

We would like to thank both reviewers for their detailed and insightful comments. These comments have helped improve and clarify the submitted manuscript. Below we reply to each comment point by point, showing the reviewers' comments in black and our responses in blue. Changes to the original manuscript are highlighted in **bold blue**. Note that the line numbers in the response are updated based on the revised manuscript, which we provide with our response.

We note already here that we reran the numerical experiments related to FI-Hyy and US-Ha1, as we detect an error related to multi-year LAI read.

Reviewer #2

Zhu et al. have greatly improved the clarity of their manuscript and mostly made adequate changes and responses to the reviewer comments on the first round. However, I still have two major concerns regarding the paper.

Response: We thank the reviewers for recognizing our efforts in the first round of reviews. After carefully considering your feedback, we fully understand your two major concerns and appreciate the opportunity to address them. Your insights are invaluable in helping us refine our work, and we have answered both questions in detail.

Continuing the discussion related to the US-Wrc site gradient fluxes. Note that there are two papers by Rastogi et al. in 2018 using gradient flux data from the same site; one has data from 2014 and COS fluxes are (for whatever reason) not published (<https://doi.org/10.1029/2018JG004430>) and the other reports fluxes from year 2015 (<https://doi.org/10.5194/bg-15-7127-2018>). Dataset for the latter (including gradient fluxes of COS) can be found from: <https://zenodo.org/records/1516332>. To reduce the considerable bias the authors currently have regarding the calculation of US-Wrc fluxes, I highly recommend to use the published COS gradient fluxes from this site, from year 2015, and to rerun the analysis once more using this dataset. This would considerably reduce bias and improve the analysis. These data can then also be used in the two-site assimilation, which, I still in its current state (when gradient fluxes are first calculated using simulations, which then are again used to simulate fluxes) I do not approve of. If proper gradient fluxes provided by Rastogi et al (in the link above) are used, only then the two-site assimilation is possible. Note that Hyytiälä forest also has flux measurements in 2015.

Response: Thank you for your investigation of the COS data at the US-Wrc site, and the advice to us in conducting assimilation experiments. COS flux measurements reported in Rastogi et al. (2018a) are from between 18 April and 31 December 2015. However, only the COS flux observations in October 2015 are available via <https://zenodo.org/records/1516332>, and we found that this dataset contains only 93 COS flux observations. Since our study focused on the hourly data combining the hourly-scale BEPS model to improve the model's performance at hourly scale, we conducted our current work in the growing-season and also the period when there are enough hourly data for assimilation. After serious evaluation of the datasets, we determined that the 2014 dataset is more suitable for our study and can be compared with other sites which are also focusing on the growing season. However, we highly appreciate the

reviewer for suggestions of alternative datasets that can potentially improve our study in the next step, by assimilating datasets from different periods.

Secondly, as you mentioned below: “observations would not be perfect, but they are ‘the best guess’ we have”, the lack of necessary parameters makes the COS fluxes estimated from COS concentration observations unreliable at US-Wrc. Therefore, we employed a correction method proposed by Leung et al. (1999) to estimate COS flux by combining COS observation and simulation. The effectiveness of this method has been validated by previous studies and is widely used (Scholze et al., 2016; Wu et al., 2020). In fact, we have also provided the diurnal variation of the corrected COS fluxes in the manuscript (Figure 3m). While the diurnal variation of the corrected COS fluxes still has some differences compared to that in Figure 4 in Rastogi et al. (2018b), they are indeed very similar, which gives us more confidence in utilizing these data for conducting assimilation experiments.

Last but not least, we do recognize that there will be errors in the measurement and processing of the data. Recognizing the imperfections and inconsistencies between theory (usually in the form of a numerical model) and observations, and combining the two to achieve the optimal estimation of the target variable, is precisely the core concept of data assimilation. The variational method employed in NUCAS takes full account of uncertainties of the model and observation, and utilized the optimization methods to minimize the difference between the model and observation (Which will not be zero, due to uncertainties in both the model and the observations). In our study, we performed a two-site assimilation experiment at FI-Hyy and US-Wrc. This experimental scheme is, in fact, an exploration of the solution to the problem of the large uncertainty in single-site COS data. With the availability of more COS data in the future, we will continue to explore solutions to the problem of large uncertainties in single-site data like this.

In order to further remind readers of the current inconsistencies in COS data processing methods, we specifically emphasize the US-Wrc site in **Section 4.5 Caveats and implications**: “Besides, the existing COS flux data were calculated based on different measurement methods and data processing steps, which poses considerable challenges for comparing COS flux measurements across sites, **“Particularly, as only raw COS concentrations was provided and a correction approach was employed, the estimated COS fluxes at US-Wrc may subject to considerable uncertainties.”** (Line 632-633)

The authors argue that sensible heat flux (H) and latent heat flux (LE) as well as soil water content (SWC) are related to COS fluxes because COS fluxes are related to transpiration. However, transpiration is only one part of ET (evapotranspiration, highly related to LE) and the other part is evaporation, which has no relation to COS fluxes. Evaporation and SWC are also highly related to water availability (precipitation) as well as other environmental variables (radiation, temperature). In addition, it is definitely not only the leaf-scale energy demand that controls the sensible heat flux at ecosystem scale. You forget soil, atmospheric turbulence, input energy from the sun, ground heat flux, evaporation, precipitation, saturation of SWC... Yes, COS fluxes could be used to estimate transpiration, but anything further is overinterpretation. Thus, I still very highly recommend completely leaving out the LE, H and SWC simulations.

Response: Thanks for your detail comment. Carbonyl sulfide (COS) has been proven as a promising tracer for photosynthesis and transpiration due to the coupling of leaf exchange of COS, CO₂ and H₂O through stomata, and the photosynthesis and transpiration play a significant role in leaf energy balance. The relationship between leaf energy balance (latent heat (LE), sensible heat (H)) and photosynthesis, and transpiration has been described in detail (e.g., in Leuning et al. (1995)). Taking into account the coupling of photosynthesis, stomatal conductance, transpiration, leaf energy balance, current ecosystem models (e.g., BEPS) have also realized the coupled simulation of them through the Ball-Berry model (Ball et al., 1987), Penman-Monteith equation (Penman, 1948; Monteith, 1965), etc. Therefore, we collected H, LE as well as SWC observations for investigating the optimization ability of COS for the model because they are related with transpiration. We note here that ecosystem-scale LE observations have been used to investigate the ability of COS to constrain transpiration (Abadie et al., 2023). Following Luo et al. (2018), we output each component of evapotranspiration (ET) and H separately, and confirmed that changes in simulated ET and H are dominated by transpiration and canopy sensible heat.

