Responses to Reviewer's Comment on "Gross primary productivity of forest ecosystems in a subtropical city and its decadal climatic and environmental drivers" by Lam et al. (MS No.: egusphere-2025-4647)

We sincerely thank the reviewers for the insightful and constructive comments, which have greatly improved the clarity and quality of our manuscript. Below, we would like to provide detailed responses to all points raised. The reviewers' comments are in blue font, our replies are in black font, our text cited from the manuscript are italicized, and our modified text are both *italicized* and highlighted in **bold**.

Response to Referee #1

Major concerns

Two points on the methodology caused me concern: The first relates to the timescales under consideration: your model (TEMIR-HK) produces hourly simulations of photosynthesis, but your study is addressing decadal trends. Could not the model be driven with longer-temporal inputs?

Thank you for your comment. The model, based upon Farquhar's photosynthesis model (Farquhar et al., 1980), is designed to run at sub-daily scale so as to capture the rapidly changing ecophysiological processes and their diurnal cycles, which could be influenced by changes in the environment (e.g., radiation, temperature) within hours. To capture the photosynthetic responses to changing climatic conditions, it is deemed crucial to capture the nonlinear responses to changes in the diurnal cycle in addition to the daily or monthly average conditions, in order to have more reliable aggregated results, regardless of the simulation timespan (e.g. Oliver et al. (2018)).

Moreover, one of the distinct advantages of our study is that temporally continuous and spatially dense meteorological observations are available from the numerous weather stations in Hong Kong. We could therefore build on these hourly observations and simulate the corresponding photosynthetic responses.

At several points in the text, you discuss photosynthetic responses to temperature but it is not made clear if these are instantaneous or acclimated responses. There is no mention in your submission of acclimation, but this must be an important theme when considering long-term trends.

Thank you for raising this concern regarding thermal acclimation. Both instantaneous and acclimated responses to temperature are included in our model. There are thermal adjustments to the carboxylation capacity (V_{cmax}) and electron transport capacity (J_{max}) according to 10-day mean temperature as introduced by Kattge and Knorr (2007). Description regarding thermal acclimation in our model are now included in the model description subsection to make this clear:

"…The model considers a thermal acclimation for C3 plants on two parameters that determines photosynthetic capacity, namely carboxylation capacity (V_{cmax}) and electron transport capacity (J_{max}). The two parameters increase adaptively with elevated temperature as described in Kattge and Knorr (2007)."

The second point relates to your finding of a preeminent role for LAI in driving GPP trends. I am not an expert on satellite products, but I think it highly likely that MODIS LAI serves as an input to MODIS GPP through estimation of that fraction of light absorbed by the vegetation (fAPAR) which is a central term of most light use efficiency models. If that is indeed the case, then it can not be surprising that you find such a strong correlation with the 'observations'. You do concede a weakness in the model approach (L358), but I worry that the problem is much more fundamental.

Thank you for raising this crucial question. This is indeed a fundamental point for both this study and the model itself. Unlike a full ecosystem model, TEMIR-HK and its parent model TEMIR in their current version are categorized as a process-based model for ecophysiology (photosynthesis and stomatal behavior) integrated with remote sensing parameters (Zhu et al., 2024). Satellite LAI data are incorporated as input for upscaling leaf-level photosynthesis to canopy level. Ultimately, the statistical analysis represents the differences between light-use efficiency (LUE) model for MODIS GPP and process-based photosynthesis model for TEMIR-HK. In light of this, the focus of this study is to identify the climatic and environmental drivers behind the trends and interannual variabilities.

We have explored different GPP products for evaluation prior to the submission of this manuscript, including eddy covariance flux measurements and solar-induced fluorescence (SIF) products. Unfortunately, the only eddy covariance flux tower in Hong Kong is located in a wetland ecosystem, which is not a dominant plant type in Hong Kong and also currently not included in both TEMIR and TEMIR-HK. SIF products, for example global, OCO-2-based SIF product (GOSIF) GPP, does not provide fine spatial resolution data for this study. It offers dataset at the resolution of 0.05°×0.05° which is 100 times larger than one simulation grid cell of our model. We looked into how well our model performed against GOSIF GPP product in the early stages of this study as an order-of-magnitude check. The aggregated GPP from TEMIR-HK is relatively close to the value produced by GOSIF GPP from 2012 to 2018. The time series of yearly total GPP among GOSIF, MODIS and TEMIR-HK outputs have now been included in the supplementary materials as Figure S4. Using a satellite-derived GPP product, particularly MODIS GPP for model evaluation is the most robust choice that we finally opted for due to its superiority spatiotemporal resolution and coverage for the scale required for this study.

