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Peer Review File

Nitrogen limitation amplifies future warming by weakening terrestrial carbon cycle feedbacks and sink capacity

Corresponding Author: Dr Gang Tang

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Mr Tang,

Your manuscript titled "Nitrogen constraints shape climate and carbon cycle futures" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication. Specifically, we ask you to:

*Clarify the attribution logic to isolate the nitrogen-limitation effect;

*Strengthen the evaluation framing including historical benchmarking;

*Clearly docoment parameter consistency across configurations;

*Articulate the unique novelty of the present study specifically in relation to previous works.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Huai Chen, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0001-7650-289X

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript investigates how nitrogen limitation modulates atmospheric carbon concentrations and the carbon–climate feedback using MAGICC CNit v2.0 (MAGcn). The authors additionally explore counterfactual configurations by (i) switching off nitrogen limitation (MAGcnoff) and (ii) running the model without the nitrogen cycle (MAGc). A key motivation is MAGICC's ability to generate large ensembles at very low computational cost. This computational efficiency is a clear strength, and the topic is timely given persistent uncertainty about nutrient constraints on terrestrial carbon uptake and their implications for projected atmospheric CO_2 trajectories.

At present, I nonetheless have substantive concerns regarding process attribution and the evaluation strategy, which together limit how directly the reported differences can be interpreted as nitrogen-limitation effects. The central issue is whether the modelling setup can separate changes attributable to nitrogen limitation from changes that arise because other processes or parameters adjust when the nitrogen cycle is altered or removed. The manuscript notes the absence of structured Earth system model experiments specifically designed to isolate nitrogen-limitation mechanisms, which makes it particularly important to state the attribution logic explicitly. Without this clarity, it remains difficult to assess whether differences between configurations reflect nitrogen limitation itself, or whether they partly reflect compensating shifts in effective representations of CO_2 fertilization, baseline productivity, carbon turnover, and related components.

I also recommend strengthening the evaluation framing. Given that the manuscript discusses "evaluation" of the MAGcn configuration and draws implications about process effects using both MAGcnoff and MAGc simulations, it would be helpful to include a historical simulation benchmarked against observational constraints that are relevant to the coupled carbon–nitrogen system. The current emphasis on SSP-based behavior is informative for projection differences, but on its own it provides less confidence that key historical sensitivities are represented plausibly. This point becomes particularly important if the evaluated metrics do not show a clear qualitative improvement across configurations, as this makes it harder to distinguish a robust process signal from configuration-dependent tuning or compensating effects.

Finally, while turning off nitrogen limitation or removing the nitrogen cycle is a sensible sensitivity approach, its interpretation depends strongly on what is held fixed across configurations. If MAGc and MAGcnoff/MAGcn differ in calibration strategy or in parameter choices that compensate for missing processes, then the resulting differences are more appropriately interpreted as differences between model setups rather than as a controlled estimate of nitrogen limitation alone. The manuscript would benefit from a clearer description of what is kept identical across configurations, what (if anything) is re-optimized, and how the counterfactual experiments are designed to isolate nitrogen-limitation mechanisms as cleanly as possible.

Overall, the study addresses an important question and leverages a powerful computational framework. With a clearer attribution narrative, a strengthened evaluation component (ideally including historical benchmarking), and more explicit documentation of parameter consistency across configurations, the conclusions regarding nitrogen limitation and feedback strength would become substantially more interpretable and convincing.

Specific comments: Fig. 1a: As already noted, for a better understanding of what drives the differences between V8cnoff and V8c, I would have expected these to be very close, analogous to Fig. 1b. Please add the spread (e.g., percentile range) across realizations for each configuration.

Table: I do not understand why only a subgroup estimates MAGcn and MAGcnoff, while another subgroup is used for MAGc; this was not mentioned or justified in the text.

Page 7, paragraph 4: How is land use treated for MAGcnoff and MAGcn?

Page 8, paragraph 1 (last sentence): Reference 33 does not appear to support the statement as written.

Page 8, paragraph 2 (first sentence): I am not sure this sentence adds essential information; it was somewhat confusing given the similar phrasing at the start of the subsequent paragraph.

Page 8, paragraph 5: The statement "are driven solely by emissions" needs clarification. Are the ESMs used for emulation emission-driven in the same sense? If not, would the conclusions of this study change under a consistent forcing protocol?

Extended Data Figures 1, 2, and 3: The caption refers to black values, but I do not see black values in the figures.

Extended Data Figure 4: The caption appears incorrect; black values do not indicate MAGc, but rather MAGcnoff.

Supplementary Information: Page 1 (first sentence): The phrase "exact future" is an extremely strong claim and should be toned down.

Supplementary Information, page 1 (second paragraph): The statement "7 ppmv higher in emission-driven runs compared to concentration-driven runs in 2014" is unclear. Does "higher" imply closer to observations, or simply a protocol difference without implying improved realism?

Supplementary Information, page 1 (second paragraph): "with the esm-historical and historical runs showing a maximum difference of ~20 ppm around 1910." - Please clarify how this relates to the immediately preceding statement; the logical connection is not clear.

Supplementary Information, page 1 (second paragraph): The sentence beginning "Similarly, ACCESS-ESM1.5 projects higher CO_2 concentrations …" is difficult to follow. The selection of models is not explained, and the transition to reporting the full ensemble percentile range is abrupt. Please clarify the rationale for highlighting these particular models and explain how the quoted percentile ranges are intended to support the preceding claim.

Reviewer #2 (Remarks to the Author):

In the study, "Nitrogen constraints shape climate and carbon cycle futures" Tang et al. use a modified version of MAGICC7 that incorporates a novel carbon–nitrogen cycle scheme to conduct coupled climate–carbon–nitrogen experiments for low (SSP1-2.6) to high (SSP5-8.5) emission scenarios. Major findings include a general weakening of the carbon—concentration and carbon—climate feedbacks with nitrogen limitation, which raises atmospheric CO2 concentrations and, therefore, global temperatures for all considered scenarios. Emissions-driven projections, as well as the inclusion of a nitrogen cycle, is an eventual aim for Earth system models and in the very early stages of discussion for CMIP7 and beyond (Sanderson et al. 2024). Therefore, results based on simpler models, or emulators, are particularly useful for producing early estimates on the relative importance of a coupled nitrogen cycle, and its impact on different feedbacks. I believe this work is quite timely, and would be of relevance to the broader Earth system modelling community.

While the reasoning, experimental design, and modelling results within the paper generally makes sense, I struggled with the imprecision in the language and presentation of the results. Thus, I have several comments that should be helpful for improving the paper. I will not comment on the specific modelling approach used here, as the parameterization and its development have been addressed in a series of previous publications and, in my view, sufficiently tested and validated.

As the submission does not include line numbers, I will refer to lines by their position from the top of each page when necessary (e.g., P3, L2 means the second line on page 3).

General comments:

1. The main contribution of this study compared to Tang et al. (2025) and Tang et al. (2026) appears to be running the ensembles in emissions-driven mode, and the inclusion of a “nitrogen-off” configuration within the CN-coupled ensemble, as to quantify the strength of the nitrogen feedback on the climate and carbon cycle. I would like to see the authors more explicitly articulate the unique novelty of the present study specifically in relation to the previous two works.

2. On a similar note, I think the title of the paper, “Nitrogen constraints shape climate and carbon cycle futures” is quite general. To me, it is certain that nitrogen limitation can, and will, shape the future climate. This is even stated in the manuscript itself on P2, L1-2. As I interpret it, the main novelty of the study is that it is able to give an answer, and relative magnitude, as to ‘how’. A more targeted title that directly addresses this question and reflects the study’s main findings (e.g., nitrogen limitation has a positive feedback on warming, or the weakening of the other feedbacks) would help prepare the reader for the paper’s novelty.

3. My background is in general Earth system modelling, and I myself am not so familiar with the inner workings of the MAGICC7 reduced complexity model, so at times I felt like I needed to seek out further information from previous papers (e.g., Tang et al. 2025 and 2026), or often jump between the main article and the Methods or Supplementary section to try and understand what was meant, or how the experiments were conducted. Overall, the manuscript would generally benefit from more information about MAGICCv7/CNit v2.0 in the Methods, and general precision in the language, so that the paper can hold up on its own without needing try and decipher what is meant, or seeking out information from other papers. I will provide specific instances in the minor comments where I felt more information or precision was needed.

4. Finally, while the paper does a good job of providing a quantitative assessment, I thought that there was little interpretation or contextualization outside of comparing the different experimental ensembles to one another. I think the discussion on the broader implications of nitrogen limitation on different feedbacks should be fleshed out more, and how the end-of-century projections shown here compare with previous studies (or how ‘wrong’ earlier work may have been, by neglecting the nitrogen feedback effect). On P2, L2-4, the authors write “Many carbon-nitrogen coupled models have performed ‘nitrogen-on’ versus ‘nitrogen-off’ experiments to isolate the role of nitrogen in different processes” with several works cited there that could be discussed more. I recognize that this can be challenging when other experiments are not following the exact experimental protocol, but I think it would be useful to make comparisons when possible. I leave it to the authors to decide how to best contextualize their work compared to previous studies or current end-of-century projections.

Minor comments:

P2, L2-4: It would be helpful to establish early on in the paper what is exactly meant by “nitrogen-on” vs. “nitrogen-off” experiments. On P1, L10 it is suggested this could mean “with and without nitrogen cycles”, but for describing your ensemble on P1, L29 “MAGcnoff” means nitrogen does not constrain carbon dynamics. Sometimes the terminology “disabling nitrogen” is also used. I would try to stick to one way of describing your experiments, and to use this consistently in text.

P2, L3: Change ‘versus’ to ‘vs.’ for consistency with the rest of the text.

P2, L12 and L26: “nitrogen effects” can be changed to “the effect of nitrogen”

P2, L19: What is meant by “climate interactions” here?

In Fig. 1, 2, and 3: the acronyms v8cn, v8cnoff, and v8c are not defined anywhere. I assume that these correspond to the ensembles MAGcn, MAGcnoff, and MAGc, but this should be made clear. In fact, I’m not even sure where the ‘v8’ comes from (MAGICCv8?), however, the explicit naming leads me to believe it is somehow important, and therefore should be explained in the Methods section. If it is not important, then it should be renamed.

P3, L9-10 and P4, L3-4: Should be clarified that the ranges provided here are for the different emission scenarios (range SSP1-2.6 to SSP5-8.5).

P3, L10: Full distributions of what? Generally, this sentence can be written in a much more precise way. At the very least, I would clarify you mean GSAT anomalies, and reference the figure subplot.

P4, L4-7: Similar to above, these sentences can also be made much more precise by explicitly stating the 5th and 95th percentiles are for atmospheric CO_2 concentration (again with reference to the figure subplots).

P4, L10: Should be clarified as the “end-of-century” decadal mean for cNetLand.

P4, L15-17: The “increases from” is misleading here. You mean the end-of-century decadal mean increases ‘by’ 58.1-76.2 GtC/yr in the MAGcn ensemble, with higher emission scenarios on the upper end of this range. Please also direct the reader to Fig. 2 in this sentence, and make it clear that by “~1–6 GtC/yr” and “~12–25 GtC/yr” you mean something like the MAGcnoff ensemble is 1–6 GtC/yr larger than the MAGcn ensemble, and the MAGc ensemble is 12–25 GtC/yr larger than the MAGcn ensemble, depending on the emission scenario,

P5, L1: I would clarify that the reduced standard deviations are in NPP.

P5, L3-5: “Means” should be clarified as the end-of-century decadal mean, and “56.0 ± 12.3 to 71.0 ± 16.1 GtC/yr in MAGcn” should be clarified as the range across the different emission scenarios.

In Fig. 2 (and similar ones in the Extended Data section): An acronym is provided for different components of the budget, along with a long name. For consistency, the long name for cNetLand and cNetOcean should also be given.

In Fig. 3, no ‘(a)’ and ‘(b)’ label in the subplots; missing figure description for (a) and (b). What do the black dot and whiskers mean here? It also should be stated here that these are dedicated experiments, and the mention of “1pctCO2-bgc” and “1pctCO2” should direct the reader to the Methods section, as it is not described in the main body of the article.

P5, L18-19: Authors should reference Fig. 3“(a)” and specify ensemble mean is meant here.