Following reviewer #3's comment: "Generally, I would say that the results for H, LE and SWC don't bring a huge amount to the paper. And large portions could be condensed or moved to supplementary material. But it certainly **does not** detract from the paper. However, it somewhat distracts the reader...", we therefore removed this section from the manuscript and present them only in the supplement.

From the response document:

L400: "due to high value of observation" or rather underestimation by simulation?

Response: Could, of course, but either, according to Kooijmans et al. (2021), the air depleted in COS can then suddenly be captured by the EC system when turbulence is enhanced in the morning.

» This is why we do storage correction to EC fluxes! Storage corrected fluxes do **not** have this problem. I am not saying that observations would be perfect, but they are "the best guess" we have. Thus, I suggest to reformulate accordingly.

Response: Thank you for your comment. But, I apologize that we have removed that sentence in the first round of review.

Specific comments:

How is this manuscript related to a preprint that is simultaneously in review (Zhu et al., 2024)? The other study seems very much related, and should be cited in this study as well.

Response: Thanks for your comment. In another manuscript, a Monte Carlo based approach was utilized to optimize GPP using COS. This topic is indeed relevant to this manuscript and we have included a reference to it. The revised sentence reads as: "**As an important probe for characterizing stomatal conductance, COS has shown great potential to constrain plant photosynthesis and transpiration and to improve understanding of the water-carbon coupling (Wohlfahrt et al., 2012; Asaf et al., 2013; Wehr et al., 2017; Kooijmans et al.,**

2019; Sun et al., 2022; Zhu et al., 2024).” (Line 59-56)

The abstract and conclusions are still missing concrete results. The authors use descriptive words such as “improved”-> improved by how much or by what metric? Describe in detail (using numbers) what were the most important results of your study (e.g. how much (in %) did the assimilation improve the prior simulation etc).

Response: Thank you for your valuable comment. Our results suggest that the assimilating of COS fluxes can notably improves the model performance in GPP and ET, with average root mean square error (RMSE) reductions of 23.54% and 16.96%, respectively. For H, the RMSE of the simulations and observations exhibited little change before and after assimilation, while the R^2 increased, on average, by 0.07. In response to your comment, we have modified the abstract to provide a quantitative description of the assimilation results: “**Comparing model simulations with validation datasets, we found that assimilating COS fluxes notably improves the model performance in gross primary productivity and evapotranspiration, with average root mean square error (RMSE) reductions of 23.54% and 16.96%, respectively.**” (Line 27-29)

Merge Figs 3 and 4 in a similar way as Fig. 6.

Response: Thanks for your comment. We have merged this two Figures as you suggested.

Eq. 9-10: $F_{cos,biotic}$ is switched to $FSWC_g$ and SWC to SWC_g between the equations. Please check that is consistent.

Response: Thank you for your comment. We have checked the manuscript and confirmed that there are no problems with the model description. According to Whelan et al. (2016) and Whelan et al. (2022), the model assumes that COS soil biotic uptake changes with SWC , and there exists an optimum SWC (SWC_{opt}) at which the simulated biotic COS flux is maximized, i.e. optimum in terms of COS soil biotic uptake (F_{opt}). Base on SWC_{opt} , F_{opt} , and the COS flux (F_g) under another soil moisture condition (SWC_g , and $SWC_g > SWC_{opt}$), it is thus possible to determine the unique shape parameter a for which the model varies with SWC and, in turn, to calculate soil biotic COS fluxes ($F_{COS,biotic}$) for any SWC condition.

$$a = \ln\left(\frac{F_{opt}}{F_{SWC_g}}\right) * \left(\ln\left(\frac{SWC_{opt}}{SWC_g}\right) + \left(\frac{SWC_g}{SWC_{opt}} - 1\right)\right)^{-1} \quad (1)$$

$$F_{COS,biotic} = F_{opt} \left(\frac{SWC}{SWC_{opt}}\right) * e^{-a\left(\frac{SWC}{SWC_{opt}} - 1\right)} \quad (2)$$

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Response: Thank you very much for the references.

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Reviewer #3

Zhu et al. present a novel study that utilises the Nanjing University Carbon Assimilation System (NUCAS) v1.0 data assimilation framework and the process-based terrestrial ecosystem model Boreal Ecosystem Productivity Simulator (BEPS) is used as the adjoint model. The authors focus on simulating carbonyl sulfide (COS) fluxes at an ecosystem scale. Additionally, prior and posterior estimates of gross primary productivity (GPP), soil water content (SWC), sensible heat (H) and latent heat (LE) are presented. Assimilating COS flux measurements is a novel approach to better our understanding of COS processes, with regards to the carbon and water cycles, and the energy budget. This research is certainly within the scope of GMD and should be considered for publication following some minor corrections.

Having read the author's response to referee comments, Zhu et al. have clearly made a significant effort to improve their work. This is evidenced in the improved narrative of the paper and clarification on some of the methodology and data inputs. However, there is room for a bit more improvement in the readability, which my recommendations are focused on.

Response: Thank you for recognizing our efforts in the first round of reviews. We sincerely appreciate your detailed comments on improving the readability of the manuscript and the opportunity to address them. Your insights are invaluable in helping us refine our work, and we have answered your comments in detail, as shown below.

General Comments:

The abstract would benefit from having at least one specific quantifiable metric that shows the improvements in COS flux or GPP made by NUCAS.

Response: Thank you for your valuable comment. We have changed the relevant sentence in the abstract to provide a quantitative description: **“Comparing model simulations with validation datasets, we found that assimilating COS fluxes notably improves the model performance in gross primary productivity and evapotranspiration, with average root mean square error (RMSE) reductions of 23.54% and 16.96%, respectively.”** (Line 27-29)

There is use phrases like ‘various constants’ and other ambiguous phrases. I understand that elaborating in every instance could lead to excessive description of basic processes or repetition. But there is flexibility for some expansion on the use of these phrases. They often offer context and provide insight to readers who may not be familiar with this field, especially that of carbonyl sulfide which is rapidly becoming a field of its own within the world of carbon cycle science. An example on Lines 129-130: “The model parameters are the various constants that are not influenced by the model state.”

Response: Thank you for your comment. We have revisited this and elaborated in more detail.

Sections 1 and 2 provide a thorough introduction to the topic and the methodology used in this work. However, the description of variables used in the modelling and assimilation is a bit light in places, leaving a lot of work for the reader to piece together. This harms to reproducibility of this work and makes it relatively inaccessible to a reader who is not familiar with matrix inversion, Bayesian statistics or data assimilation.

- A sentence distinguishing biotic and abiotic soil processes would be helpful in Section 2.1.3.