The wording "*observation*" have been replaced by "***satellite-derived GPP***" to avoid ambiguity, and the above discussion have been incorporated into the next revision.

Minor concerns

L19: remove 'vastly'

The word "*vastly*" has been removed.

L33-34: what is meant by feedback here?

It has been simplified as

*"…**total** CO_2 fertilization feedback contributes to 2.5 PgC yr^{-1} of terrestrial carbon uptake…"*

since the bidirectional atmosphere-biosphere interactions are not the focus of the study.

L36-38: somewhere in here you need to distinguish between instantaneous and acclimated responses.

It has been rewritten as

*"It is well established that the **instantaneous** temperature response curve of leaf photosynthesis follows a parabolic shape: photosynthesis rate increases with temperature until an optimum temperature is reached,…**Under consistently elevated temperatures, plants can adapt to it by increasing their photosynthetic capacity, which is known as temperature acclimation (Kattge and Knorr, 2007), despite diverging species-specific responses (Way et al., 2015)** ."*

L47-48: try rewording - you are talking about conductance of CO2? How does uptake cause damage? What does ozone do inside the leaf?

A short description regarding ozone damage to cell walls have been included:

"Tropospheric O_3 enters the plant through the stomata, where it undergoes chemical reactions that damage cell walls or even result in cell mortality (Long and Naidu, 2002)."

L60: but stomatal closure is a plant response to water shortage?

Not only stomatal closure is a plant response to water shortage, but also response to elevated atmospheric CO_2. A short elaboration on stomatal responses has been included:

"Plants control the openness of stomata to regulate both carbon uptake and water loss through transpiration, which are regarded as competing demands."

L63-64: unsure what this sentence means.

It has been rephrased as

"Climatic, environmental and human factors also induce changes in photosynthesis indirectly by altering canopy structure"

to improve readability.

L135-136: more explanation required here; how are both sunlit and shaded leaves incorporated into a single big leaf?

Further elaborations have been included as

"…the radiative transfer model, direct and diffuse radiation are used to calculate the absorbed photosynthetically active radiation (PAR) by sunlit and shaded leaves, following a ***"two-leaf"*** *approach.* ***This is an extension to the "big-leaf" approach, which represents the plant canopy as a single leaf with canopy-average photosynthetic parameters that are calculated using proper scaling to account for vertical variations in light attenuation and leaf nitrogen content. The "two-leaf" approach used here essentially builds on the big-leaf approach, but also uses a canopy radiative transfer model to partition the canopy into the sunlit and shaded parts that operate with different photosynthesis rates (Bonan et al., 2021).****"*

L136-138: this assumes too much prior knowledge on the part of the reader. And the Farquhar model makes no mention of PFTs.

It has been refined as

"Leaf photosynthesis is calculated with the standard Farquhar photosynthesis model, which distinguishes among three limiting regimes based on chloroplasmic enzyme kinetics, electron transport chain, cellular biochemistry, as functions of intercellular CO_2 concentration, light intensity, and biochemical parameters. The limiting regimes can be understood as enzyme-substrate-limited regime, light-limited regime, and photosynthate export-limited regime (Bonan, 2016). We introduced PFT-specific parameters in the model"

to avoid confusion and also improve readability.

Table 1: Is there not a case for testing the effect of some metric of water availability e.g. VPD? Or is Hong Kong never short of water?

There is not a specific case for water availability, but the effect of atmospheric dryness and soil water availability have been taken into account. We used relative humidity (which is a proxy for VPD effects) and soil water data to drive the simulations and the processes are included in the model.

There are rooms for future focus on plant hydraulic responses to water availability and the corresponding advancing model developments.

L179-180: it's not clear here how those affect your chosen model (Fig 1). Useful to have some explanatory text here.

It has been further elaborated as

"***They serve as boundary conditions for the soil component in the model***".

L186: try rewording. MODIS GPP is adopted as your 'observations'?

It has been rephrased as

"***We used Moderate Resolution Imaging Spectroradiometer (MODIS) MOD17A2HGF Version 6.1 GPP product (hereafter "MODIS GPP") as the proxy dataset for model evaluation***",

and the wording "*observation*" has been replaced by "***satellite-derived GPP***" throughout the entire document.