P5-6, L19-22: I don’t understand “The strongest feedback (1.57 GtC/ppm) occurs in CNit(BCC-CSM2-MR), a C-only ESM” but then also “The MAGcnoff maximum (1.58 GtC/ppm) arises from the nitrogen-off mode of 6 CNit(UKESM1-0-LL)”. How is the strongest feedback 1.57 GtC/ppm, but the maximum of all ensembles 1.58 GtC/ppm? I believe you are trying to compare the MAGcn and MAGc ensemble mean to the MAGcnoff ensemble mean here, but this comparison should be written more explicitly.

P6, L4-5: Please specify that these numbers refer to the ensemble mean here.

Table 1: I suppose this table is meant to list the models in alphabetical order, so ‘CNit(CESM2-WACCM)’ can be moved up a few rows.

P7, L12-14: In Fig. 1, the differences in the ensembles only minimally change the timing of when 1.5 °C is reached, and it is more strongly affected by the given SSP scenario. I would instead focus on a 2 °C GSAT anomaly, as nitrogen feedbacks seem to more significantly change what year this is reached for, e.g., SSP2-4.5 (see Fig. 1a).

P8, L5: DGVM not defined.

Why does nitrogen not amplify the carbon–climate feedback in CNit(CMCC-CM2-SR5), CNit(CMCC-ESM2), and CNit(CESM2-WACCM)? Are there structural reasons for this difference compared to the other ensemble members (e.g., climate sensitivity, a certain parameter, or pool)? I found this particularly interesting, as disabling nitrogen cycling strengthens the other considered feedback in all cases.

P8: The acronym GCB was previously introduced as GCB2024.

P10: The authors state that the MAGICC-CNit v2.0 ensemble is run in an emissions-driven mode. Some more text should be added to describe the experimental set-up in greater detail, particularly for readers who are not modellers, or familiar with differences between MAGICC and ESMs. Specifically, it would be useful to clarify what exactly is prescribed to the model. For example, are only CO2 (and CH4) emissions prescribed? Is N2O provided as a concentration, like other models? This was difficult for me to decipher, as in Tang et al. (2026) it was stated that: “CNit v1.0 uses net land use emissions (for both carbon and nitrogen) as input. However, this is not always the case, such as when gross emissions are provided instead of net emissions in certain scenarios.”

P10, L2-5: Is there a technical reason why you change between “model”, “module”, and “emulator”?

You start by talking about MAGICC6 (P10, L2), before establishing that you use MAGICC7 (P10, L3 and L14), while also stating that its actually MAGICC v7.5.3 (P10, L14 and L18), which is sometimes introduced as MAGv7 (P10, L19). Later, you forgo the ‘7’ and MAGv7 convention entirely, and use MAGICC-CNit v2.0 (P10, L20) to discuss the model that’s used in this work. At later points, this v2.0 is dropped so that you only refer to it as MAGICC-CNit (P10, L29-30). On top of that, the experimental ensembles also forgo the established MAGv7 convention, and use the names MAGc, MAGcn, and MAGcnoff. As is, it is a bit difficult to read, and to understand when we are discussing a different model, versus a different modelling set-up. At the very least, I recommend to stick to one naming convention for the model used in the paper (e.g., MAGICC7-CNit v2.0).

P10: “—a key advancement over CMIP5” I don’t think this is necessary as it has already been established earlier in the sentence, and we are quickly moving towards CMIP7.

P10, L25: “one using C-only ESM emulations… and another using CN-coupled ESM calibrations” Is there a technical reason why the word ‘emulation’ is used for one ensemble, but ‘calibration’ for another?

P11, L14: Typo: “quadruped” should be “quadrupled”. It might be helpful to state in the Methods that results for these dedicated experiments are evaluated at year 140.

Extended data is not included in the Communications Earth & Environment article format, so I leave it to the authors to decide what should be moved to the supplementary.

P16, Extended Data Fig. 5: Caption should be made more precise. I realize ‘warming’ is given a definition on P3, L2, but I would write out here that “projected peak warming” means maximum GSAT anomaly relative to the mean of years 1850–1900.

References:

Sanderson, B. M., Booth, B. B. B., Dunne, J., Eyring, V., Fisher, R. A., Friedlingstein, P., Gidden, M. J., Hajima, T., Jones, C. D., Jones, C. G., King, A., Koven, C. D., Lawrence, D. M., Lowe, J., Mengis, N., Peters, G. P., Rogelj, J., Smith, C., Snyder, A. C., Simpson, I. R., Swann, A. L. S., Tebaldi, C., Ilyina, T., Schleussner, C.-F., Séférian, R., Samset, B. H., van Vuuren, D., and Zaehle, S.: The need for carbon-emissions-driven climate projections in CMIP7, Geosci. Model Dev., 17, 8141–8172, https://doi.org/10.5194/gmd-17-8141-2024, 2024.

Tang, G., Nicholls, Z., Norton, A., Zaehle, S., and Meinshausen, M.: Synthesizing global carbon–nitrogen coupling effects – the MAGICC coupled carbon–nitrogen cycle model v1.0, Geosci. Model Dev., 18, 2193–2230, https://doi.org/10.5194/gmd-18-2193-2025, 2025.

Tang, G., Zaehle, S., Nicholls, Z., Norton, A., Ziehn, T., & Meinshausen, M.: Understanding the drivers of carbon–nitrogen cycle variability in CMIP6 ESMs with MAGICC CNit v2.0: Model and calibration updates, Journal of Advances in Modeling Earth Systems, 18, e2025MS005270. https://doi.org/10.1029/2025MS005270, 2026.

Reviewer #3 (Remarks to the Author):

The paper under review presents a novel analysis regarding the use of the MAGICC RCM. By incorporating the CNit submodule it emulates the behavior, in an emission driven configuration, of 14 ESMs for six SSPs and two idealized scenarios, with the aim of understanding the role and impact of Nitrogen limitation for global climate and carbon cycle. In doing this, the paper structure is very well defined, the results are presented in a precise, clean and concise manner and are well understandable. In my opinion, this paper is useful to the scientific community as it summarizes the main impacts that the implementation of Nitrogen limitation has in CMIP6 ESMs, most importantly by isolating Nitrogen effect (CNit and CNoff runs) and performing simulations that allow for the nutrient direct feedback on atmospheric CO2 concentrations (emission-driven runs).
Probably, it is a bit unclear to me what it is the main claim of the paper. Is it a synthesis of carbon-nitrogen status in ESMs for SSPs type of paper, or rather an analysis aiming to stress the importance of emulators in performing simulations compared to ESMs? What is the real novelty of this analysis? For example, it is already well known that Nitrogen influences climate and carbon cycle by limiting vegetation productivity, as reported in the manuscript (1,2,3). Additionally, the authors have recently published the methodological paper of MAGICC-CNitv2.0 (4), which presents and thoroughly examines C-N cycles variability in CMIP6 ESMs. To conclude, please try to stress the added scientific step that this work has done.

That said, I have just one point I am curious about. While it is explained that Nitrogen primarily limits CO2 fertilization and thus the carbon-concentration feedback, it is not clear why MAGcn ensemble has a lower (16%, in absolute terms) carbon-climate feedback compared to MAGcnoff. Could you try to elaborate why Nitrogen could decrease the (absolute) value of gamma?

Finally, some minor points that I think may improve the quality of the manuscript. Please find them listed below:

Please insert “Introduction” as a title section. Also, it would be easier to have line numbers at this revision stage for simplifying the review process and pointing to specific sentences. Not a problem, just a suggestion!
Please insert letters a) and b) for the panels in Figure 3.

References:
Thornton PE, Lamarque J-F, Rosenbloom NA, Mahowald NM. Influence of carbon-nitrogen cycle coupling on land model response to CO2 fertilization and climate variability. Global Biogeochemical Cycles 2007, 21(4).

LeBauer DS, Treseder KK. Nitrogen limitation of net primary productivity in terrestrial ecosystems is globally distributed. Ecology 2008, 89(2): 371-379

Zaehle S, Friedlingstein P, Friend AD. Terrestrial nitrogen feedbacks may accelerate future climate change. Geophysical Research Letters 2010, 37(1).

Tang, G., Zaehle, S., Nicholls, Z., Norton, A., Ziehn, T., & Meinshausen, M. (2026). Understanding the drivers of carbon–nitrogen cycle variability in CMIP6 ESMs with MAGICC CNit v2. 0: Model and calibration updates. Journal of Advances in Modeling Earth Systems, 18(1).

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Dear Dr Tang,

Your manuscript titled "Nitrogen limitation amplifies future warming by weakening terrestrial carbon cycle feedbacks and sink capacity" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

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ChenRui Diao, PhD
Associate Editor,
Communications Earth & Environment
Consulting Editor,
Communications Sustainability

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

During my reading of the revised manuscript, I found that the overall objective and scope of the study became clearer. This may partly reflect increased familiarity with the manuscript, but it also suggests that my previous comments have been taken into account. Nevertheless, I still feel that the level of detail in the methods section could be improved.

Reviewer #2 (Remarks to the Author):

Thank you for the opportunity to review the manuscript, “Nitrogen limitation amplifies future warming by weakening terrestrial carbon cycle feedbacks and sink capacity” by Tang et al. This is my second time reviewing this paper.

As mentioned before, I still think this is quite a timely study, and it has improved considerably since the first round of review. The authors have adequately addressed almost all remarks I raised, and I am generally satisfied with the changes. The manuscript is now much clearer, and therefore, I am happy to recommend it for publication. While I have a few minor (non-technical) comments listed below, I believe these can be addressed without the need for an additional round of review.

Page 4: Should “end-of-central” be “end-of-century”?
Page 5: Same comment as above.
Page 5, Figure 3: I am still missing an explicit statement on what the black whiskers represent here.
Supplementary Page 2: In your response, you stated you originally used MAGICCv8 to mean MAGICC-CNit, to distinguish it from MAGICC v7.5.3. To keep consistency with the main text, I wonder if it would make sense to forgo the MAGv8 here and replace it with MAGICC-CNit, since the v8 is not used elsewhere.

Reviewer #3 (Remarks to the Author):

The reviewed version of the manuscript incorporates the suggestions proposed during the first round of revisions. Most importantly, it clarified two main points that were raised, regarding the explicit novelty compared to the two previous manuscripts (Tang et al., 2025, 2026), and the further explanation regarding the carbon-climate feedback values in MAGcn and MAGcnoff simulations. In my opinion, this, together with the modifications taken thanks to the other two reviewers, have improved the clarity and completeness of the work. I therefore recommend this research paper for publication.

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Response to Reviewer

For Tang et al., Nitrogen limitation amplifies future warming by weakening terrestrial carbon cycle feedbacks and sink capacity

In this Response to Reviewer:

- The original referee comments are in black (directly copied from the comments).
- Our responses are in blue.
- *The text we quoted from the manuscript is in gray italics.*

We sincerely thank all referees for their constructive comments and feedback on our manuscript.

Best regards,

Gang Tang (GT, as referenced below)

on behalf of all co-authors

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Reviewer #1 (Remarks to the Author):

The manuscript investigates how nitrogen limitation modulates atmospheric carbon concentrations and the carbon–climate feedback using MAGICC CNit v2.0 (MAGcn). The authors additionally explore counterfactual configurations by (i) switching off nitrogen limitation (MAGcnoff) and (ii) running the model without the nitrogen cycle (MAGc). A key motivation is MAGICC's ability to generate large ensembles at very low computational cost. This computational efficiency is a clear strength, and the topic is timely given persistent uncertainty about nutrient constraints on terrestrial carbon uptake and their implications for projected atmospheric CO_2 trajectories.

At present, I nonetheless have substantive concerns regarding process attribution and the evaluation strategy, which together limit how directly the reported differences can be interpreted as nitrogen-limitation effects. The central issue is whether the modelling setup can separate changes attributable to nitrogen limitation from changes that arise because other processes or parameters adjust when the nitrogen cycle is altered or removed. The manuscript notes the absence of structured Earth system model experiments specifically designed to isolate nitrogen-limitation mechanisms, which makes it particularly important to state the attribution logic explicitly. Without this clarity, it remains difficult to assess whether differences between configurations reflect nitrogen limitation itself, or whether they partly reflect compensating shifts in effective representations of CO_2 fertilization, baseline productivity, carbon turnover, and related components.

I also recommend strengthening the evaluation framing. Given that the manuscript discusses "evaluation" of the MAGcn configuration and draws implications about process effects using both MAGcnoff and MAGc simulations, it would be helpful to include a historical simulation benchmarked against observational constraints that are relevant to the coupled carbon–nitrogen system. The current emphasis on SSP-based behavior is informative for projection differences, but on its own it provides less confidence that key historical sensitivities are represented plausibly. This point becomes particularly important if the evaluated metrics do not show a clear qualitative improvement across configurations, as this makes it harder to distinguish a robust process signal from configuration-dependent tuning or compensating effects.