Response: Thank you for your comment. In response to your comments, we have provided additional clarification on soil biotic and abiotic processes as detailed below:

“ $F_{COS,abiotic}$ is controlled by abiotic degradation of soil organic matter (Whelan and Rhew, 2015), can be described as an exponential function of the temperature of soil T_{soil} (°C).” (Line 182-183)

“ $F_{COS,biotic}$ is attributed to CA in microbial communities (Sauze et al., 2017), calculated according to Behrendt et al. (2014) and Whelan et al. (2016): ” (Line 186-187)

- Elaborate a bit on the parameters being referred to in Section 2.2.

Response: Thanks for your comment. In response to your comment, we provide a more detailed description of the model parameters in Section 2.2, especially the five parameters optimized in this study. as shown below: **“NUCAS v1.0 can optimize 76 parameters belonging to BEPS. Of these parameters, some are global (i.e., the ratio of photosynthetically active radiation to shortwave radiation (f_{leaf})), and others differentiated by PFT (i.e., maximum carboxylation rate of Rubisco at 25°C (V_{cmax25})) or soil texture class (i.e., $Ksat_{scalar}$, the scaling factor of saturated hydraulic conductivity ($Ksat$)).”** (Line 200-203)

- Expand on the addition made in this iteration in Section 2.4.3, regarding coupling of COS with LE, H and SWC. It’s still a bit light. At the very least include some additional references. Alternatively, this could be elaborated on in the Discussion (Section 4), to which you could point the reader to from Section 2.4.3.

- After giving this point some further thought, perhaps elaboration would be best suited to when the main scientific questions are presented in Section 1. I will leave this up to the discrepancy of the author(s).

Thank you for your valuable comments. We have changed the sentence as you suggested, i.e., while adding more references, we have clarified to the reader that a more detailed elaboration will be provided in the Discussion (Section 4). The revised sentences read as: **“Due to the coupling between leaf exchange of COS, CO₂ and H₂O, GPP, and ET data are selected to evaluate the model performance of COS assimilation in this study. In addition, we further explored the ability of COS to constrain H simulations, since the transpiration contribute to a decrease in temperature within the leaf (Gates, 1968; Konarska et al., 2016), and the leaf-air temperature gradient is a key control factor of H (Monteith and Unsworth, 2013; Dong et al., 2017). Moreover, SWC is used in model evaluation as the key role of SWC in modelling $F_{COS,biotic}$ (as shown in Eq. (9)) and that the water dissipated in transpiration originates from soil (Berry et al., 2006). A more detailed elaboration will be provided in the discussion.”** (Line 280-285)

- Section 3 is a thorough summary of the results. However, there are 2 key points that could

be improved, firstly some more summary of the implication of what the parameters mean, i.e. what does it mean if the relative change in VJ_slope is large for example. Does it mean the assimilation has changed the posterior results significantly with regards to the prior and if so, what does that actually tell us? The summary at the end of 3.6 is a good example of where this has been done well. A few more sentences like that would be good! Secondly and a much more minor point, there could be more specific reference to which figure and sub-figure is being referred to. It doesn't need to be excessive but try and help the reader from having to constantly check tables to cross-reference the PFT and soil texture between sites when this is referenced, for example. A good example of this occurring is at the start of Section 3.7, where you introduce the topic of this section and then just point the reader to Figure S4-S7. Provide a bit more guidance and refer to them as they are discussed in turn – I have included some specifics in the minor comments.

Response: Thanks for your kindly comment. In response to your another comment, “Elaborate a bit on the parameters being referred to in Section 2.2.”, we have we provide a more detailed description of the model parameters in Section 2.2. For VJ_slope, the slope of the V_{cmax} and J_{max} (maximum electron transport rate) relationship, a low value of it indicates that photosynthesis at this site is more likely to be limited by the rate of the light reaction.

Thank you for your comment regarding to providing more specific reference to figure and sub-figure, we have made improvements in this regard to enhance the readability of the manuscript.

- Generally, I would say that the results for H, LE and SWC don't bring a huge amount to the paper. And large portions could be condensed or moved to supplementary material. But it certainly **does not** detract from the paper. However, it somewhat distracts the reader from what I would consider very good results regarding posterior COS fluxes and GPP. This is meant as an opinion and how to deal with this can be left to the discretion of the authors.

Response: Thank you for your valuable comments. We agree with your statement that these variables are not as well optimized as GPP, and that the presentation of these results distracts the readers. We have therefore removed this section.

- There seems to be interchangeable use of “COS flux data”, “COS flux measurements”, “COS data” and “COS measurements”. There is a big difference between a measurement of COS fluxes and ambient COS concentration for example. A good example is the first sentence of section 2.5.2: “After the ability of NUCAS to assimilate COS flux data was confirmed by twin experiments, we could then use the system was then utilised to conduct data assimilation experiments with real COS observations under single-site and multi-site conditions” – Is “real COS observations” referring to measurements of the flux? Please be diligent in distinguishing this throughout the paper. From what I can tell, the only use of ambient COS concentrations is to drive the estimation of prior COS fluxes.

Response: Thank you for your detailed comments. We acknowledge that the there is a big difference between a measurement of COS fluxes and ambient COS concentration.

However, since these flux data are actually derived from concentrations, we have difficulty in describing them specifically in some places. However, we did make changes in this area to provide a more consistent description, such as changing “COS flux data” to “COS fluxes”.

“real COS observations” does refer to measurements of the flux. The reason for this designation is to distinguish it from the “pseudo-observations” of the twin experiment.

As you said, the only use of ambient COS concentrations is to drive the estimation of COS plant uptake.

Some sentences are unclear. I have highlighted potential improvements in the readability of these in the Minor Comments. Please read them carefully to ensure there has not been a misinterpretation.

Response: Thank you for your detailed comments. Your revisions have greatly improved the readability of this manuscript, for which we are deeply grateful. However, for a very few cases, we have not made the changes exactly as you suggested. We have explained this point by point, please see below for more details.

Minor Comments:

- L22-24: improve readability. Example: “Data assimilation experiments were conducted to investigate the robustness of NUCAS, and to test the feasibility and applicability of assimilating carbonyl sulfide (COS) fluxes from seven surface sites, in order to better our understanding of stomatal conductance and photosynthesis.”

Response. Thank you for your valuable comment. We have carefully reviewed the manuscript and made revisions to improve its readability.

- L26: I assume you mean COS fluxes: “assimilation of COS fluxes can”.

Response: Thanks for your comment. As you mentioned, we assimilated COS fluxes in this study, therefore we have modified it accordingly.

- L28: Be consistent in referring to NUCAS as “the NUCAS” or simply “NUCAS”. Also see note for Line 73.

Response: Thank you for your comment. Now, we have referred to NUCAS consistently as “NUCAS”.

- L28: “to the” -> “with”

Response: Thanks for your comment. We have modified the sentence accordingly.

