on 48 half-hourly values. The idea is to assign greater weight to the source that shows less fluctuation within a day, reflecting more stable data quality. This approach is especially beneficial in handling periods where eddy covariance observations may be less reliable. For instance, in situations such as nighttime periods or rainfall events where eddy covariance data tend to be noisy or unreliable, the STEMMUS-SCOPE simulations which is physically constrained receives more weight.

We have revised the description of section 3.4 Optimal Interpolation with the above explanation (Line 151-161). We observed that this simple and computationally efficient scheme can preserve diurnal variability while smoothing extreme outliers. The fused fluxes generated via optimal interpolation were exclusively at flux tower locations to enhance the quality of the training data for Random Forest model. The Random Forest model, driven by satellite and meteorological inputs, was then used to generate the global gridded fluxes.

Comment 2: 2. Section 3.5: The authors said that they used three commonly used statistical evaluation metrics. What's the third one, except for RMSE and r?

Response 2: Thanks for noticing this. This was a typo, should be two instead of three. We have changed “three” to “two” in line 164.

Comment 3: 3. The authors should acknowledge the limitations and biases in the STEMMUS-SCOPE simulations in the Discussion Section.

Response 3: We thank the reviewer for this valuable comment. In fact, we have acknowledged and discussed the limitations and biases of the STEMMUS-SCOPE simulations in the Discussion Section 5.1 (lines 319-328). We elaborated on the model performance and its varying accuracy across variables and vegetation types. We have now revised the paragraph slightly to more explicitly frame it as a discussion of model limitations and biases to improve clarity (lines 317-319).

In addition, we have now added a concluding sentence in lines 328-329 to this paragraph to explicitly emphasize the limitations in model applicability: “These results suggest that while STEMMUS-SCOPE performs reliably under certain vegetation conditions, its applicability may be limited in ecosystems with high heterogeneity or lacking comprehensive observational data.”

Specific comments:

Comment 4: 1. line 12: “First the integrated STEMMUS-SCOPE model” ---> “First, …” Suggested to separate by a comma. Similarly, line 15 ---> “Next, …”

Response 4: Thanks for your suggestion. We have added comma in line 12 and 15.

Comment 5: 2. line 124: What does “Method ML” mean?

Response 5: Thanks for pointing this out. We have changed “Method ML” to “Machine Learning Method”.

Advice received during initial submission

1) Please ensure that the colour schemes used in your maps and charts allow readers with colour vision deficiencies to correctly interpret your findings. Please check your figures using the Coblis – Color Blindness Simulator (https://www.color-blindness.com/coblis-color-blindness-simulator/) and revise the colour schemes accordingly. --> Figs. 4, 5, S5, S9

Response: Thanks a lot for reminding us. We have checked our figures (Figs 4, 5, S5, S9, 6, 7, 8, S10, S11, S12) with Coblis, and we changed to new color schemes which are friendly for readers with colour vision deficiencies..

2) Please number the sections of the supplement as "S1", "S2", etc.

Response: We have renumbered all sections in the Supplementary Information as S1–S5 (i.e., “Section 1” → “S1”, “Section 2” → “S2”, etc.). In the main text, we have also updated lines 92 and 311, now refer to “Section S1 of the Supplementary Information” (instead of “Section 1”).

Additional revision from our side

(1) In line 113, we have changed from “under review paper” to “(Phan and Fukui 2024)”.

(2) Figure S9, we have changed the legend from "FLUXCOM" to "TROPOMISIF".
(3) In Table 1, the link for LAI data was not correct, we have replaced with the correct link.

Reference

Wang, Y., Zeng, Y., Alidoost, F., Schilperoort, B., Song, Z., Yu, D., Tang, E., Han, Q., Liu, Z., & Peng, X. A physically consistent dataset of water-energy-carbon fluxes across the Soil-Plant-Atmosphere Continuum. *Sci. Data, 12*, 1146, 2025