Finally, while turning off nitrogen limitation or removing the nitrogen cycle is a sensible sensitivity approach, its interpretation depends strongly on what is held fixed across configurations. If MAGc and MAGcnoff/MAGcn differ in calibration strategy or in parameter choices that compensate for missing processes, then the resulting differences are more appropriately interpreted as differences between model setups rather than as a controlled estimate of nitrogen limitation alone. The manuscript would benefit from a clearer description of what is kept identical across configurations, what (if anything) is re-optimized, and how the counterfactual experiments are designed to isolate nitrogen-limitation mechanisms as cleanly as possible.

Overall, the study addresses an important question and leverages a powerful computational framework. With a clearer attribution narrative, a strengthened evaluation component (ideally including historical benchmarking), and more explicit documentation of parameter consistency across configurations, the conclusions regarding nitrogen limitation and feedback strength would become substantially more interpretable and convincing.

GT: Thank you for reviewing our manuscript and for your positive feedback on the topic and its value. We appreciate your constructive comments and suggestions. Detailed responses to the specific comments are provided below. Here we also address the key points raised in the general comments above.

First, regarding process attribution and whether the modelling setup can separate nitrogen-limitation effects from other adjustments when the nitrogen cycle is altered or removed. Our experimental design explicitly addresses this through the comparison between MAGcn and MAGcnoff, which use identical carbon-cycle parameter sets within the same MAGICC-CNit emulator, differing only in whether the nitrogen cycle and carbon–nitrogen coupling are active. Consequently, differences between MAGcn and MAGcnoff isolate the nitrogen effects independent of broader model structural differences. In contrast, the comparison between MAGc and MAGcn reflects the structural differences between ESMs with and without a nitrogen cycle. This two-step comparison allows us to separate nitrogen effects from structural differences across ESM formulations.

The absence of nitrogen-off experiments in CN-coupled ESMs motivates this emulation-based approach. Because CN-coupled ESMs have only been calibrated and evaluated in their nitrogen-on configurations, there is no direct ESM benchmark for nitrogen-off behaviour. MAGICC-CNit therefore provides a framework to explore this counterfactual consistently across models.

While a direct validation of nitrogen-off behaviour is not possible, several lines of evidence suggest that the diagnosed response is physically reasonable. First, MAGcnoff and MAGc produce very similar ensemble-mean carbon–concentration and carbon–climate feedback strengths (Table 1). This indicates that disabling nitrogen limitation in the CN-coupled emulations produces behaviour comparable to that of C-only ESMs, as would be expected given that all ESMs are constrained through calibration to historical benchmarking data. Second, comparisons for 2014–2023 with observations and the Global Carbon Budget 2024 (Extended Data Fig. 5; Supporting Information Section S2) show that MAGcnoff produces results similar to MAGc for key carbon fluxes. Together, these results increase confidence that the nitrogen-off configuration behaves consistently with expectations.

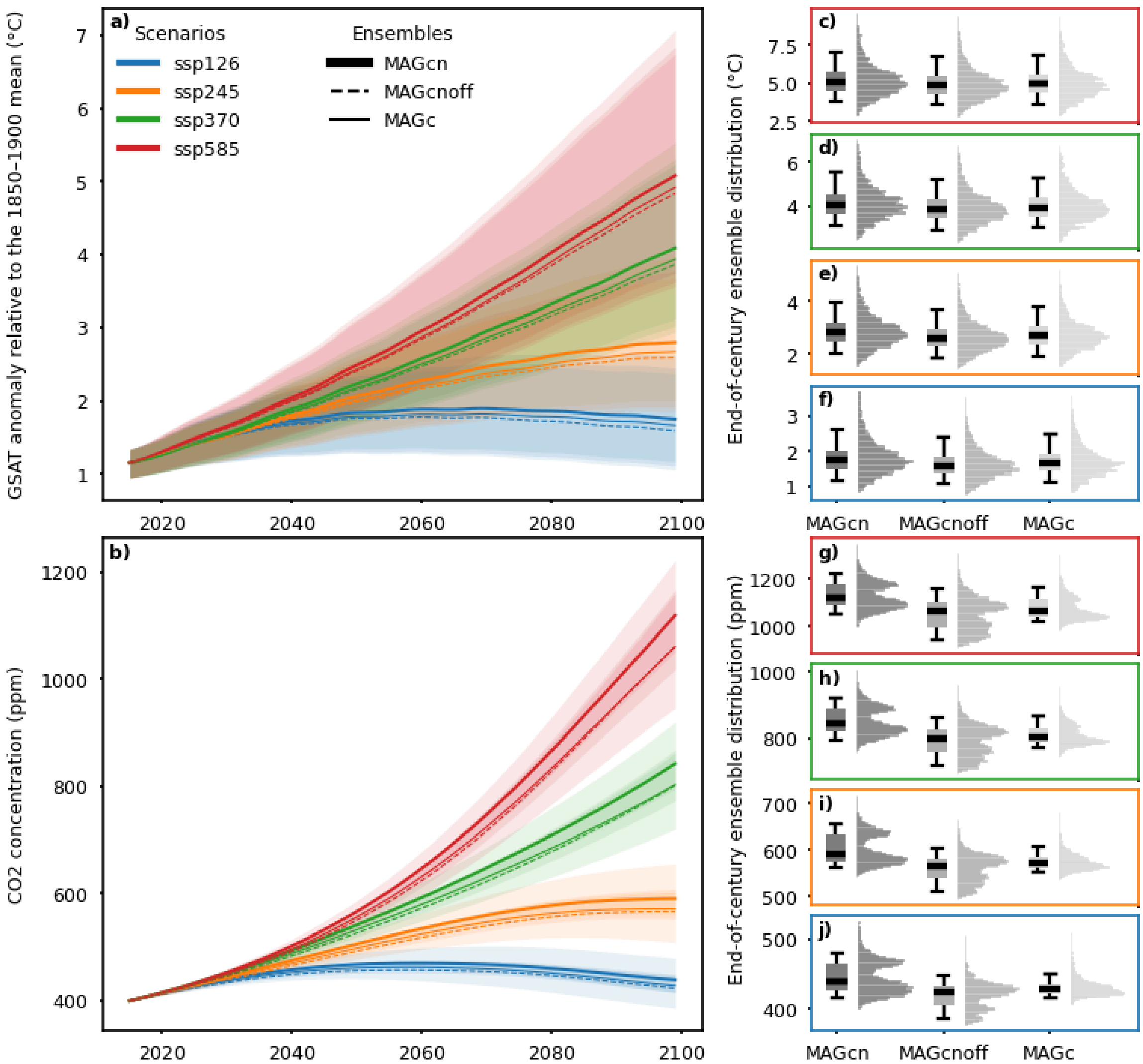
Regarding historical benchmarking, the historical period is already incorporated in the calibration of MAGICC-CNit to the emulated ESMs (Tang et al., 2025; Tang et al., 2026). As a result, the emulator reproduces the historical behaviour of the individual ESMs by design. These previous studies already analyse nitrogen-related behaviour in the historical simulations of the ESMs themselves. Conducting a full observational benchmarking of both nitrogen-on and nitrogen-off configurations from 1850 onward would therefore go beyond the scope of the present work, which focuses on probabilistic cross-ESM projections. Moreover, substantial variability exists among ESM historical simulations in key carbon and nitrogen fluxes and pool sizes, and even basic mass conservation issues have been documented in many models (Tang et al., 2025). Consequently, distinguishing whether discrepancies arise from the experimental setups or from the underlying ESMs would require a probabilistic ensemble of carbon–nitrogen parameter sets constrained by observational estimates (i.e., an ensemble that treats individual ESM outputs as references while sampling parameter distributions consistent with observational uncertainties). Developing such a framework is an ongoing line of work (as mentioned in the second last paragraph of Discussions "*This self-consistency enables integration of information from the GCB estimates, CMIP assessments, and observations (not shown here but representing the logical next step for constructing a Bayesian ensemble) without violating fundamentals such as mass conservation, which can be difficult when combining results from different models (e.g., the mass imbalance in the GCB estimates)*". The present study, as highlighted in the Abstract and Introduction and clarified in the Methods, focuses instead on probabilistic cross-ESM projections, emulating the behaviour of ESMs regardless of how well the individual models reproduce the historical carbon cycle simulation.

Finally, regarding configuration consistency, all ESM emulations are calibrated using the same methodology (Tang et al., 2026). The climate response, radiative forcing, and gas-cycle parameters are consistently drawn from MAGICC v7.5.3 across all ensembles. The only differences among MAGc, MAGcn, and MAGcnoff lie in the carbon–nitrogen cycle parameter values and the activation state of the nitrogen cycle. Specifically, MAGc uses parameter sets calibrated to C-only ESMs, MAGcn uses parameter sets calibrated to CN-coupled ESMs, and MAGcnoff uses the same CN-coupled parameter sets but with the nitrogen cycle and carbon–nitrogen coupling disabled. We have clarified this point in the revised Methods section as follows:

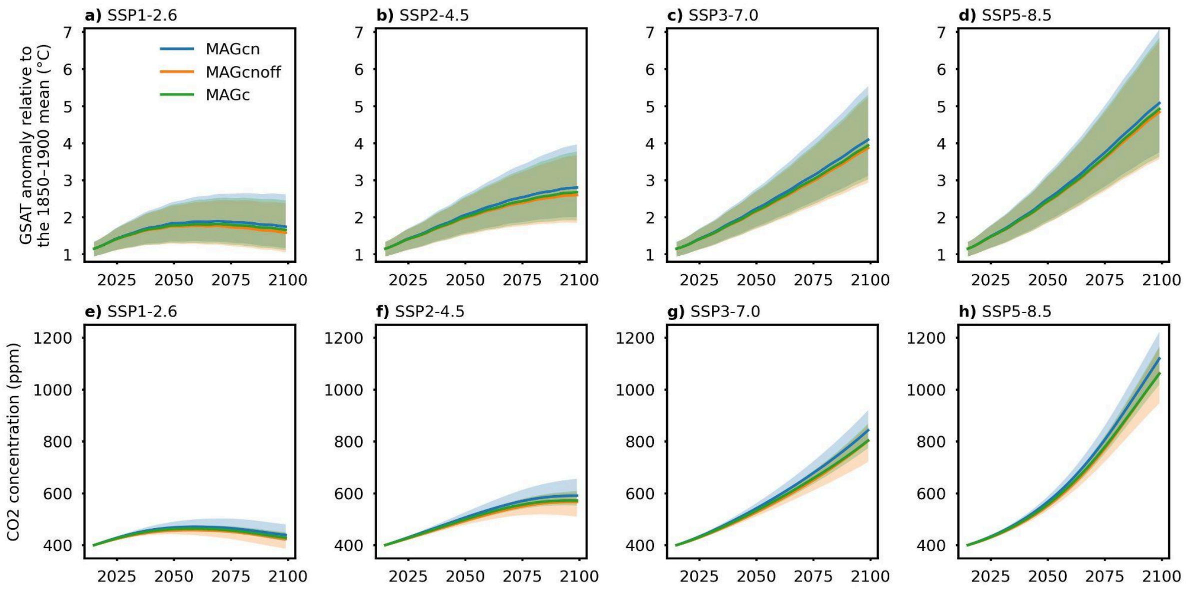
Consequently, MAGc, MAGcn, and MAGcnoff differ only in their carbon–nitrogen parameter values — reflecting the emulated C-only ESMs, the emulated CN-coupled ESMs, and the emulated CN-coupled ESMs with deactivated carbon–nitrogen coupling, respectively. Thus, comparing MAGc and MAGcn highlights the structural differences between ESMs with and without a nitrogen cycle, while comparing MAGcn and MAGcnoff disentangles the nitrogen effect independent of those broader model structural differences.

Specific comments: Fig. 1a: As already noted, for a better understanding of what drives the differences between V8cnoff and V8c, I would have expected these to be very close, analogous to Fig. 1b. Please add the spread (e.g., percentile range) across realizations for each configuration.

GT: Thank you for the comment and suggestion. We initially attempted to include the ensemble spread in the projection time series. However, with four SSP scenarios and three ensembles, plotting all spreads together makes the figure difficult to interpret (see figure below).