- L36: “of earth system” -> “of the Earth system”

Response: Corrected.

- L37: the biosphere ‘significantly mitigating climate change’ is a bit speculative in my opinion. It has certainly reduced the full potential of climatic changes since 1850. But the reasoning is

partly through feedbacks, such as longer growing seasons in the Northern Hemisphere, as a result of climate change. I would just end the sentence at 1850 (and of course keep the reference).

Response: Thank you for your valuable comment. We have modified the sentence accordingly.

- L38-39: “of terrestrial biosphere have changed” -> “of the terrestrial biosphere has changed”.

Response: Corrected.

- L41: “important tool to investigate” -> “important tool used to investigate”.

Response: Corrected.

- L44: “data” -> “datasets”

Response: Corrected.

- L45: The reference the Scholze et al. (2017) feels a bit random. Be more specific about the ‘various observations’ you’re referring to at the beginning of the sentence or just remove the reference. I would just start the sentence: “Observations such as sun-induced..”

Response: Thanks for your detailed comment. This is because Scholze et al. (2017) is a review article on datasets used in terrestrial carbon cycle data assimilation. We believe it would be better to keep this article because it would facilitate readers to learn more about the earth observations used in terrestrial carbon cycle data assimilation. According to your suggestion, we have modified the sentence: **“Observations such as sun-induced chlorophyll fluorescence (Schimel et al., 2015) and soil moisture (Wu et al., 2018), have been used to estimate or constrain carbon fluxes in terrestrial ecosystems (Scholze et al., 2017).”** (Line 47-49)

- L47: Not sure this is a ‘recent’ finding any more. Remove.

Response: Thanks for your comment. We have removed “Recently” accordingly.

- L52-54: Move these two sentences (starting ‘Plants’ and ‘As’ to the paragraph above), as they are still discussing the relationship between CO₂ and COS. The sentence afterwards moves into discussing COS and GPP modelling.

Response: Thanks for your comment. In fact, as you say, these two sentences are still discussing COS and CO₂. but our idea is to have a second paragraph that gives a preliminary introduction to terrestrial ecosystem model and the datasets used to constrain the model, and lead into COS. In the third paragraph, COS is then described specifically, such as the role of COS in indicating not only GPP but also stomatal conductance and transpiration, the development of COS modeling, COS observations, etc.

- L58: What other ‘key ecosystem variables’ are you referring to? Are there any others that COS can estimate better than direct measurement? The reason it’s useful to estimate GPP is because it is impossible to measure GPP directly at large spatial scales. Perhaps be specific about what you are referring to, the reader might be interested.

Response: Thanks for your comment. As you mentioned, GPP cannot be directly measured due to the hindrance of respiration. Similarly, the measurements of transpiration are confounded by

evaporation. Due to the coupling of leaf exchange of COS, CO₂ and water vapor through stomata, COS exchange measurement can provide independent and direct way for estimating transpiration and stomatal conductance. Currently, some studies have combined COS models with COS observations to estimate not only GPP but also stomatal conductance and transpiration (Wohlfahrt et al., 2012; Wehr et al., 2017; Sun et al., 2022). Therefore, we have modified the sentences to be more specific: **“A number of empirical or mechanistic COS plant uptake models (Campbell et al., 2008; Wohlfahrt et al., 2012; Berry et al., 2013) and soil exchange models (Kesselmeier et al., 1999; Berry et al., 2013; Launois et al., 2015; Sun et al., 2015; Whelan et al., 2016; Ogée et al., 2016; Whelan et al., 2022) have been developed to simulate COS fluxes in order to more accurately estimate gross primary productivity (GPP), stomatal conductance as well as transpiration.”** (Line 56-60)

- L58: “However, with the lack of” -> “However, due to the lack of”.

Response: Corrected.

- L58-61: Move the references to Wohlfahrt et al. (2012) and Kooijmans et al. (2021) to the end of the sentence. Remove Bruhl et al. (2012), I don't understand the context of that reference here.

Response: Thanks for your comment. We have relocated the references of Wohlfahrt et al. (2012) and Kooijmans et al. (2021) accordingly. As done in Wohlfahrt et al. (2012), Brühl et al. (2012) was cited here for arguing that there is a lack of ecosystem-scale field COS measurements.

- L62: “behavior” -> “behaviour”.

Response: Corrected.

- L65: remove “various”.

Response: Corrected.

- L68-71: I think it's clear you're referring to data assimilation techniques. This sentence could be reduced and read better, example: “More specifically, the observed dynamics of ecosystems can be more accurately portrayed, additionally, our understanding of ecosystem processes can be deepened, with respect to their responses to climatic changes.”

Response: Thanks for your valuable comment. We think it is important to mention “process-based model” here. Thus, we have revised the sentences as: **“More specifically, by applying data assimilation methods to process-based models, not only can the observed dynamics of ecosystems be more accurately portrayed, but also our understanding of ecosystem processes can be deepened, with respect to their responses to climate changes (Luo et al., 2011; Keenan et al., 2012; Niu et al., 2014).”** (Line 70-73)

- L73: Include a note that NUCAS v1.0 will be referred to as NUCAS for the remainder of the paper: “NUCAS v1.0, hereafter referred to as NUCAS, is designed”

Response: Thank you for your valuable comment. We have modified the sentence accordingly.

- L73-74: Improved punctuation. “to assimilate multiple observational data streams including COS flux data to improve the process based” -> “to assimilate multiple observational data

streams, including COS flux data, to improve the process-based”.

Response: Thank you for your detailed comment. We have modified the sentence accordingly, and revisited the manuscript to improve punctuation.

- L77-78: An example of non-line-breaking hyphen. This will be the only in-text mention I refer to. **Please see Technical Note on this.**

Response: Response: Thank you for your detailed comment and the useful notes. We have thoroughly examined the use of non-line-breaking hyphen and made modifications.

- L82: Clarify “COS fluxes”

Response: Thanks for your comment. We have revised the sentence accordingly.

- L85: Again, are you referring to COS observations or COS flux observations?

Response: Thank you. Here we want to refer to COS flux observations, for which we have modified accordingly.

- L88: Combine and clean these sentences: “Materials and methods used in our study are described in Sect. 2, such as the BEPS model and NUCAS, are introduced”.

Response: Thanks for your valuable comment. We have revised the sentence accordingly.

- L89: What data do you mean? Just be a bit more specific: “along with the data used to drive BEPS and assimilated into NUCAS.”

Response: Thanks for your valuable comment. We have revised the sentence accordingly.

- L104: “if” -> “is” and “First” -> “first”.

Response: Thanks for your detail comment. We've corrected the typos.

- L119: Remove BFGS acronym definition. It isn't used again.

Response: Thanks for your detail comment. We have removed it accordingly.