Figure 3: label the plots a, b, c for clarity. It is very difficult to make out the lines in (c). Tell us what each point represents.

The plots have been amended aesthetically according to the suggestions. Extra explanation has been included in the caption as

"The grey points indicate the values of the simulated and satellite-based GPP in each grid cell, and the colored contours show the density of the points".

L256: The slope of the line in 4c is 0.69 indicating that simulations underestimate??

The sentence in L256

"*…although an overestimation of the rising trend by TEMIR-HK is evident (Fig. 4c)*"

has been improved as

"***although TEMIR-HK overestimates the rising trend at low values and underestimates it at high values***".

Figure 5: it is difficult to distinguish the case lines in (a). Even so, I think it would be useful to include the trend of the 'observations' (MODIS).

The linewidth of the case lines have been reduced to improve visual readability and the time series of MODIS GPP has been included in (a).

L268-269: I don't follow why the Full and Additive do not agree.

The additive effect is a mathematical assumption. There are interactive effects between the climatic and environmental factors that a single driver case could not address. This phenomenon has been refined in the discussion as

"*The additive (CO_2 + LAI + Temperature + O_3 effects) trend is slightly higher (+12.4 %) than the full trend, indicating some interactive effects* ***between the drivers*** *might have offset part of the joint positive trend*".

L285-287 and Figure 6: I don't understand the correlation metrics here: the CO2 effect on GPP is flat and yet we have R=0.94?? And how does that flat line reconcile with Fig5b indicating that CO2 is second to LAI in influence?

The dotted lines are the normalized environmental variabilities which have standard magnitudes, while the solid lines are the detrended annual variabilities of the induced GPP changes. The CO_2 effect is small on GPP interannual variability, yet the temporal patterns between GPP IAV and the normalized CO_2 variability is similar, so the Pearson correlation shows decent result. The correlation indicates the level of linearity between the environmental variability and the induced IAV of GPP. The magnitude of the effect is explained separately by the standard deviation of IAV of GPP, which can be deduced from the solid lines in this figure.

GPP dynamics were dissected into two dimensions: trends (Fig. 5) and interannual variability (Fig. 6). The results illustrated from the two dimensions do not necessarily have to be consistent with each other.

The above elaboration is now included in Section 3.4 as in the following. We have also included the elaboration in the caption of Fig. 6 to avoid confusion.

"The correlation indicates the level of linearity between the environmental variability and the induced IAV of GPP. The magnitude of the effect is explained separately by the standard deviation of IAV of GPP, which can be deduced from the solid lines in Fig. 6".

L326: soil fertility?

The word "*fertility*" will be more precisely described as "***soil fertility***".

L333-334: try rewording, this reads as if the radiation becomes saturated. It is the (GPP?) response that levels off.

It has been rewritten as

"*Rising LAI generally first result in an almost linear increase in GPP, but as LAI increases further, the dense leaves would attenuate more solar radiation,* ***in which absorbed radiation per leaf area would later reach a plateau*** *after a certain LAI threshold. The growth rate of GPP would thus decline when LAI surpasses that threshold*".

L371-373: again the time period under consideration is important.

The time information has been included as

"***A 2-year translocation*** *study from Li et al. (2016) showed certain species demonstrate a positive relationship between photosynthesis and variations in leaf temperature, whereas one particular species displays a decreased net photosynthetic rate in a warmer environment.*"

L379-380: see optimality approaches (e.g. Stocker et al. New Phytologist 2025 Vol. 245 Issue 1) where Vcmax is assumed to be independent of soil N availability.

Thank you for your suggestion. This interesting approach has been put into the discussion section and act as an important reference for the direction of our model development, especially when TEMIR is not a full ecosystem model that concerns N fluxes dynamically:

"An alternative approach was also put forth by Stocker et al. (2025) that estimates V_{cmax} by optimizing carbon assimilation relative to transpiration water loss. This could be an essential reference for our future model development since the proxies of photosynthesis are dynamic under changing atmospheric environment. The proxies are then also independent of soil nitrogen content, which is usually taken from global databases."

L404-406: but vegetations differ in their sequestration potential e.g. long-lived trees versus grasses. All green (as seen from space) is not equal in this context.

Thank you for the clarification. The intention of the statement was to state the practical difficulty to "plant more tree". We have rewritten the statement as

"reforestation measures would not be beneficial toward neutrality targets as it is difficult to further increase the ***coverage. Instead, the focus should be 1. preserving existing ecosystems, whereby any further loss in land carbon sink is another burden to mitigation strategies, and 2. replantation (already ongoing), whereby competitive exotic Acacia species are replaced with native ones that have a higher carbon sequestration potential****"*.