We therefore retained the ensemble spread in panels c–j, where it can be visualized more clearly. In addition, we have now included the spread in a new Extended Data Fig. 1 and referenced the similarities and differences in the manuscript.



Extended Data Figure 1 Projected global surface air temperature (GSAT) anomalies relative to the 1850–1900 mean and atmospheric CO_2 concentrations from the MAGICC-CNit ensembles under different SSP scenarios. Solid lines indicate the ensemble medians, and shaded areas show the 5th–95th percentiles.

Table: I do not understand why only a subgroup estimates MAGcn and MAGcnoff, while another subgroup is used for MAGc; this was not mentioned or justified in the text.

GT: Thank you for the comment. MAGICC-CNit is calibrated to emulate a total of 14 ESMs, including nine CN-coupled and five C-only models. Accordingly, MAGcn refers to the ensemble emulating CN-coupled ESMs, MAGc refers to the ensemble emulating C-only ESMs, and MAGcnoff refers to the CN-coupled ESM emulations with the nitrogen cycle disabled such that nitrogen does not affect carbon dynamics (now clarified in the Methods).

In Table 1, CNit(XXX) denotes the specific ESM emulated by CNit. For example, CNit(ACCESS-ESM1-5) represents the CNit emulation of ACCESS-ESM1-5, which is itself a CN-coupled ESM. Therefore, it does not belong to the MAGc ensemble, and the carbon-cycle feedback values under the MAGc column are not applicable. We have now added a note to the table to clarify this point.

– indicates not applicable because the emulated ESM is not included in the corresponding ensemble (see Methods).

Page 7, paragraph 4: How is land use treated for MAGcnoff and MAGcn?

GT: Thanks for the question. They use the same land-use parameterization with identical parameter values (now clarified in Methods).

Page 8, paragraph 1 (last sentence): Reference 33 does not appear to support the statement as written.

GT: Thanks for the comment. The description and discussion is now updated as below.

This aligns with previous CNit results showing CO_2 fertilization is artificially enhanced to compensate for missing nitrogen deposition and fertilization 20, as well as with evidence from dynamic global vegetation models indicating that calibrating terrestrial carbon uptake to the historical sink can create a disconnect between simulated carbon and nitrogen cycling processes 33.

Page 8, paragraph 2 (first sentence): I am not sure this sentence adds essential information; it was somewhat confusing given the similar phrasing at the start of the subsequent paragraph.

GT: Thank you for the comment. The sentence is intended to compare our results, which include 14 ESMs, with earlier estimates based on 11 ESMs [ref. 11], thereby highlighting the uncertainty in carbon cycle feedbacks across CMIP6 models. Specifically, by including additional ESMs, our estimate of the multi-model mean feedback changes relative to the earlier widely used values. In this comparison, CN-coupled and C-only ESMs are combined to provide a direct comparison with the estimates in ref. 11, which also aggregate models.

The subsequent paragraphs, in contrast, focus on differences between the emulated CN-coupled and C-only ESMs (i.e., comparisons among MAGcn, MAGcnoff, and MAGc), which represent subgroups within the full model ensemble.

To clarify this distinction, we have revised the sentence as follows::

For the multi-model mean combining all ESMs, we find a weaker carbon–concentration feedback and stronger carbon–climate feedback compared with earlier results (0.83 ± 0.40 GtC ppm^{-1} and -52.21 ± 41.21 GtC °C^{-1} with 14 ESMs here vs. 0.97 ± 0.40 GtC ppm^{-1} and -45.1 ± 50.6 GtC °C^{-1} with 11 ESMs in reference 11).

Page 8, paragraph 5: The statement “are driven solely by emissions” needs clarification. Are the ESMs used for emulation emission-driven in the same sense? If not, would the conclusions of this study change under a consistent forcing protocol?

GT: Thank you for the comment. Here, we mean that MAGICC-CNit runs emission-driven experiments with all other forcings kept consistent across ensembles. This contrasts with most CMIP6 ESM experiments, which are typically concentration-driven, using prescribed CO_2 concentrations from earlier MAGICC runs. The original phrasing “driven solely by emissions” was intended to distinguish this setup from ESMs that may include differing forcings — such as the inclusion of direct vs. indirect aerosol effects — but the term “solely” is somewhat redundant and potentially confusing. We have therefore removed it for clarity.

Extended Data Figures 1, 2, and 3: The caption refers to black values, but I do not see black values in the figures.

Extended Data Figure 4: The caption appears incorrect; black values do not indicate MAGc, but rather MAGcnoff.

GT: Thank you for the comment. These errors arose from copying the Figure 1 caption. We have now carefully reviewed all figure captions and corrected them.

Supplementary Information: Page 1 (first sentence): The phrase “exact future” is an extremely strong claim and should be toned down.

GT: Thanks for the comment. The sentence has been revised as follows:

However, even under a given scenario — whether defined by concentrations or emissions — no single model can fully determine the future due to limitations in our understanding of the climate system, imperfect model representation of processes, and intrinsic natural variability.

Supplementary Information, page 1 (second paragraph): The statement “7 ppmv higher in emission-driven runs compared to concentration-driven runs in 2014” is unclear. Does “higher” imply closer to observations, or simply a protocol difference without implying improved realism?

GT: Thank you for the comment. This comparison simply reflects the difference between the models’ concentration-driven and emission-driven runs. For concentration-driven runs, CO_2 concentrations are prescribed based on historical estimates from the Global Carbon Budget (a synthesis of observations and modeling), so these runs necessarily align most closely with observations. The statement does not imply that the emission-driven runs are more realistic.

Supplementary Information, page 1 (second paragraph): “with the esm-historical and historical runs showing a maximum difference of ~20 ppm around 1910.” - Please clarify how this relates to the immediately preceding statement; the logical connection is not clear.

Supplementary Information, page 1 (second paragraph): The sentence beginning “Similarly, ACCESS-ESM1.5 projects higher CO_2 concentrations …” is difficult to follow. The selection of models is not explained, and the transition to reporting the full ensemble percentile range is abrupt. Please clarify the rationale for highlighting these particular models and explain how the quoted percentile ranges are intended to support the preceding claim.

GT: Thank you for the comments. These examples were originally intended to illustrate studies comparing concentration-driven and emission-driven results, supporting the point that “to our knowledge, there is currently no comprehensive multi-model analysis of emission-driven projections, with only a few studies presenting results from individual models.” However, because our focus includes not only atmospheric CO_2 but also the full carbon cycle, integrating flux comparisons led to a somewhat awkward and disjointed discussion. We have now revised the paragraph to improve the flow, clarify the connection between statements, and explain the rationale for highlighting specific models and their percentile ranges.

To our knowledge, there is currently no comprehensive multi-model analysis of emission-driven projections, with only a few studies presenting results from individual models. For example, NASA’s GISS ModelE2.1-G-CC projects CO_2 concentrations in 2100 that are ~25 ppm higher in its esm-SSP5-8.5 run compared to the corresponding concentration-driven SSP5-8.5 run (1160 vs. 1135 ppm) 7, with the esm-historical and historical runs showing a maximum difference of ~20 ppm around 1910. Similarly, ACCESS-ESM1.5 projects higher CO_2 concentrations and warmer futures under esm-SSP5-8.5 compared to the concentration-driven SSP5-8.5 run 8. The concentration difference partially reflects a limitation of the experimental design: because the concentration pathways used to drive the ESMs were originally generated by MAGICC (MAGv7), exact agreement between MAGICC-simulated concentrations and emission-driven ESM simulations is not expected. Nevertheless, the esm-SSP5-8.5 concentrations from ESMs are well encompassed by the 5th–95th percentile range of the mixed MAGcn and MAGc ensembles (i.e., the full ensemble encompassing all emulated CMIP6 ESMs, hereafter MAGv8; 1028.58–1212.96 ppm) and lie toward the upper end of the MAGv7 ensemble range (994.82–1175.7 ppm). Moreover, the 2100 air-to-land and air-to-sea fluxes from ModelE2.1-G-CC (3.8 and 5.2 GtC yr^{-1}, respectively) closely match the MAGv8 ensemble medians of 3.9 and 5.6 GtC yr^{-1}. These agreements further demonstrate the *improved emulation capability of the updated MAGICC during the projection period and increase confidence in the reliability of its emission-driven projections.*

Reviewer #2 (Remarks to the Author):

In the study, "Nitrogen constraints shape climate and carbon cycle futures" Tang et al. use a modified version of MAGICC7 that incorporates a novel carbon–nitrogen cycle scheme to conduct coupled climate–carbon–nitrogen experiments for low (SSP1-2.6) to high (SSP5-8.5) emission scenarios. Major findings include a general weakening of the carbon—concentration and carbon—climate feedbacks with nitrogen limitation, which raises atmospheric CO2 concentrations and, therefore, global temperatures for all considered scenarios. Emissions-driven projections, as well as the inclusion of a nitrogen cycle, is an eventual aim for Earth system models and in the very early stages of discussion for CMIP7 and beyond (Sanderson et al. 2024). Therefore, results based on simpler models, or emulators, are particularly useful for producing early estimates on the relative importance of a coupled nitrogen cycle, and its impact on different feedbacks. I believe this work is quite timely, and would be of relevance to the broader Earth system modelling community.

While the reasoning, experimental design, and modelling results within the paper generally makes sense, I struggled with the imprecision in the language and presentation of the results. Thus, I have several comments that should be helpful for improving the paper. I will not comment on the specific modelling approach used here, as the parameterization and its development have been addressed in a series of previous publications and, in my view, sufficiently tested and validated.

As the submission does not include line numbers, I will refer to lines by their position from the top of each page when necessary (e.g., P3, L2 means the second line on page 3).

GT: Thank you for reviewing our manuscript, acknowledging its strengths, and providing constructive comments to help improve it. Please see our responses to the specific comments below. Regarding the line numbers, we followed the journal's formatting instructions, which do not include them.

General comments:

1. The main contribution of this study compared to Tang et al. (2025) and Tang et al. (2026) appears to be running the ensembles in emissions-driven mode, and the inclusion of a "nitrogen-off" configuration within the CN-coupled ensemble, as to quantify the strength of the nitrogen feedback on the climate and carbon cycle. I would like to see the authors more explicitly articulate the unique novelty of the present study specifically in relation to the previous two works.

GT: Thank you for the comment. We have now clarified the specific novelty of the present study relative to our previous work [Tang et al. 2025; Tang et al., 2026]. In particular, we emphasize that this work extends those studies by performing large emission-driven, fully-coupled ensemble projections and by introducing a nitrogen-off configuration within the CN-coupled ensemble to explicitly diagnose nitrogen feedbacks on the coupled climate-carbon-nitrogen system.

Our recent work partially resolves both challenges: using CNit, a unified framework with demonstrated emulation performance for the majority of CMIP6 ESMs, we have synthesized and quantified the magnitude and uncertainty of nitrogen effects represented in state-of-the-art ESMs 19, 20. Yet, the critical next step — quantifying how these nitrogen constraints dynamically interact with the climate and carbon cycle within fully coupled, emission-driven projections — remains unexplored. To bridge this gap, we use MAGICC-CNit …

2. On a similar note, I think the title of the paper, "Nitrogen constraints shape climate and carbon cycle futures" is quite general. To me, it is certain that nitrogen limitation can, and will, shape the future climate. This is even stated in the manuscript itself on P2, L1-2. As I interpret it, the main novelty of the study is that it is able to give an answer, and relative magnitude, as to 'how'. A more targeted title that directly addresses this question and reflects the study's main findings (e.g., nitrogen limitation has a positive feedback on warming, or the weakening of the other feedbacks) would help prepare the reader for the paper's novelty.

GT: Thank you for the comment. We have now revised the title to be more detailed and specific:

Nitrogen limitation amplifies future warming by weakening terrestrial carbon cycle feedbacks and sink capacity

3. My background is in general Earth system modelling, and I myself am not so familiar with the inner workings of the MAGICC7 reduced complexity model, so at times I felt like I needed to seek out further information from previous papers (e.g., Tang et al. 2025 and 2026), or often jump between the main article and the Methods or Supplementary section to try and understand what was meant, or how the experiments were conducted. Overall, the manuscript would generally benefit from more information about MAGICCv7/CNit v2.0 in the Methods, and general precision in the language, so that the paper can hold up on its own without needing try and decipher what is meant, or seeking out information from other papers. I will provide specific instances in the minor comments where I felt more information or precision was needed.

