

Uncertainty in Land Carbon Fluxes Simulated by CMIP6 Models from Treatments of Crop Distributions and Photosynthetic Pathways.

Joseph Ovwemuvwose¹, I. Colin Prentice^{2,3}, and Heather. D. Graven¹

¹Department of Physics, Imperial College London, London, UK.

²Georgina Mace Centre for the Living Planet, Department of Life Sciences, Imperial College London, London, UK

³Department of Earth System Science, Tsinghua University, Beijing, China

Correspondence to: Joseph Ovwemuvwose (j.owwemuvwose22@imperial.ac.uk & jsovw@gmail.com)

We are grateful to the referees for their comments and suggestions, which we have used to improve the manuscript. Below we describe the modifications we have made to the manuscript in response to the comments.

Responses to Referees

Referee Major Comments and Responses

Comment 1

Core assumptions about C₃/C₄ GPP and Cveg. CMIP6 models do not directly report GPP or Cveg partitioned into C₃ and C₄ components. The authors therefore estimated C₃ and C₄ values by multiplying total GPP or Cveg by the areal fraction of C₃ or C₄ vegetation. As they themselves noted, this is problematic: on average, GPP per unit area is higher for C₄ than for C₃ vegetation, whereas Cveg per unit area is likely greater for C₃ plants (often woody) than for predominantly herbaceous C₄ plants. These relationships also vary geographically, shaped by climate and soil. Consequently, it is difficult to place much confidence in downstream analyses of C₃/C₄ GPP, Cveg, and their trends.

Response

We accept this point. We state that ‘In Luo et al. (2024) it was found that the per unit area photosynthetic rate of C₄ grass was generally higher than for C₃ vegetation, so these calculations may overestimate the proportion assigned to C₃ vegetation.’ However, we can only make use of the variables that were made available by the modelling groups. We have applied a particular analytical approach in a consistent manner to all the models, so our findings do indeed reveal inconsistencies among the models.

Moreover, in the absence of full information, our approach should provide a reasonable approximation – because in pixels where plants with both pathways co-occur, the differences in GPP between C₃ and C₄ components will generally not be large due to the extra cost invested by C₄ vegetation in the CO₂ concentrating process (Still et al, 2000, Ehleringer and Bjorkman 1976, and Ehleringer 1978). In forested and other locations dominated by woody C₃ vegetations, the biomass per unit area will be larger for C₃ but in locations that are mosaics of both C₃ and C₄ grasses, there will be a small difference in the annual relative biomass of C₃ and C₄ since the relative distribution of their biomass is controlled by the seasonal availability of water (Winslow et al 2003). Nonetheless, it is unfortunate that the modelling groups did not provide enough information for us to do a more exact analysis. We suggest that the GPP and Cveg of C₃ and C₄ vegetation should be explicitly reported in future.

We agree that C₃/C₄ competition varies geographically and is shaped by climate and soil properties. However, the objection raised here applies only to the approximate calculations we made within grid cells. We can reasonably expect the models to incorporate environmental and land-use controls on this competition appropriately, in order to represent geographic variations among pixels. The large inconsistencies we see suggest that models need to address this issue more rigorously.

A similar concern applies to the treatment of C₄ crops. Several CMIP6 models lack explicit fractions for C₄ crops versus natural C₄ vegetation, so the authors assume C₄ crop fraction = C₄ fraction × total crop fraction. This implies that C₃/C₄ ratios for crops mirror those of natural vegetation at each grid cell—an unlikely scenario. Open data (e.g., Luo et al., 2024) show no such pixel-level pattern.

Response

Please see our response to Referee 1, Comment 2, on this issue. We have compared our approach to LUH and found that they are consistent.

The LUH C4-crop fraction itself is also problematic: in LUH c4 crop fraction remains static (based on ~2000 data), so the apparent change in C4 crop area likely reflects total cropland expansion or contraction. It is unclear how individual CMIP models treat this issue, and I would not regard the LUH C4 crop trend as an “observation,” nor use it to declare a model (e.g., UKESM1) incorrect (L155, L328).

Response

Thank you for this comment. We have added a statement about the LUH C4 crop fraction remaining static, and revised the text so as *not* to imply that LUH is an observation that could invalidate a model.

If the aim is a robust model–data comparison of C3/C4 fractions in area, GPP, and Cveg, I suggest considering the TRENDY DGVM ensemble, which may provide explicit estimates for these variables.