- L128-132: Keep this paragraph with the previous one. The discussion is still relevant, i.e. regarding cost function.

Response: Thank you. We have made the changes you suggested.

- L140: Change “new” to “updated”. I wouldn't refer to literature written in 1999 as new.

Response: Corrected.

- L153-155: Be more specific about what is being referenced here: “The canopy-level COS plant uptake $F_{cos,plant}$ ($\text{pmol m}^{-2}\text{s}^{-1}$) was calculated by upscaling the resistance analog model of COS uptake, as presented by Berry et al. (2013), with the upscaling scheme recommended by Chen et al. (1999).”

Response: Thanks for your valuable comment. We have revised the sentence accordingly.

- L161: No need to end the sentence. Also include another reference to Berry et al. (2013), as

you are directly quoting the calculation: “where $COSa$ is the COS mole fraction in the bulk air and g_{sw} and g_{bw} are the stomatal conductance and leaf laminar boundary layer conductance to water vapor (H_2O), respectively (Berry et al., 2013).”

Response: Thank you for your valuable comment. We have revised the sentence accordingly.

- L164-165: Try to avoid starting a sentence with a lower case, even if it is a relevant parameter. Perhaps: “The apparent conductance for COS uptake from the intercellular airspaces is denoted by g_{cos} and combines the mesophyll conductance and the biochemical reaction rate of COS and carbonic anhydrase (CA).”

Response: Thanks for your detailed comment. We have revised the sentence accordingly.

- L166-169: Move the references to the end and lead the sentence into equation 4. Perhaps: “rate of Rubisco at 25°C (Badger and Price, 1994; Evans et al., 1994), such that: Eq. (4) where α is a scaling parameter that is calibrated”

Response: Corrected.

- L170: These values of α need a reference. If it’s from Stimler et al. (2012), then move the reference in the previous sentence to here.

Response: Thank you. We have added a reference of the values of α (Haynes et al., 2020).

- L170-171: “With reference to the COS modelling scheme: Simple” -> “According to the COS modelling scheme: Simple”

Response: Corrected.

- L178: End this sentence with : rather than . As you are referring to Eq. 7 anyway.

Response: Thanks for your detailed comment. We have modified the punctuation accordingly.

- L180: Same as above. : rather than .

Response: Corrected.

- L182: Now start sentence with where, instead of Where.

Response: Corrected.

- L187: “Here” -> “In Eq. 10,” Also it is not clear what a is used for. Maybe add some context?

Response: Thanks for your comment. According to Whelan et al. (2016) and Whelan et al. (2022), the model assumes that COS soil biotic uptake changes with SWC, and there exists an optimum SWC (SWC_{opt}) at which the simulated biotic COS flux is maximized, i.e. optimum in terms of COS soil biotic uptake (F_{opt}). Base on SWC_{opt} , F_{opt} , and COS flux (F_g) under another soil moisture condition (SWC_g , and $SWC_g > SWC_{opt}$), it is thus possible to determine the unique shape parameter a for which the model varies with SWC and, in turn, to calculate soil biotic COS fluxes ($F_{COS,biotic}$) for any SWC condition. In summary, parameter a is a curve shape constant of the empirical model, which can be determined by SWC and the corresponding COS abiotic flux observations.

- L187: “maximum” -> “optimal”.

Response: Corrected.

- L190: “reference of” -> “reference to”

Response: Corrected.

- L191: This is the first use of SWC as an abbreviation, so define it, i.e. soil water content (SWC).

Response: Thank for your comment. We have revised the sentence to include a definition of SWC accordingly.

- L195: use the variable names, “COS plant uptake” -> “Fcos,plant” and COS soil fluxes -> “Fcos,soil”. You can probably just remove this sentence to be honest. But up to the author.

Response: Thanks for your comment. This sentence is not necessary and we have deleted it.

- L198: “tuned previous model in development” -> “tuned in past model development”

Response: Corrected.

- L200: Specifically refer to the research: “The prior uncertainty of parameters is set based on previous studies by Chen et al. (2022) and Ryu et al. (2018).”

Response: Thank you for your valuable comment. Due to our current lack of understanding of model parameters, and the fact that not all parameter uncertainties have available references (or perhaps we have not found them), we have made the following modifications to the sentence: **“The prior uncertainty of parameters is set based on previous research, i.e., Ryu et al. (2018) and Chen et al. (2022).”**(Line 204-205) In addition, we have also explained this in section 4.5: **“Furthermore, there is a lack of understanding of the prior uncertainty for certain model parameters, such as VJ_slope, which makes the uncertainty estimates subject to potentially large errors.”** (Line 640-641)

- L222: “was” -> “were”

Response: Corrected.

- L223-224: Sentence about LAI, meteorology and soil datasets need a reference. Or as they were mentioned in 2.1.2, perhaps direct the reader there.

Response: Thanks for your comment. We have now added the necessary references here, i.e., Liu et al. (1997) and Chen et al. (1999).

- L226: “soil water content (SWC) at these sites collected at the sites were used” -> “SWC collected at the sites were used”.

Response: Corrected.

- L231: “LAI product represents Lead area index at a” -> “LAI product quantifies leaf area index”.

Response: Corrected.

- L231-233: spatial resolution is traditionally presented as 8×8 km for example. As it is a 2-D shape. Please clarify.

Response: Corrected.

- L235: You may as well be specific in the products you are referring to. “The other two LAI products were used to investigate the effect of the LAI products” -> “The GLASS and MODIS LAI products were used to investigate the effect of different LAI products”

Response: Thanks for your detailed comment. We have revised the sentence accordingly.

- L238-239: This last sentence is a bit wordy, but I understand what is to be communicated. Perhaps: “In addition, the 8-day temporal resolution of the LAI data was interpolated into daily values using the nearest neighbour method.”

Response: Thanks for your understanding and suggestion. We have revised the sentence accordingly.

- L241-245: These few sentences could be a bit neater and consistent. Perhaps: “Standard hourly meteorological data was inputted in BEPS, including air temperature at 2 m, shortwave radiation, precipitation, relative humidity and wind speed, taken from the FLUXNET database (for sites: AT-Neu, DK-Sor, ES-Lma, FI-Hyy and US-Ha1 see <https://fluxnet.org>), the AmeriFlux database (for sites: US-Ha1, US-Wrc, see <https://ameriflux.lbl.gov>) and the ERA5 dataset (for sites: AT-Neu, IT-Soy, US-Ha1 see <https://cds.climate.copernicus.eu/cdsapp#!/dataset/reanalysis-era5singlelevels?tab=overview>).”

- L247: Move the reference to just before the comma. Also remove “Particularly” at the start of the next sentence.

Response: Corrected.

- L257-258: I think the eddy-covariance and gradient-based approach both require a reference.