GT: Thank you for the comment. In general terms, MAGICC can be viewed as an extremely simplified Earth system model, with CNit representing the carbon–nitrogen cycle component within it. We appreciate the specific points raised later in the minor comments, and we have revised the manuscript accordingly to provide more information in the Methods and to improve clarity for a broader range of readers. Please see our responses to the specific minor comments below.

4. Finally, while the paper does a good job of providing a quantitative assessment, I thought that there was little interpretation or contextualization outside of comparing the different experimental ensembles to one another. I think the discussion on the broader implications of nitrogen limitation on different feedbacks should be fleshed out more, and how the end-of-century projections shown here compare with previous studies (or how 'wrong' earlier work may have been, by neglecting the nitrogen feedback effect). On P2, L2-4, the authors write "Many carbon-nitrogen coupled models have performed 'nitrogen-on' versus 'nitrogen-off' experiments to isolate the role of nitrogen in different processes" with several works cited there that could be discussed more. I recognize that this can be challenging when other experiments are not following the exact experimental protocol, but I think it would be useful to make comparisons when possible. I leave it to the authors to decide how to best contextualize their work compared to previous studies or current end-of-century projections.

GT: Thank you for the comment. We agree that broader contextualization is important. However, direct comparisons are challenging because emission-driven studies focusing specifically on nitrogen effects are extremely rare, if they exist at all. Nitrogen-on vs. nitrogen-off experiments are sometimes conducted in dynamic global vegetation models (DGVMs), in offline setups with prescribed climate and atmospheric CO_2, to evaluate their newly implemented nitrogen cycle modules.

For emission-driven experiments, however, a climate component is required, meaning the model must effectively function as an Earth system model or AOGCM. As noted, most CMIP6 experiments are concentration-driven rather than emission-driven. As a result, emission-driven ESM studies are already limited, and, to our knowledge, none explicitly examine nitrogen effects. This creates a gap in the literature and makes direct comparison difficult. At the same time, it highlights one of the key motivations and contributions of our study: to help fill this gap by providing emission-driven projections that explicitly diagnose nitrogen feedbacks.

Minor comments:

P2, L2-4: It would be helpful to establish early on in the paper what is exactly meant by “nitrogen-on” vs. “nitrogen-off” experiments. On P1, L10 it is suggested this could mean “with and without nitrogen cycles”, but for describing your ensemble on P1, L29 “MAGcnoff” means nitrogen does not constrain carbon dynamics. Sometimes the terminology “disabling nitrogen” is also used. I would try to stick to one way of describing your experiments, and to use this consistently in text.

GT: Thank you for the comment. In our definition, “nitrogen-on” versus “nitrogen-off” (also described as disabling nitrogen or nitrogen not constraining carbon dynamics) refers to configurations within the same model, using the same parameterizations, where carbon–nitrogen coupling is either enabled or disabled. This provides the cleanest way to diagnose the role of nitrogen in the carbon cycle because it isolates the nitrogen effect without introducing differences in model structure.

However, current ESM studies do not perform “nitrogen-on vs. nitrogen-off” experiments within the same model. Instead, analyses rely on comparisons between ESMs with and without nitrogen cycles (CN-coupled and C-only ESMs) to infer the role of nitrogen. As noted in the manuscript:

The first challenge is ***partly addressed*** *by Earth system models (ESMs), which explicitly couple the carbon-nitrogen cycle with the climate system 10. For example, comparisons of ESMs with and without nitrogen cycles in the Coupled Model Intercomparison Project Phase 6 (CMIP6) indicate that including nitrogen improves the simulation of carbon cycle dynamics 11, 12.* ***Yet, such comparisons do not isolate nitrogen effects within the same ESM, conflating differences in model structure with nitrogen inclusion.***

In this study, our primary focus is the comparison between MAGcn and MAGcnoff, which quantifies the “pure” nitrogen effect independent of model structural differences — an aspect not previously quantified. We also compare MAGc and MAGcnoff to assess whether structural differences between ESMs with and without nitrogen representations influence the results (i.e., whether MAGcnoff behaves similarly to MAGc or how different they are).

To clarify this terminology, we have added the following explanation to the manuscript.

Many carbon-nitrogen coupled models have performed “nitrogen-on” versus “nitrogen-off” experiments to isolate the role of nitrogen in different processes (i.e., the same model and parameterizations, with nitrogen constraints on carbon dynamics enabled or disabled).

P2, L3: Change ‘versus’ to ‘vs.’ for consistency with the rest of the text.

GT: Thank you for the comment. We have now changed all instances to “vs.” for consistency throughout the text.

P2, L12 and L26: “nitrogen effects” can be changed to “the effect of nitrogen”

GT: Thank you for the comment. We have retained “nitrogen effects” throughout the manuscript to indicate that it encompasses the collection of impacts of nitrogen on multiple processes.

P2, L19: What is meant by “climate interactions” here?

GT: Thank you for the comment. Here, “climate interactions” refers to the two-way coupling between climate and the carbon–nitrogen cycle in ESMs, which is absent in offline models where climate is prescribed and unaffected by carbon–nitrogen dynamics. We have now made this explicit in the manuscript:

The second challenge — large inter-model spread — persists and even amplifies when climate-carbon-nitrogen interactions are represented in ESMs.

In Fig. 1, 2, and 3: the acronyms v8cn, v8cnoff, and v8c are not defined anywhere. I assume that these correspond to the ensembles MAGcn, MAGcnoff, and MAGc, but this should be made clear. In fact, I’m not even sure where the ‘v8’ comes from (MAGICCv8?), however, the explicit naming leads me to believe it is somehow important, and therefore should be explained in the Methods section. If it is not important, then it should be renamed.

GT: Thank you for the comment. We apologize for this confusion. At the very beginning we called the MAGICC-CNit MAGICCv8 to distinguish it from MAGICC v7.5.3. However there are some other updates in MAGICC that are happening right now and so for this paper we name the ensembles MAGcn, MAGcnoff, and MAGc. We actually have all the updated figures but I forgot to replace them before submission (I replaced the texted but mistakenly left the figures). Now all the figures are updated, including the extended data figures. Thanks again for bringing this out.

P3, L9-10 and P4, L3-4: Should be clarified that the ranges provided here are for the different emission scenarios (range SSP1-2.6 to SSP5-8.5).

GT: Thank you for the comment. We have now clarified that the ranges refer to different emission scenarios by adding “across these scenarios”:

Across these scenarios, the medians exceed those of the MAGcnoff ensemble by 0.16–0.25 °C and those of the MAGc ensemble by 0.09–0.16 °C.

Across these scenarios, the medians are higher than MAGcnoff and MAGc by 15.32-57.04 ppm and 10.72-58.09 ppm, respectively.

P3, L10: Full distributions of what? Generally, this sentence can be written in a much more precise way. At the very least, I would clarify you mean GSAT anomalies, and reference the figure subplot.

P4, L4-7: Similar to above, these sentences can also be made much more precise by explicitly stating the 5th and 95th percentiles are for atmospheric CO2 concentration (again with reference to the figure subplots).

GT: Thank you for the comments. We have revised the text for clarity. GSAT and CO_2 are described in separate paragraphs, with the corresponding subplot references provided at the beginning of each paragraph. Repeating the references in every sentence is unnecessary and was avoided to reduce repetition while maintaining focus on ensemble differences.

The full distributions of GSAT anomalies — including 5th and 95th percentiles and the minimum and maximum — also shift upward from MAGc and MAGcnoff to MAGcn.

The 5th and 95th percentiles of CO_2 concentrations also shift upward, and ensemble distributions differ notably.

P4, L10: Should be clarified as the "end-of-century" decadal mean for cNetLand.

GT: Thank you for the comment. We have now made it clear.

Under SSP5-8.5, the MAGcn ensemble projects an end-of-central decadal mean cNetLand of 3.0 ± 2.3 GtC yr^{-1}, less than half of the 6.5 ± 3.9 GtC yr^{-1} in MAGcnoff (Fig. 2).

P4, L15-17: The "increases from" is misleading here. You mean the end-of-century decadal mean increases 'by' 58.1-76.2 GtC/yr in the MAGcn ensemble, with higher emission scenarios on the upper end of this range. Please also direct the reader to Fig. 2 in this sentence, and make it clear that by "~1–6 GtC/yr" and "~12–25 GtC/yr" you mean something like the MAGcnoff ensemble is 1–6 GtC/yr larger than the MAGcn ensemble, and the MAGc ensemble is 12–25 GtC/yr larger than the MAGcn ensemble, depending on the emission scenario,

GT: Thank you for the comment. Our original phrasing — "Examining cNetLand components, net primary production (NPP) increases from 58.1 to 76.2 GtC yr^{-1} across SSP1-2.6 to SSP5-8.5 in the MAGcn ensemble" — is accurate. Here, "increases from 58.1 to 76.2 GtC yr^{-1}" indicates the values for SSP1-2.6 and SSP5-8.5, not a change "by" this amount. These values are slightly lower than MAGcnoff by ~1–6 GtC yr^{-1} and markedly lower than MAGc by ~12–25 GtC yr^{-1}, depending on the emission scenario. To make this explicit, we have revised the sentence to:

Examining cNetLand components, net primary production (NPP) in the MAGcn ensemble ranges from 58.1 to 76.2 GtC yr^{-1} across SSP1-2.6 to SSP5-8.5 (Fig. 2). These values are slightly lower than those in MAGcnoff by ~1–6 GtC yr^{-1} and markedly lower than those in MAGc by ~12–25 GtC yr^{-1}, depending on the emission scenario (Extended Data Fig. 2).

P5, L1: I would clarify that the reduced standard deviations are in NPP.

P5, L3-5: "Means" should be clarified as the end-of-century decadal mean, and "56.0 ± 12.3 to 71.0 ± 16.1 GtC/yr in MAGcn" should be clarified as the range across the different emission scenarios.

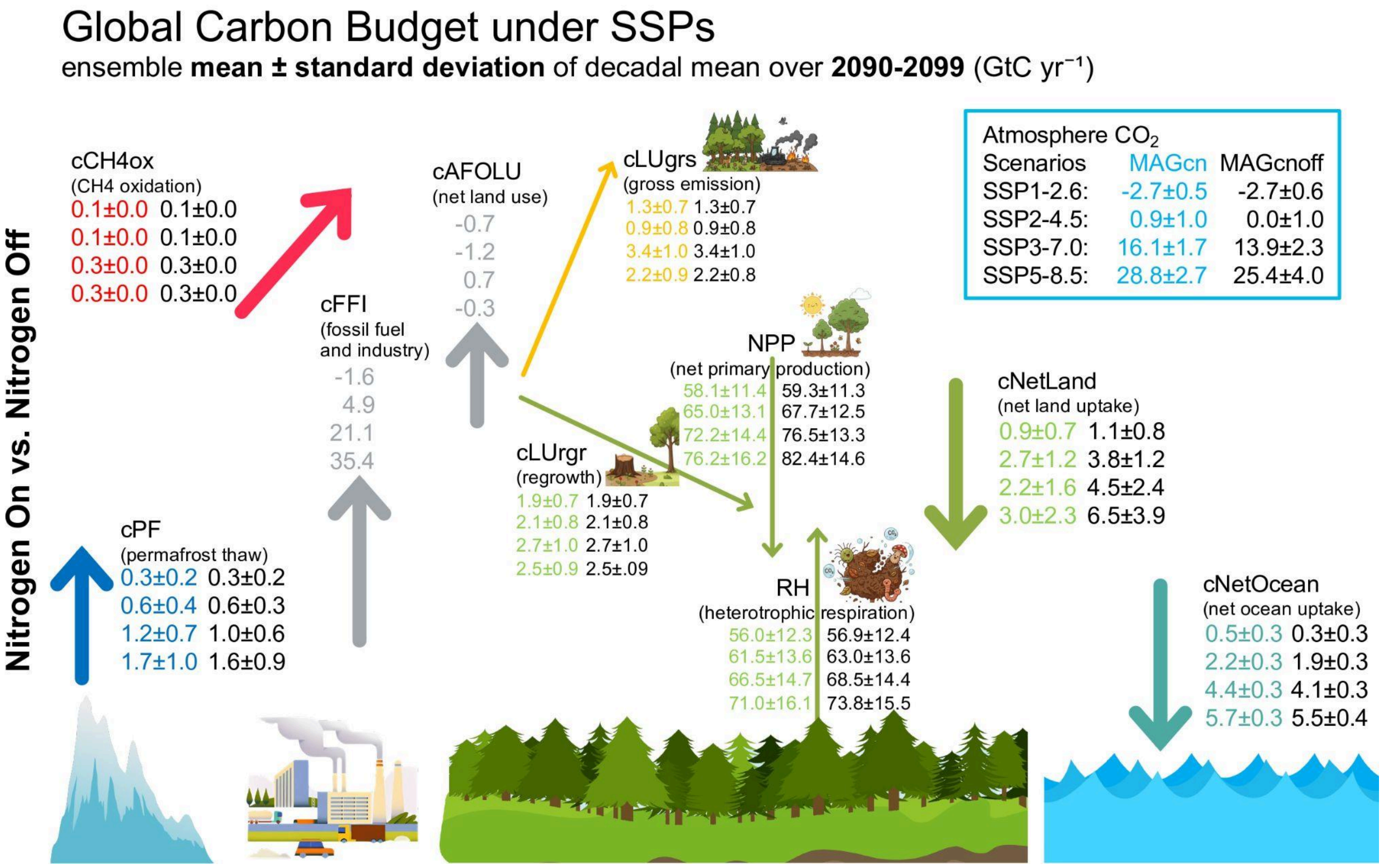
GT: Thank you for the comments. We have clarified these points as follows:

The MAGcn ensemble also shows substantially reduced standard deviations of NPP relative to MAGc,

Heterotrophic respiration (RH) shows a similar pattern, with smaller differences in end-of-central decadal means and comparable spreads (e.g., 56.0 ± 12.3 to 71.0 ± 16.1 GtC yr^{-1} in MAGcn vs. 56.9 ± 12.4 to 73.8 ± 15.5 GtC yr^{-1} in MAGcnoff across SSP1-2.6 to SSP5-8.5).