Responses to Referee #2

Major concerns

The manuscript primarily evaluates modeled GPP against MODIS MOD17 GPP, which is itself a modeled product strongly constrained by remotely sensed canopy structure (FPAR/LAI). Because MODIS LAI is also a key input in your simulations, the reported spatial agreement may partly reflect shared inputs rather than an independent validation of GPP simulations. Why do use independent data (e.g., FLUXCOM-X, SIF products, or local eddy-covariance data) to validate the model's performance?

Thank you for raising this crucial question. The other referee has a similar concern. Please refer to our response above and the revised text therein.

It is not clear whether any parameter calibration or local constraint was performed for Hong Kong vegetation (e.g., using eddy-covariance site observations, trait databases, or parameter tuning).

Thank you for pointing out the ambiguity. We explored certain calibration opportunities at the beginning of the project. The major limitation is that no existing local observations were available. As mentioned above, the only eddy-covariance site is located in a wetland ecosystem that is not suitable for the purpose of this study.

Such limitations are now discussed in the manuscript (L391 to L402):

"Collecting local trait data (e.g., V_{cmax}) is possible yet highly resource consuming, especially for subtropical and tropical terrestrial ecosystems comprising such diverse plant species. The data collection includes not only gas-exchange measurements for dominant species in Hong Kong, but also their corresponding relative abundances in terms of PFTs. The manpower and financial budget for this project were insufficient to support such intensive field work. As a compromise, we made use of the trait database from (Kattge et al., 2009), which is also the default option in the model."

The authors partitioned grid-level LAI into PFT-specific LAI using a "base-year PFT LAI" and a single scaling factor. The appears to assume that all PFTs within a pixel vary synchronously, which is problematic. The authors need to clarify how the "base year" was selected and justify why a single scalar is sufficient to capture the phenological differences between competing PFTs in a mixed pixel.

We agree that in reality the phenological differences between competing PFTs could not be explained by a single scalar.

We have now incorporated this discussion of assumptions in the methodology section:

"We acknowledge that the phenological differences between competing PFTs could not be explained by a single scalar. It was a compromising decision due to methodological limitations. On one hand, we had a high-resolution (30-m resolution) land cover dataset that allows multi-PFT simulations on a single grid. On the other hand, LAI data resolution is much coarser than that of the land cover dataset, covering only one value per grid cell. It presented a dilemma whether we should adopt a multi-PFT approach based on land cover data, or a single-PFT approach based on LAI data. Both approaches require certain levels of presumptions and compromises with the model. We adopted the former approach and assumed that the ratios of LAI among different PFTs in a mixed pixel are temporally static but spatially dynamic."

Minor concerns

L13: Units for trend and interannual variability are not consistent

Thank you for pointing out the mistake. We have corrected the unit.

L30: Typo "Michaelis-Menten"

Thank you for pointing out the mistake. It has been corrected to "***Michaelis-Menten"***

L166: "base year" is not defined. Specify what year it refers to and how $LAI_{base,PFT}$ is derived and whether it varies by space/season.

Thank you very much for pointing out this ambiguity. We have adjusted the section and provide additional information as

*"**TEMIR takes in gridded LAI values and further processes them into PFT-specific values. In TEMIR-HK we applied the same approach but the process was computed outside of the model due to model structure difficulties.** We adopted MODIS MCD15A3H v006 4-day composite LAI data product with 500 m resolution. Daily LAI values were linearly interpolated from the dataset. The dataset was further processed to be PFT-specific using current day PFT fraction (PFT frac) and a PFT-specific LAI from a base year ($LAI_{base,PFT}$) (Figure 1). **We used the default base-year PFT-specific LAI from TEMIR, which was derived and deaggregated from MODIS satellite data as described in Lawrence and Chase (2007). This prescribed gridded PFT-specific LAI has a daily resolution for year 2000, which reflects both spatial and temporal (including seasonal) changes in the ratios.** This approach allocates the current day total LAI of the grid cells to different PFTs based on the PFT fractions."*

L243: Check the unit for IAV

Thank you for pointing out the mistake. We have corrected the unit.

L296: Typo “Yangtsz River Delta”

Thank you for pointing out the mistake. It has been corrected to “***Yangtze River Delta”***.

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