Response: Thanks for your comment. We have now added the necessary references here, i.e., Baldocchi (2003), Kohonen et al. (2020) and Wu et al. (2015).

- 258-259: “The COS soil measurements were collected using soil chamber, except at US-Ha1, where a sub-canopy flux-gradient approach was used to calculate the soil COS flux.” -> “The COS soil flux measurements were collected using soil chambers, except at US-Ha1, where a sub-canopy flux-gradient approach was used.”

Response: Thanks for your comment. We have revised the sentence accordingly.

- L260: “COS flux measurements”

Response: Corrected.

- L262-264: The first sentence of this paragraph needs to be reworded. There is very little context, and the ordering is confusing. Also, what is the aerodynamic gradient method? IS this the same as the gradient-based approach? Perhaps: “US-Wrc utilises the gradient-based approach to measure COS ecosystem flux (reference), however available data is limited to only COS concentration measurements and lacking other parameters required, therefore this site

risks introducing biases.” Or similar.

Response: Thanks for your comment. Following the description of COS flux measurement in Rastogi et al. (2018), “the aerodynamic gradient method” were utilized here. However, we do agree with you that such a description may confuse readers. Therefore, we revised the sentence as you suggested: **“US-Wrc utilises the gradient-based approach to measure COS ecosystem flux (Rastogi et al., 2018), however available data is limited to only COS concentration measurements and lacking other parameters required, therefore this site risks introducing biases”** (Line 267-269)

- L268-273: summary of Equation 11 could be much more concise. Perhaps: “This was done using the mean, \bar{M} , and standard deviation, σm , of the simulated COS flux to correct the COS flux observations (O):

Eq. 11”

where \bar{O} and σO are mean and standard deviation of the observed COS flux series. F is the corrected observed COS flux and the COS simulations were calculated using the prior parameters for the time period corresponding to the COS flux observations.

Response: Thanks for your comment. We have revised the sentences accordingly.

- L273: No need to start new paragraph. Move up to previous.

Response: Thank you for your comment. We have relocated the sentences accordingly.

- L273: “as an estimate”

Response: Corrected.

- L276: “LE data are selected” -> “LE, data were selected”

Response: Corrected.

- L277: This needs a reference.

Response: Thanks for your comment. Now, we have added the necessary references to this sentence, i.e., Wohlfahrt et al. (2012) and Whelan et al. (2018).

- L283: Move this reference to the end of the sentence.

Response: Corrected.

- L306-307: “With reference” -> “Regarding”

Response: Corrected.

- L309: “After the ability of NUCAS to assimilate COS flux data was confirmed by twin experiments, we could then use the system” -> “After the ability of NUCAS to assimilate COS flux data was confirmed by twin experiments, the system was then utilised”

Response: Corrected.

- L312: ~~of these~~

Response: Corrected.

- L317-318: put comma (,) after experiment and simultaneously.

Response: Done.

- L325: “in twin experiment” -> “in a twin experiment”.

Response: Corrected.

- L326: was calculated “using Eq. 12”.

Response: Corrected.

- L328: “where” -> “Where”.

Response: Thanks for your comment. As you mentioned before, it would be more appropriate to use “where” as the start of a sentence. Therefore, we reserve the use of “where” here.

- L352: I only see D_{final} in Table S5.

Response: Thanks for your comment. Due to an identical perturbation size (0.2) being applied in all twin experiments, the initial value ($D_{initial}$) of D_x is constant, with a value of 7.48. Thus, we describe $D_{initial}$ in the title of the table.

- L355: “nearly zero with the maximum value below” -> “nearly zero, where the maximum value was below”.

Response: Corrected.

- L356: pseudo-observations

Response: Thanks for your detailed comment. We have corrected this typo.

- L358: ~~results during the assimilation process.~~

Response: Corrected.

- L364: ~~4.87%~~ I read this as 6.35% in Table 2?

Response: Thanks for your detailed comment. We have corrected this error.

- L365: “with the cost function reduction of 16.39% and 15.70%.” -> “~~with the cost function reduction~~ of 16.39% and 15.70% respectively.”

Response: Corrected.

- L366: Include the percentage values for these 2 sites: “FI-Hyy (21.47%)” and “US-Wrc (27.71%)”.

Response: Thanks for your detailed comment. We have revised the sentences accordingly.

- L367-368: I believe you mean July of 2015. This sentence needs to be reworded, perhaps: “In August 2014 and July 2015, the cost function reduction was between 40.59 % and 50.94 %, while in July of all other years, the cost function reduction was much lower, ranging from 4.87% to 18.94 %.”

Response: Thanks for your comment. Actually, we want to refer to July 2014 and August 2014 here. According to your suggestion, we have revised the sentence: **“In July 2014 and August 2014, the cost function reductions were 20.17% and 38.86% respectively, while in July of all other years, the cost function reductions were are much lower, ranging from 2.84 % to 5.88 %.”** (Line 379-381)

- L371-372: This is a bit of a throwaway sentence. If prior simulations were that good, we wouldn't need an inversion scheme right. I don't think sentence is necessary and almost devalues the posterior results.

Response: Thanks for your valuable comment. We have removed the sentence as you suggested.

- L377: I found this to be a particularly interesting finding. In that the way the COS fluxes are being calculated in the posterior are clearly missing one or more processes to exactly replicate measurements. Certainly something to investigate in future research and perhaps highlight in your conclusions.

Response: Thanks for your valuable comment. We believe there are two main reasons for the ineffective simulation of nighttime COS fluxes, and we have already covered this in the discussion: **“On the one hand, this is due to the substantial gap between current modelled COS soil fluxes and observations (Whelan et al., 2022). On the other hand, this also stems from the fact that the nighttime stomatal conductance was set to a low and constant value (1 mmol m⁻² s⁻¹) in the BEPS model”** (Line 516-518). Of course, we agree with your point that this should be investigated in future research.

- L382: You can remove the sentence starting “Similar to”. I believe you raised this in the previous sentence.

Response: Thanks for your valuable comment. We have re-reviewed the article and corrected this.

- L384: 6.94 ~~pmol m⁻² s⁻¹~~ to 3.09 pmol m⁻² s⁻¹

Response: Corrected.

- L387: missed at: nighttime.

Response: Thank you for your detailed comment. We have corrected this typo.

- L390: “FI-Hyy and US-Wrc have different soil textures, with sandy loam and loam, respectively.” -> “FI-Hyy and US-Wrc have different soil textures; sandy loam and loam respectively.”

Response: Corrected.

- L391: “took this difference into account and” -> “accounted for this difference appropriately and”.

Response: Corrected.

- L392: I calculate this to be 26.28% but perhaps I have mixed up numbers. Please check.