In Fig. 2 (and similar ones in the Extended Data section): An acronym is provided for different components of the budget, along with a long name. For consistency, the long name for cNetLand and cNetOcean should also be given.

GT: Thank you for the comment. The figures have been updated.



In Fig. 3, no '(a)' and '(b)' label in the subplots; missing figure description for (a) and (b). What do the black dot and whiskers mean here? It also should be stated here that these are dedicated experiments, and the mention of "1pctCO2-bgc" and "1pctCO2" should direct the reader to the Methods section, as it is not described in the main body of the article.

GT: Thank you for the comment. The figure and figure captions have been updated.

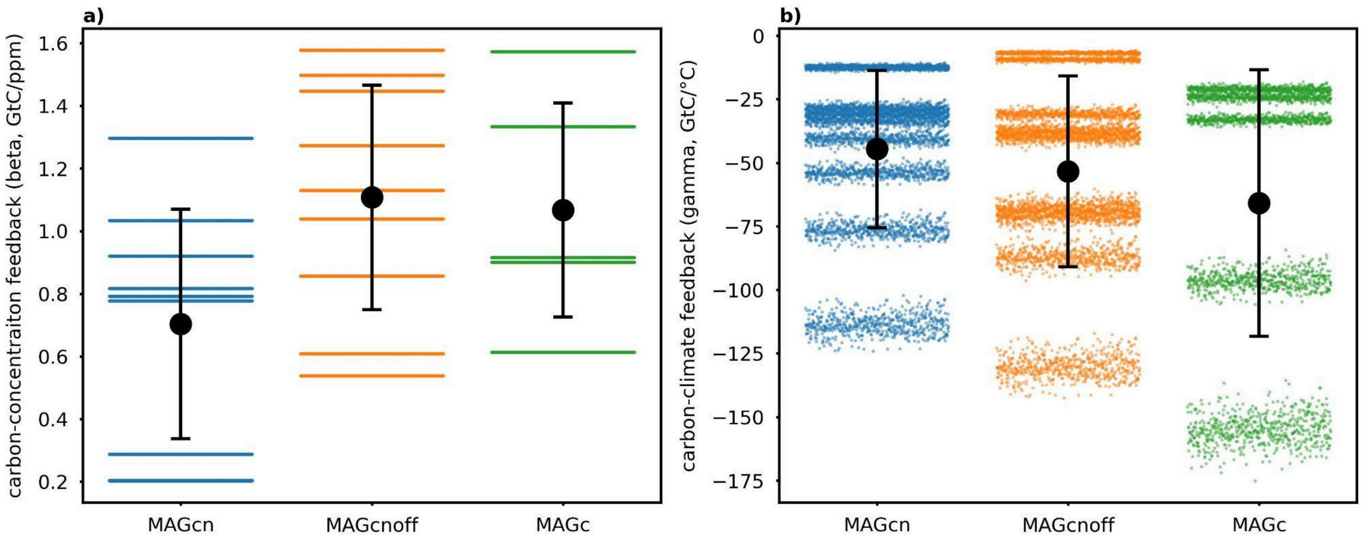


Figure 3 Land carbon–concentration (panel a) and carbon–climate (panel b) feedbacks derived from the MAGICC-CNit MAGcn, MAGcnoff, and MAGc ensembles, diagnosed from 1pctCO2 and 1pctCO2-bgc experiments (see Methods). In MAGICC, temperature change is simulated as a function of greenhouse gas concentrations through the emission–concentration–radiation–climate cascade 21. In 1pctCO2-bgc, the

radiation code is unresponsive to CO_2, resulting in no simulated temperature change. Consequently, carbon–concentration feedbacks appear as discrete values, each corresponding to an emulated CMIP6 ESM (see Methods). In contrast, carbon–climate feedbacks are continuous, as each carbon-cycle parameter set is paired with MAGICC's ensemble of climate-response parameters, generating a spread in temperature outcomes for the same CO_2 forcing and representing uncertainty in climate sensitivity.

P5, L18-19: Authors should reference Fig. 3"(a)" and specify ensemble mean is meant here.

P6, L4-5: Please specify that these numbers refer to the ensemble mean here.

GT: Thank you for the comment. We have added the figure reference.

The carbon–concentration feedback is 0.70 ± 0.37 GtC ppm^{-1} in MAGcn, 1.11 ± 0.36 GtC ppm^{-1} in MAGcnoff, and 1.07 ± 0.34 GtC ppm^{-1} in MAGc (ensemble mean ± standard deviation; Fig. 3a; Table 1).

The carbon–climate feedback is weakest in MAGcn (–44.62 ± 30.88 GtC °C^{-1}, ensemble mean ± standard deviation; Fig. 3b; Table 1), followed by MAGcnoff (–53.35 ± 37.53 GtC °C^{-1}) and MAGc (–65.86 ± 52.43 GtC °C^{-1}).

P5-6, L19-22: I don't understand "The strongest feedback (1.57 GtC/ppm) occurs in CNit(BCC-CSM2-MR), a C-only ESM" but then also "The MAGcnoff maximum (1.58 GtC/ppm) arises from the nitrogen-off mode of 6 CNit(UKESM1-0-LL)". How is the strongest feedback 1.57 GtC/ppm, but the maximum of all ensembles 1.58 GtC/ppm? I believe you are trying to compare the MAGcn and MAGc ensemble mean to the MAGcnoff ensemble mean here, but this comparison should be written more explicitly.

GT: Thank you for the comment. To clarify, MAGc and MAGcn are direct emulations of ESMs, while MAGcnoff is the diagnostic nitrogen-off configuration derived from MAGcn. In our original wording, "The strongest feedback (1.57 GtC ppm^{-1})" refers to all the emulated ESMs (MAGc and MAGcn), whereas "The MAGcnoff maximum (1.58 GtC ppm^{-1})" refers to the MAGcnoff ensemble. We have now revised the text for clarity while keeping this distinction:

Across all emulated ESMs (combining MAGcn and MAGc), the strongest feedback (1.57 GtC ppm^{-1}) occurs in CNit(BCC-CSM2-MR), a C-only ESM, while the weakest (0.20 GtC ppm^{-1}) appears in CNit(CMCC-CM2-SR5) and CNit(CMCC-ESM2), both CN-coupled models.

Table 1: I suppose this table is meant to list the models in alphabetical order, so 'CNit(CESM2-WACCM)' can be moved up a few rows.

GT: Thank you for the comment. The original sequence followed our earlier calibration order, but arranging the models alphabetically may improve readability. We have now updated Table 1 accordingly.

P7, L12-14: In Fig. 1, the differences in the ensembles only minimally change the timing of when 1.5 °C is reached, and it is more strongly affected by the given SSP scenario. I would instead focus on a 2 °C GSAT anomaly, as nitrogen feedbacks seem to more significantly change what year this is reached for, e.g., SSP2-4.5 (see Fig. 1a).

GT: Thank you for the comment. We did not intend to focus on the 1.5 °C threshold specifically. Rather, our discussion addresses peak warming and its timing, which are of particular relevance for mitigation research.

P8, L5: DGVM not defined.

GT: Thank you for the comment. Since the term appears only once, we have replaced the abbreviation with its full name, dynamic global vegetation model.

Why does nitrogen not amplify the carbon–climate feedback in CNit(CMCC-CM2-SR5), CNit(CMCC-ESM2), and CNit(CESM2-WACCM)? Are there structural reasons for this difference compared to the other ensemble members (e.g., climate sensitivity, a certain parameter, or pool)? I found this particularly interesting, as disabling nitrogen cycling strengthens the other considered feedback in all cases.

GT: Thank you for the comment. The carbon-concentration (beta) and carbon-climate (gamma) feedbacks are calculated as follows:

beta = delta_cland_bgc / delta_co2

gamma = (delta_cland - delta_cland_bgc) / delta_dt

Disabling nitrogen consistently increases land carbon accumulation (i.e., delta_cland_bgc_Noff > delta_cland_bgc_Non). Because net primary production (NPP) is generally more nitrogen-limited than heterotrophic respiration (RH), removing this limitation allows the CO2-induced enhancement of NPP to significantly outpace RH. This widening gap leads to a stronger CO2 fertilization effect and a larger beta.

Disabling nitrogen can affect gamma in both directions. This occurs because disabling nitrogen mixes climate and nitrogen effects, alongside the legacy effects from increased NPP (more carbon inputs alter carbon turnover rates). Effectively, the outcome is determined by the relative increases in RH and NPP i.e., (RH_Noff - RH_Non) vs. (NPP_Noff - NPP_Non).

In all cases, when disabling nitrogen, NPP increases (making more carbon available for decomposition), and warming enhances respiration. The divergence in gamma depends on the baseline respiration response:

> More negative gamma: If respiration is primarily limited by nitrogen, disabling it leads to further increased respiration. All these factors together increase the respiration difference, ultimately leading to a situation where (RH_Noff - RH_Non) > (NPP_Noff - NPP_Non), and thus a more negative gamma.
>
> Less negative gamma: However, if respiration is not very limited by nitrogen, disabling it actually offsets the respiration increases driven by warming and the added substrate. This can lead to a situation where (RH_Noff - RH_Non) < (NPP_Noff - NPP_Non), and thus a less negative gamma.

The latter happens in CNit(CMCC-CM2-SR5), CNit(CMCC-ESM2), and CNit(CESM2-WACCM). Our previous ESM emulation [Tang et al., 2026] shows that their respiration response to nitrogen is different from other models. These three models show a strong nitrogen enhancement of recalcitrant soil organic matter decomposition (cSR), alongside a strong nitrogen limitation on litter and labile soil organic matter decomposition (cLD). When disabling nitrogen, cLD increases while cSR does not. This leaves the total RH change smaller than the NPP change (i.e., resulting in more carbon storage in the Noff experiment) and leads to the less negative gamma.

This is indeed an interesting point we have missed before. Actually now looking into Table 1, the same explanation applies to the marginally enhanced carbon-climate feedback in the two NorESM models. In the NorESM cases, disabling nitrogen does curb the baseline soil respiration rate due to that same lack of nitrogen enhancement. However, this suppression is ultimately overwhelmed by a massive surge in litter production (i.e., leaf dropping) once its severe nitrogen limitation is removed. This immense influx of fresh litter substrate drives total respiration just enough to slightly outpace the NPP gains, resulting in a small but negative shift in the feedback.

We have now added discussion on this (quoted below). Thank you.