Response

We consulted the TRENDY output available from trendyv12-gcb2023 at <https://mdosullivan.github.io/GCB/>. Indeed, several of the models (CLM5.0, JULES, LPJ-GUESS, LPJ-ML, OCN, SDGVM, VISIT, YIBs, LPX-Bern and JSBACH) have some PFT-specific variables available but these are not well defined for both C₃ and C₄ vegetations. As the reviewer suggests, these could provide explicit estimates of variables for crops vs natural vegetation and for C₃ vs C₄. In the Discussion, we have suggested analysis based on TRENDY models as a potential avenue for further research.

Comment 2

Writing quality and over-generalisation. Although I usually refrain from commenting on style, some issues affect the paper’s scientific clarity. I gave a few examples below

Carelessness: L137: “*Click or tab here to enter text*” is clearly a placeholder. The manuscript contains two “Discussion” sections (4 and 6); Section 6 seems intended as a Conclusion. Please proofread carefully. More examples in minor comments below.

Response

Our apologies for these oversights, which have been corrected.

Overstatements: The manuscript repeatedly describes the work as analysing “vegetation diversity” or “vegetation types” (L11, L16, L19, L74, L215, L275). In fact, it focuses on the photosynthetic pathway (C₃ vs C₄). Terms such as diversity or even vegetation types/PFTs carry broader meaning and could mislead readers. Being precise will not diminish the study’s novelty.

Response

We agree. We have replaced 'vegetation type' or 'vegetation diversity' with 'photosynthetic pathway' in several places. We have retained 'vegetation type' in some situations where a more general term seems appropriate.

Referee 2 other Comments and Responses

L31: “Land Cover and Land Use Change (LUCC)” is a more standard term.

Corrected to ‘land cover and land use change (LUCC)’.

L38: Molotoks et al.—only one reference is cited; the “a” after 2018 is unnecessary (cf. L286).

Corrected.

L43–44: The description of C₃ vs C₄ lacks detail on anatomical/structural differences that drive their distinct climate responses—important for motivating later arguments.

We have added a statement on the anatomical differences that drive their distinct climate responses.

L70: The “3” in C₃ should be subscript.

Corrected.

L125: You did not describe how C₃/C₄ Cveg was estimated; I assume you applied the same method as for GPP.

Response: Yes, we applied the same approach. We have revised the text to clarify this point.

L139: Define Δ_{tot} before using it.

Done.

L140: Too many simplifying assumptions undermine the credibility of later detailed analyses; the variation in Δ is particularly large.

Response: We are not sure what the reviewer meant by this comment. We are estimating the effect of C_3/C_4 vegetation proportion changes alone on global Δ – and therefore we have to use a fixed Δ for each photosynthetic pathway. We use values based on the Cornwell database.

L186: Please clarify the source of the 10–45 % cropland figures.

Response: The percentage of C_4 crop area is calculated as $(\text{C}_4 \text{ area} / \text{Total crop area}) \times 100$ for each of the models. The range given is from minimum to maximum for the year 2014. Fig. 4(a) and (g) have been added to the manuscript for improved clarity.

L201: “UKESM is quite consistent”—please explain.

This sentence has been restated as ‘UKESM1 estimate of the area of natural C_4 vegetation for 2000–14 compares well with Luo et al (2024)’s’.

L224: I remain unconvinced by the C_4 GPP and C_{veg} values.

We have tried to straightforwardly explain the method we used, and its limitations. We hope that future modelling studies will provide more information about the GPP and carbon content of C_3 vs C_4 vegetation.

L315: Instead of stating that COS measurements prove models underestimate CO_2 fertilisation, consider a more cautious phrasing, e.g. “we noticed there is still difference between models and COS on the quantification of eCO_2 effect and highlight the uncertainty in future ^{13}C discrimination rate as eCO_2 benefits C_3 more than C_4 ...”

Response: This point is noted. However, we find the statement is not overly confident as written, since it says “there may have been”: “In particular, since the GPP trend in the models (11 to 18 %, Fig. S8) is weaker than the CO_2 fertilization effect over the 20th century based on carbonyl sulphide data (+30 %; Campbell et al. 2017), there may have been a more positive C_3 vegetation-driven trend in discrimination in reality than in the models.”