Response: Thanks for your detailed comment. We have mixed up numbers, and we have corrected this error.

- L408: If the two-site assimilation method achieved similar results to the single-site, why do we need the two-site? Was it more of proof-of-concept? A sentence summarising why it was useful would be helpful.

Response: Thanks for your valuable comment. A detailed description of the purpose of conducting the two-site experiment has been provided in the conclusions, i.e., “Our two-site setup constitutes a challenge for the assimilation system, the model and the observations. In this setup, the assimilation system has to determine a parameter set that achieves a fit to the observations at both sites, and NUCAS passes this important test. It should be noted that NUCAS was designed as a platform that integrates multiple data streams to provide a consistent map of the terrestrial carbon cycle although only ecosystem COS flux data were used to evaluate the performance of NUCAS in this study. The “two-site” assimilation experiment conducted in this study gives us more confidence that the calibrated model will provide a reasonable parameter set and posterior simulation throughout the plant functional type. In other words, what we present here is a pre-requisite for applying the model and assimilation system at regional to global scales.”

- L410: regarding ‘as mentioned before’, it looks like this material has been moved to the appendix. Please check and update accordingly.

Response: Thanks for your detailed comment. We have checked and updated this.

- Section 3.4: It isn’t really clear if a positive or negative change is a good thing. Especially as the majority of the summary refers to absolute differences. Could this be elaborated on?

Response: Thank you for your suggestions. In this section, we mainly want to characterize the actual results of parameter changes. Whether the results of parameter tuning are good or bad (as I understand it in terms of improving the model simulation) needs to be judged subsequently by performing an evaluation of the simulation results. Since the parameter changes are relative to the prior values, absolute averages are used here to better reflect the magnitude of fluctuations in the parameter changes.

- Line 419: 45.09% surely this value is hugely skewed by ES-Lma? What is the value excluding this site?

Response: Thank you for your comment. With the exception of ES-Lma, the mean absolute change of V_{cmax25} is 34.94%.

- L425-427: my interpretation of IT-Soy is that on paper the RMSE is ok and improved in the posterior due to improvements during the daytime. However, it’s minimal change from the prior suggests it is not particularly sensitive to assimilation of COS flux data.

Response: Thank you for your comment. Since the prior simulations and observations at IT-Soy are already very close, the assimilation effect of the COS flux at this site is limited and the cost function reduction is small.

- L430: remove capital T.

Response: Corrected.

- L432: include at the end of this sentence “(note the difference in x-axis scales)”. By eye it is misleading initially.

Response: Thank you for your valuable comment. We have revised the sentence accordingly.

- L435-438: These values are very different. Is it appropriate to be comparing variables like-forlike in this way? A bit more explanation of the implications of the results would be helpful.

Response: Thank you for your valuable comment. Here we have calculated and compared the sensitivity indexes of different parameters using concomitant-based sensitivity analysis. The sensitivity indexes of the different parameters varied significantly, indicating that the impacts of these parameters on the ecosystem COS flux simulation and the cost function were significantly different. A more detailed explanation of the implications are presented in Section 4.2.

- L442: Unless you have specifically excluded DK-Sor, I would remove this.

Response: Thank you for your valuable comment. This is because the sensitivity of VJ_slope at DK-Sor was only 12.05%, which is notably smaller than at the other sites. We have revised the sentence in accordance with your comments: **“With the SIs ranging from 12.05% to 45.71% and 0.94% to 14.43%, VJ_slope and f_leaf also play important roles in the modelling of COS.”** (Line 453-454)

- L444-445: [at end of sentence] respectively.

Response: Corrected.

- L447: Maybe this is clearer: “Our results also suggest that f_leaf tends to play a more important role in the COS assimilation at the forest sites (DK-Sor, FI-Hyy, US-Ha1 and US-Wrc) compared to the low-stature vegetation type sites (AT-Neu, ES-Lma and IT-Soy), with the mean absolute SIs about two times than that of the latter, with the exception of DK-Sor.” Optional.

Response: Thank you for your valuable comment. We have revised the sentence as follows: **“Our results also suggest that f_leaf tends to play a more important role in the COS assimilation at the forest sites (except DK-Sor, including FI-Hyy, US-Ha1 and US-Wrc) compared to the low-stature vegetation type sites (AT-Neu, ES-Lma and IT-Soy), with the mean absolute SIs about two times than that of the latter.”** (Line 459-461)

- L445: Does a lower R2 value not suggest that the assimilation has worsened the result? Also do you mean Figure S3?

Response: Thank you for your valuable comment. In terms of R², the R² between the posterior simulated GPP and the observed GPP is indeed a bit lower than that of the prior. However, we believe this is acceptable because the cost function itself considers the difference between the model and the observation, not the correlation. And, we do mean Figure S3 here, as we labeled the scatterplot of GPP with R².

- Section 3.6: Lots of plots being referred to. Include ‘see Figure 6c’ etc.. where necessary. Help

the reader.

Response: Thank you for your kindly comment. Now, we have given more detailed instructions on this.

- L465: I calculate the 3.81% to be 8.61%. Please check.

Response: Thank you for your comment. We have checked and confirmed the results are correct. For ES-Lma, the reason that you calculated a result of RMSE reduction of 8.61% and we present a result of 8.60%, is because we only present the RMSE with two decimals reserved.

- L471: GPP? Not COS.

Response: Thank you for your comment. The changes in the GPP simulation are caused by the assimilation of COS. Therefore, we present here the posterior simulation results for both COS and GPP. In order to improve the readability of the manuscript, we have added clarifications to the sentence: “In parallel, the model-observation difference **of GPP** also reduced, by 12.36% and 28.10%, respectively.” (Line 483-484)

- L472: Drop line after ‘underestimated.’

Response: Corrected.

- L484: struggling to get where these 2 percentages have come from.

Response: Thank you for your comment. These two percentages refer to the difference between the RMSE reductions of GPP in the two-site experiment and the RMSE reductions of GPP in the single-site experiments.

- L495: Please refer to figures S4-S7 as they are discussed. Rather than just listing them at the start of a section. Also as a note, if you are having to discuss and refer to figures in supplementary material, it’s probably a sign that you are trying to present too much. As mentioned earlier, you could probably remove sections 3.7 and 3.8.

Response: Thank you for your valuable comment. We have removed the section in accordance with your comment.

- L551: Requires more references, bottom-up or top-down. Kooijmans et al. (2021), Ma et al.(2021), Maignon et al. (2021) and Remaud et al. (2022). For example.