The enhanced carbon-climate feedback from MAGcn to MAGcnoff reflects that warming and increased substrate trigger a respiration surge that outpaces NPP gains from disabling nitrogen limitation. Conversely, the weakened feedback seen in CNit(CMCC-CM2-SR5), CNit(CMCC-ESM2), and CNit(CESM2-WACCM) results from the lack of strong nitrogen-driven enhancement on soil respiration (i.e., recalcitrant soil organic matter decomposition) when nitrogen is disabled. This dynamic also explains the marginally enhanced feedback in the two NorESM models, where the dampening of soil respiration is slightly outweighed by a substantial surge in litter production (i.e., providing more substrate for decomposition) once its nitrogen limitation is removed. Ultimately, this bi-directional shift in carbon-climate feedback strength highlights the substantial uncertainty inherent to representations and parameterizations of the nitrogen cycle and carbon-nitrogen coupling in current ESMs 9, 10, 18, 34, 35.

P8: The acronym GCB was previously introduced as GCB2024.

GT: Thank you for the comment. While GCB2024 refers to estimates from a specific year, here we are discussing GCB estimates more generally. We have revised the text accordingly:

This self-consistency enables integration of information from the GCB estimates, CMIP assessments, and observations (not shown here but representing the logical next step for constructing a Bayesian ensemble) without violating fundamentals such as mass conservation, which can be difficult when combining results from different models (e.g., the mass imbalance in the GCB estimates).

P10: The authors state that the MAGICC-CNit v2.0 ensemble is run in an emissions-driven mode. Some more text should be added to describe the experimental set-up in greater detail, particularly for readers who are not modellers, or familiar with differences between MAGICC and ESMs. Specifically, it would be useful to clarify what exactly is prescribed to the model. For example, are only CO2 (and CH4) emissions prescribed? Is N2O provided as a concentration, like other models? This was difficult for me to decipher, as in Tang et al. (2026) it was stated that: “CNit v1.0 uses net land use emissions (for both carbon and nitrogen) as input. However, this is not always the case, such as when gross emissions are provided instead of net emissions in certain scenarios.”

GT: Thank you for the comment. In MAGICC-CNit, “emissions-driven” means that all 43 simulated greenhouse gases — including CO_2, CH_4, N_2O, and others — are prescribed as emissions, and the concentrations and radiative forcings are simulated accordingly. These emissions cover multiple sectors, such as agriculture, industry, and land use. CNit is formulated to accept both net land-use emissions and gross emissions for broader applications; however, gross emissions are generally unavailable for most of the scenarios. For the four Tier-1 SSPs analyzed here, net emissions are available and being used. We have now added these details in the Methods to clarify the experimental setup.

These simulations incorporate full emissions for 43 greenhouse gases (e.g., CO2, CH4, and N2O; using both total and sector-specific emissions where available), based on data from the SSP Public Database Version 2.0 (https://tntcat.iiasa.ac.at/SspDb, last accessed 22 June 2025) 22, 44.

P10, L2-5: Is there a technical reason why you change between "model", "module", and "emulator"?

GT: Thank you for the comment. The terminology reflects the functional role of each component within MAGICC. The sea level component is implemented as a stand-alone physical model (previously referred to as the "sea level model"; Nauels et al., 2017), the permafrost component is a process module within the carbon cycle framework, and the Arctic sea-ice component is an emulator designed to reproduce the behaviour of more complex sea-ice models (Chilcott et al., 2025). We have retained these terms to reflect their intended roles and to remain consistent with the original model descriptions.

Nauels, A., Meinshausen, M., Mengel, M., Lorbacher, K., and Wigley, T. M. L.: Synthesizing long-term sea level rise projections – the MAGICC sea level model v2.0, Geosci. Model Dev., 10, 2495–2524, https://doi.org/10.5194/gmd-10-2495-2017, 2017.

Chilcott, S. M., Meinshausen, M., and Notz, D.: SASIEv.1: a framework for seasonal and multi-centennial Arctic sea ice emulation, Geosci. Model Dev., 18, 4965–4982, https://doi.org/10.5194/gmd-18-4965-2025, 2025.

You start by talking about MAGICC6 (P10, L2), before establishing that you use MAGICC7 (P10, L3 and L14), while also stating that its actually MAGICC v7.5.3 (P10, L14 and L18), which is sometimes introduced as MAGv7 (P10, L19). Later, you forgo the '7' and MAGv7 convention entirely, and use MAGICC-CNit v2.0 (P10, L20) to discuss the model that's used in this work. At later points, this v2.0 is dropped so that you only refer to it as MAGICC-CNit (P10, L29-30). On top of that, the experimental ensembles also forgo the established MAGv7 convention, and use the names MAGc, MAGcn, and MAGcnoff. As is, it is a bit difficult to read, and to understand when we are discussing a different model, versus a different modelling set-up. At the very least, I recommend to stick to one naming convention for the model used in the paper (e.g., MAGICC7-CNit v2.0).

GT: Thank you for the comment and suggestion. The different MAGICC labels reflect distinct aspects of the model: MAGICC6, MAGICC7, etc. refer to general model generations, while MAGICC v7.5.3 is the specific software version used in this study. To distinguish the model version and the ensemble, we adopt the MAGxxx for the specific ensembles: for example, MAGv7 refers to the ensemble with parameter sets from MAGICC v7.5.3. MAGcn, MAGcnoff, and MAGc are ensembles with parameter sets from MAGICC v7.5.3-CNit v2.0.

We have now revised the text to consistently use MAGICC-CNit when referring to the updated model throughout the manuscript. The introduction now reads:

With the latest version of MAGICC (MAGICC v7.5.3 with CNit v2.0, hereafter MAGICC-CNit), …

P10: "—a key advancement over CMIP5" I don't think this is necessary as it has already been established earlier in the sentence, and we are quickly moving towards CMIP7.

GT: Thank you for the comment. It is now removed.

P10, L25: "one using C-only ESM emulations… and another using CN-coupled ESM calibrations" Is there a technical reason why the word 'emulation' is used for one ensemble, but 'calibration' for another?

GT: Thank you for the comment. There is no technical distinction intended between the two terms in this context. For clarity, we have now changed both to “emulations”.

P11, L14: Typo: “quadruped” should be “quadrupled”. It might be helpful to state in the Methods that results for these dedicated experiments are evaluated at year 140.

GT: Thanks for the comment. The typo has been corrected and the 140-year simulation is now specified.

… both of which are driven by prescribed CO2 concentration with 1% increment every year until quadrupled (i.e., a 140-yr simulation) and …

Extended data is not included in the Communications Earth & Environment article format, so I leave it to the authors to decide what should be moved to the supplementary.

GT: Thank you for the comment. We prefer to keep the current format for now and will adjust the placement of extended data to the supplementary material at a later stage if needed.

P16, Extended Data Fig. 5: Caption should be made more precise. I realize ‘warming’ is given a definition on P3, L2, but I would write out here that “projected peak warming” means maximum GSAT anomaly relative to the mean of years 1850–1900.

GT: Thanks for the comment. Now it is revised to make it explicit.

Extended Data Figure 6 Projected peak warming (i.e., the maximum GSAT anomaly relative to the 1850–1900 mean) and its timing under SSP1-2.6 from the MAGcn and MAGcnoff ensembles.

References:

Sanderson, B. M., Booth, B. B. B., Dunne, J., Eyring, V., Fisher, R. A., Friedlingstein, P., Gidden, M. J., Hajima, T., Jones, C. D., Jones, C. G., King, A., Koven, C. D., Lawrence, D. M., Lowe, J., Mengis, N., Peters, G. P., Rogelj, J., Smith, C., Snyder, A. C., Simpson, I. R., Swann, A. L. S., Tebaldi, C., Ilyina, T., Schleussner, C.-F., Séférian, R., Samset, B. H., van Vuuren, D., and Zaehle, S.: The need for carbon-emissions-driven climate projections in CMIP7, Geosci. Model Dev., 17, 8141–8172, https://doi.org/10.5194/gmd-17-8141-2024, 2024.

Tang, G., Nicholls, Z., Norton, A., Zaehle, S., and Meinshausen, M.: Synthesizing global carbon–nitrogen coupling effects – the MAGICC coupled carbon–nitrogen cycle model v1.0, Geosci. Model Dev., 18, 2193–2230, https://doi.org/10.5194/gmd-18-2193-2025, 2025.

Tang, G., Zaehle, S., Nicholls, Z., Norton, A., Ziehn, T., & Meinshausen, M.: Understanding the drivers of carbon–nitrogen cycle variability in CMIP6 ESMs with MAGICC CNit v2.0: Model and calibration updates, Journal of Advances in Modeling Earth Systems, 18, e2025MS005270. https://doi.org/10.1029/2025MS005270, 2026.

Reviewer #3 (Remarks to the Author):

The paper under review presents a novel analysis regarding the use of the MAGICC RCM. By incorporating the CNit submodule it emulates the behavior, in an emission driven configuration, of 14 ESMs for six SSPs and two idealized scenarios, with the aim of understanding the role and impact of Nitrogen limitation for global climate and carbon cycle. In doing this, the paper structure is very well defined, the results are presented in a precise, clean and concise manner and are well understandable. In my opinion, this paper is useful to the scientific community as it summarizes the main impacts that the implementation of Nitrogen limitation has in CMIP6 ESMs, most importantly by isolating Nitrogen effect (CNit and CNoff runs) and performing simulations that allow for the nutrient direct feedback on atmospheric CO2 concentrations (emission-driven runs).

GT: Thank you for reviewing our manuscript and for the positive feedback on its clarity and contribution. We appreciate your constructive comments very much. Please see our detailed responses and corresponding revisions below.

Probably, it is a bit unclear to me what it is the main claim of the paper. Is it a synthesis of carbon-nitrogen status in ESMs for SSPs type of paper, or rather an analysis aiming to stress the importance of emulators in performing simulations compared to ESMs? What is the real novelty of this analysis? For example, it is already well known that Nitrogen influences climate and carbon cycle by limiting vegetation productivity, as reported in the manuscript (1,2,3). Additionally, the authors have recently published the methodological paper of MAGICC-CNitv2.0 (4), which presents and thoroughly examines C-N cycles variability in CMIP6 ESMs. To conclude, please try to stress the added scientific step that this work has done.

GT: Thank you for the comment. The main purpose of this study is to quantify the effects of nitrogen limitation on future climate and carbon cycle projections. Specifically, we address two key questions that have not been previously reported: a) the behavior of emission-driven, fully coupled future climate–carbon–nitrogen projections, which provide more realistic scenarios but have not been explored in CMIP6 ESMs; and b) the projection uncertainty obtained by synthesizing CMIP6 ESMs through a cross-ESM probabilistic ensemble.

While emulator-based projections are important and discussed briefly in the manuscript and more extensively in the Supporting Information, the focus of this work is on the nitrogen-driven impacts, rather than the emulator methodology itself.

This study differs from our previous work, which was limited to its calibration to concentration-driven CMIP6 ESM runs. Previous work can synthesize “what are the nitrogen effects in ESMs” but cannot determine “what is the impact for the future, particularly under fully coupled, emission-driven scenarios with climate–carbon–nitrogen interactions.”

To highlight the novelty, we have added the following to the last paragraph of the Introduction:

Our recent work partially resolves both challenges: using CNit, a unified framework with demonstrated emulation performance for the majority of CMIP6 ESMs, we have synthesized and quantified the magnitude and uncertainty of nitrogen effects represented in state-of-the-art ESMs 19, 20. Yet, the critical next step — quantifying how these nitrogen constraints dynamically interact with the climate and carbon cycle within fully coupled, emission-driven projections — remains unexplored. To bridge this gap, …

That said, I have just one point I am curious about. While it is explained that Nitrogen primarily limits CO2 fertilization and thus the carbon-concentration feedback, it is not clear why MAGcn ensemble has a lower (16%, in absolute terms) carbon-climate feedback compared to MAGcnoff. Could you try to elaborate why Nitrogen could decrease the (absolute) value of gamma?

GT: Thank you for the comment. The carbon-concentration (beta) and carbon-climate (gamma) feedbacks are calculated as follows:

beta = delta_cland_bgc / delta_co2

gamma = (delta_cland - delta_cland_bgc) / delta_dt

Disabling nitrogen consistently increases land carbon accumulation (i.e., delta_cland_bgc_Noff > delta_cland_bgc_Non). Because net primary production (NPP) is generally more nitrogen-limited than heterotrophic respiration (RH), removing this limitation allows the CO2-induced enhancement of NPP to significantly outpace RH. This widening gap leads to a stronger CO2 fertilization effect and a larger beta.

Disabling nitrogen can affect gamma in both directions. This occurs because disabling nitrogen mixes climate and nitrogen effects, alongside the legacy effects from increased NPP (more carbon inputs alter carbon turnover rates). Effectively, the outcome is determined by the relative increases in RH and NPP i.e., (RH_Noff - RH_Non) vs. (NPP_Noff - NPP_Non).

In all cases, when disabling nitrogen, NPP increases (making more carbon available for decomposition), and warming enhances respiration. The divergence in gamma depends on the baseline respiration response:

More negative gamma: If respiration is primarily limited by nitrogen, disabling it leads to further increased respiration. Along with the NPP surge and warming, this further increases the respiration difference, ultimately leading to a situation where (RH_Noff - RH_Non) > (NPP_Noff - NPP_Non), and thus a more negative gamma.

Less negative gamma: However, if respiration is not very limited by nitrogen, disabling it actually offsets the respiration increases driven by warming and the added substrate. This can lead to a situation where (RH_Noff - RH_Non) < (NPP_Noff - NPP_Non), and thus a less negative gamma.

The more negative gamma occurs in most cases (and in the multi-model mean) because most models exhibit nitrogen limitation on both NPP and respiration [Tang et al., 2026]. When this limitation is removed, both NPP and respiration increase. At the same time, the higher NPP provides additional substrate for respiration, further enhancing it. Consequently, the land loses more carbon when nitrogen is turned off, resulting in a more negative gamma.

What's interesting is the less negative gamma. This happens in CNit(CMCC-CM2-SR5), CNit(CMCC-ESM2), and CNit(CESM2-WACCM). Our previous ESM emulation [Tang et al., 2026] shows that their respiration response to nitrogen is different from other models. These three models show a strong nitrogen enhancement of recalcitrant soil organic matter decomposition (cSR), alongside a strong nitrogen limitation on litter and labile soil organic matter decomposition (cLD). When disabling nitrogen, cLD increases while cSR does not. This leaves the total RH change smaller than the NPP change (i.e., resulting in more carbon storage in the Noff experiment) and leads to the less negative gamma.

This is indeed an interesting point we have missed before. Actually now looking into Table 1, the same explanation applies to the marginally enhanced carbon-climate feedback in the two NorESM models. In the NorESM cases, disabling nitrogen does curb the baseline soil respiration rate due to that same lack of nitrogen enhancement. However, this suppression is ultimately overwhelmed by a massive surge in litter production (i.e., leaf dropping) once its severe nitrogen limitation is removed. This immense influx of fresh litter substrate drives total respiration just enough to slightly outpace the NPP gains, resulting in a small but negative shift in the feedback.

We have now added discussion on this (quoted below). Thank you.

The enhanced carbon-climate feedback from MAGcn to MAGcnoff reflects that warming and increased substrate trigger a respiration surge that outpaces NPP gains from disabling nitrogen limitation. Conversely, the weakened feedback seen in CNit(CMCC-CM2-SR5), CNit(CMCC-ESM2), and CNit(CESM2-WACCM) results from the lack of strong nitrogen-driven enhancement on soil respiration (i.e., recalcitrant soil organic matter decomposition) when nitrogen is disabled. This dynamic also explains the marginally enhanced feedback in the two NorESM models, where the dampening of soil respiration is slightly outweighed by a substantial surge in litter production (i.e., providing more substrate for decomposition) once its nitrogen limitation is removed. Ultimately, this bi-directional shift in carbon-climate feedback strength highlights the substantial uncertainty inherent to representations and parameterizations of the nitrogen cycle and carbon-nitrogen coupling in current ESMs 9, 10, 18, 34, 35.

Finally, some minor points that I think may improve the quality of the manuscript. Please find them listed below:

Please insert "Introduction" as a title section. Also, it would be easier to have line numbers at this revision stage for simplifying the review process and pointing to specific sentences. Not a problem, just a suggestion!

Please insert letters a) and b) for the panels in Figure 3.

GT: Thank you for the comment. We have added a heading for the Introduction section. Figure 3 and its caption has been updated. However, in accordance with the journal's formatting requirements, line numbers have not been included.

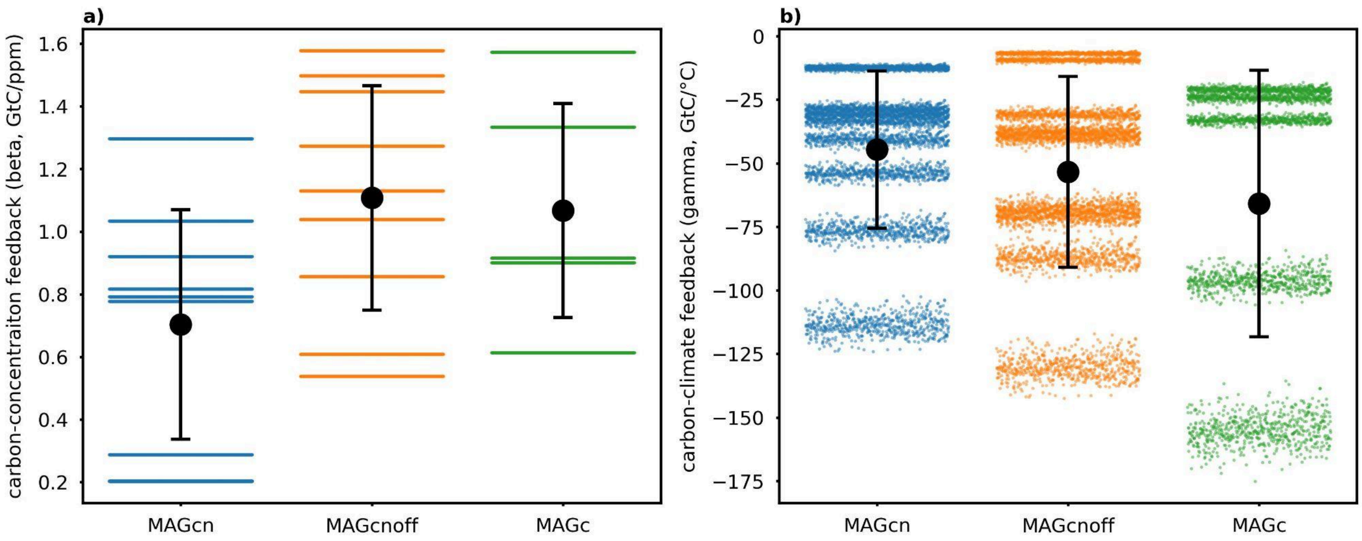


Figure 3 Land carbon–concentration (panel a) and carbon–climate (panel b) feedbacks derived from the MAGICC-CNit MAGcn, MAGcnoff, and MAGc ensembles, diagnosed from 1pctCO2 and 1pctCO2-bgc experiments (see Methods). In MAGICC, temperature change is simulated as a function of greenhouse gas concentrations through the emission–concentration–radiation–climate cascade 21. In 1pctCO2-bgc, the radiation code is unresponsive to CO_2, resulting in no simulated temperature change. Consequently, carbon–concentration feedbacks appear as discrete values, each corresponding to an emulated CMIP6 ESM (see Methods). In contrast, carbon–climate feedbacks are continuous, as each carbon-cycle

parameter set is paired with MAGICC's ensemble of climate-response parameters, generating a spread in temperature outcomes for the same CO_2 forcing and representing uncertainty in climate sensitivity.

References:

Thornton PE, Lamarque J-F, Rosenbloom NA, Mahowald NM. Influence of carbon-nitrogen cycle coupling on land model response to CO2 fertilization and climate variability. Global Biogeochemical Cycles 2007, 21(4).

LeBauer DS, Treseder KK. Nitrogen limitation of net primary productivity in terrestrial ecosystems is globally distributed. Ecology 2008, 89(2): 371-379

Zaehle S, Friedlingstein P, Friend AD. Terrestrial nitrogen feedbacks may accelerate future climate change. Geophysical Research Letters 2010, 37(1).

Tang, G., Zaehle, S., Nicholls, Z., Norton, A., Ziehn, T., & Meinshausen, M. (2026). Understanding the drivers of carbon–nitrogen cycle variability in CMIP6 ESMs with MAGICC CNit v2. 0: Model and calibration updates. Journal of Advances in Modeling Earth Systems, 18(1).

Response to Reviewer

For Tang et al., Nitrogen limitation amplifies future warming by weakening terrestrial carbon cycle feedbacks and sink capacity

In this Response to Reviewer:

- The original referee comments are in black (directly copied from the comments).
- Our responses are in blue.
- *The text we quoted from the manuscript is in gray italics.*

We sincerely thank all referees for their constructive comments and feedback on our manuscript.

Best regards,

Gang Tang (GT, as referenced below)

on behalf of all co-authors

Correspondence: gang.tang.au@gmail.com; gang.tang@student.unimelb.edu.au; tgang@bgc-jena.mpg.de

Reviewer #1 (Remarks to the Author):

During my reading of the revised manuscript, I found that the overall objective and scope of the study became clearer. This may partly reflect increased familiarity with the manuscript, but it also suggests that my previous comments have been taken into account. Nevertheless, I still feel that the level of detail in the methods section could be improved.

GT: Thank you very much for reviewing our manuscript and the revised version. The Methods section has been updated since the previous revision, incorporating and consolidating all reviewer comments.

Reviewer #2 (Remarks to the Author):

Thank you for the opportunity to review the manuscript, “Nitrogen limitation amplifies future warming by weakening terrestrial carbon cycle feedbacks and sink capacity” by Tang et al. This is my second time reviewing this paper.

As mentioned before, I still think this is quite a timely study, and it has improved considerably since the first round of review. The authors have adequately addressed almost all remarks I raised, and I am generally satisfied with the changes. The manuscript is now much clearer, and therefore, I am happy to recommend it for publication. While I have a few minor (non-technical) comments listed below, I believe these can be addressed without the need for an additional round of review.

GT: Thank you very much for reviewing our manuscript and the revised version. All the below have been resolved. Specifically:

Page 4: Should “end-of-central” be “end-of-century”?

Page 5: Same comment as above.

GT: Yes and corrected.

Page 5, Figure 3: I am still missing an explicit statement on what the black whiskers represent here.

GT: We have revised the caption of Figure 3 as follows to improve clarity:

Figure 3 Land carbon–concentration (panel a) and carbon–climate (panel b) feedbacks derived from the MAGICC-CNit MAGcn, MAGcnoff, and MAGc ensembles, diagnosed from 1pctCO2 and 1pctCO2-bgc experiments (see Methods). The scatter points represent all ensemble members (600 climate members combined with 14 CMIP6 ESM-calibrated carbon-cycle parameter sets; see Methods). The variation in feedback strength is primarily driven by the carbon-cycle parameter sets, as reflected by the distinct clustered groups within each ensemble. Black dots and error bars indicate the ensemble mean and standard deviation. In MAGICC, temperature change is simulated as part of an emission–concentration–radiation–climate cascade 21. In the 1pctCO2-bgc configuration, the radiation scheme is decoupled from CO_2, so no temperature response is produced. As a result, carbon–concentration feedbacks take discrete values corresponding to individual emulated CMIP6 ESMs (see Methods). By contrast, carbon–climate feedbacks are continuous. Each carbon-cycle parameter set is combined with MAGICC climate-response parameters, producing a distribution of temperature responses under the same CO_2 forcing and reflecting uncertainty in climate sensitivity.

Supplementary Page 2: In your response, you stated you originally used MAGICCv8 to mean MAGICC-CNit, to distinguish it from MAGICC v7.5.3. To keep consistency with the main text, I wonder if it would make sense to forgo the MAGv8 here and replace it with MAGICC-CNit, since the v8 is not used elsewhere.

GT: In the manuscript, MAGv8 and MAGv7 refer to ensembles with specific parameter configurations, whereas MAGICC and MAGICC-CNit are models with different carbon-cycle model versions. Therefore, we should either use “the mixed MAGcn and MAGc” or introduce a new name, “MAGv8,” to refer to the ensemble. We adopt the latter for brevity.

Reviewer #3 (Remarks to the Author):

The reviewed version of the manuscript incorporates the suggestions proposed during the first round of revisions. Most importantly, it clarified two main points that were raised, regarding the explicit novelty compared to the two previous manuscripts (Tang et al., 2025, 2026), and the further explanation regarding the carbon-climate feedback values in MAGcn and MAGcnoff simulations. In my opinion, this, together with the modifications taken thanks to the other two reviewers, have improved the clarity and completeness of the work. I therefore recommend this research paper for publication.

GT: Thank you very much for reviewing our manuscript and the revised version.