Response: Thank you for your comment and the references. We have added more references to the sentence, including those you have listed. The revised sentence reads as: “**This is because COS plant fluxes are much larger than COS fluxes of soil in general (Whelan et al., 2016; Whelan et al., 2018; Spielmann et al., 2019; Kooijmans et al., 2021; Ma et al., 2021; Maignan et al., 2021; Remaud et al., 2022) and the soil hydrology-related parameters cannot directly influence the COS plant uptake.**” (Line 508-511)

- L567-569: “COS plant uptake is governed by the hydrolysis reaction of COS (Wohlfahrt et al.,2012), catalysed by CA, though it can also be degraded by other photosynthetic enzymes, e.g., RuBisCo (Lorimer and Pierce, 1989), and the reaction is not dependent on light (Stimler et al., 2011; Whelan et al., 2018).” I think reads a bit better. Optional.

Response: Thanks for your comment. We have revised the sentences accordingly.

- L586: Proven.

Response: Corrected.

- L593: CA, not carbonic anhydrase.

Response: Corrected.

- L595: “capable to influence” -> “capable of influencing”

Response: Corrected.

- L599: “sensitivity of Vcmax25” -> “sensitivity in Vcmax25”

Response: Corrected.

- L608-611: It’s not clear if you’re saying your work also found this. Please clarify and amend accordingly.

Response: Thank you for your valuable comment. In the sensitivity analyses conducted in this study, we examined only the sensitivity of the cost function to the model parameters and did not examine the sensitivity of photosynthetic capacity-related variables (e.g., GPP) to the model parameters. So, we can’t get to that conclusion. We thus revised the sentence to avoid misunderstanding, the revised sentence reads as: **“Similar findings by Sun et al. (2019) found that the simulated GPP was more sensitive to radiation at forested vegetation types and less sensitive at low-stature vegetation types.”** (Line 566-568)

- L623-624: “In comparison, the RMSEs of GPP simulations were reduced by an average of 25.37% within the assimilation of COS, while that of LE were reduced by 16.27 %.” -> “In comparison, the RMSEs of GPP simulations were reduced by an average of 25.37 % as a result of assimilating COS, but reducing LE by only 16.27 %.”

Response: Thank you for your valuable comment. We have modified the sentences accordingly.

- L630: at the end of sentence: “via evapotranspiration”.

Response: Corrected.

- L635: behaviour

Response: Corrected.

- L639: ‘remarkable differences’ is an odd phrase. Be specific. Also if you mean large, this is a different narrative to Section 3.

Response: Thank you for your valuable comment. We have revised the sentence, as: “However, our results also show that there are **obvious** discrepancies between the ecosystem COS flux simulations and observations, and that discrepancies cannot be effectively reduced by the adjustment by the photosynthesis related parameters due to the simplification of BEPS for nighttime stomatal conductance modelling” (Line 596-599). Moreover, we have also deleted the sentence in section 3 you mentioned: ~~“Results show that the prior simulations can~~

~~accurately reflect the magnitude of ecosystem COS fluxes and effectively capture the daily variation and the diurnal cycle of COS.”~~

- L641: BEPS

Response: Corrected.

- L679: move reference to end of sentence.

Response: Done.

- Some of the discussion in Section 4.5 is a bit wordy (mainly last paragraph). Below are a few instances of trying to improve readability and flow.

- Final paragraph, sentence 2: “As the nighttime COS plant uptake is driven by stomatal conductance (Kooijmans et al., 2021), g_n , nighttime COS fluxes can therefore be used to test the capability of BEPS to model g_n .”

Response: Thank you for your comment. In this manuscript, g_n refers specifically to the **nighttime** stomatal conductance, which is slightly different with the stomatal conductance (g_s). Therefore, we retained the original sentence, but deleted “the” in response to your comment: “As the nighttime COS plant uptake is driven by stomatal conductance (Kooijmans et al., 2021), ~~the~~ nighttime COS fluxes can therefore be used to test the accuracy of the model settings for nighttime stomatal conductance (g_n).” (Line 652-654)

- L699: space between 1 and mm.

Response: Corrected.

- Final paragraph sentence 5: “Similar findings by Resco De Dios et al. (2019), showed that the median g_n in the global dataset was $40 \text{ mmol m}^{-2} \text{ s}^{-1}$.”

Response: Corrected.

- Final paragraph sentence 8: “As different enzymes have different physiological characteristics, Cho et al. (2023) proposed a new temperature function for the CA enzyme and showcased the considerable difference in temperature response of enzymatic activities of CA and RuBisCo, which provided valuable insights into the modelling and assimilation of COS.”

Response: Corrected.

- L707: CA

Response: Corrected.

- L709: N = nitrogen?

Response: Thanks for your comment. We have replaced “N” with “nitrogen”.

- L710: in -> by

Response: Corrected.

- Final paragraph final sentence: “Therefore, using the global microbial C biomass, soil N content and MAP datasets, the relationships between these variables, and the associated COS exchange processes, it is to be expected that a more accurate modelling of terrestrial ecosystem COS fluxes could be achieved, further increasing our understanding of the global COS budget and facilitate the assimilation of COS fluxes.”

Response: Thanks for your valuable comment. We have revised the sentence accordingly.

- L724-726: Perhaps: “Fourteen twin experiments, thirteen single-site experiments and one two-site experiment covering the period from 2012 to 2017, were conducted to investigate the capability of NUCAS to assimilate COS fluxes and optimize output parameters and variables. COS flux observations from a range of ecosystems were used, including four PFTs and three soil textures.”

Response: Thanks for your comment. We have revised the sentence accordingly.

- L729: COS fluxes

Response: Corrected.

- L749: “throughout the plant function type” -> “for different PFTs”.

Response: Thank you for your valuable comment. But for the convenience of our readers, especially those who don't have the time to read the whole manuscript, here we prefer to keep the original sentences without abbreviations.

- L753-754: “However, the ‘equifinality’ can be avoided by imposing additional observational constraints (Beven, 2006).” Such as? i .e., ‘in this instance, we refer to the calculation and assimilation of multiple datasets, other than just COS fluxes.’

Response: Thanks for your comment. The assimilation of multiple datasets, other than just COS fluxes, is just what we want to express.

Technical Notes:

- Be consistent with the following phrases single-site, two-site, AmeriFlux, process-based, etc..in terms of the use of hyphen and capitalisation. Please check other potential sources of inconsistency.

Response: Thanks for your valuable comment. We have carefully rechecked the manuscript to avoid inconsistencies terms of the use of hyphen and capitalisation.

- Use non-line-breaking hyphen where possible. This way be resolved in the editing by EGU, but for future reference to avoid a hyphenated phrase, or unit, breaking a line, use ctrl+shit+-.

Response: Thank you for this extremely detailed technical note. We have changed the line-breaking hyphen as you suggested.

- % symbols should be immediately adjacent to values, not with a space

Response: Thank you for your comment. We have corrected the use of “%” and spaces.

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