

# Response to Reviewers

Land carbon response to positive, zero, and negative CO<sub>2</sub> emissions across  
Earth system models

Earth System Dynamics · Ref: EGUSPHERE-2026-1673

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We thank the reviewers for their time and effort to review our manuscript, and for their constructive comments. We have addressed all reviewer comments and think that doing so has improved the clarity of our results and argument.

Both reviewers commented on their desire to know more about how model structure may be impacting land carbon cycle behavior across this ensemble (R1.2, R2.4). Our results indicate that there are no simple answers (Figs. R2, R3, R4, R5, R6), and that this ensemble likely has too many varying degrees of freedom to make clear attributions. A more fruitful way forward for developing that knowledge would be to use controlled experiments that hold more factors constant rather than this ensemble of opportunity, such as changing structure or parameters in more controlled ways within a single ESM.

Below we addressed each individual comment. Reviewer comments appear in *italics* inside shaded boxes and our responses follow immediately.

## Reviewer 1 Comments

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**R1.1** *The paper begins by pointing out that the land has served as a carbon sink over the historical period, suggesting that increases in photosynthesis have outpaced increases in respiration. However, the future of this land carbon sink is highly uncertain, due to competing impacts of meteorological and atmospheric conditions on the individual processes, as well as potential effects from events like fires and pest/disease outbreaks.*

*The “zero emissions commitment” (ZEC) is defined as the Earth system response when emissions cease, and it is quantified as the change in global mean temperature after annual emissions reach zero. Prior ESM experiments suggest that after zero emissions are reached, the land and oceans continue to uptake CO<sub>2</sub> (leading to cooling), but ocean heat uptake slows down (leading to warming). These processes generally balance to give a ZEC of 0+/-0.3. A limitation of these previous experiments is that the emissions were different in each model, so timescales for carbon cycle responses were not consistent across models. Now in a new set of experiments, flat10MIP, the emission rates are specified ahead of time. This paper focuses on the response of the land carbon pools and fluxes to specified emissions in 10 ESMs that participated in flat10MIP.*

*This paper provides an important contribution to our understanding of the land carbon cycle response to net zero emissions, a policy-relevant concept that can help us anticipate carbon-climate feedbacks if countries keep to the ambitions of the Paris Climate Agreement. It comprehensively covers the responses during phases of positive emissions, net zero, and net negative emissions.*

*The figures are very helpful for understanding the text, as well.*

**Response:** We thank the reviewer for their constructive comments and appreciate the interest in learning more about how our analysis can inform development in soil carbon modeling.

**R1.2** *I have several minor editorial comments. My most substantial comment relates to Section 4. I believe the authors have rightly pointed out the importance of soil carbon processes as a key limiting factor in predicting Earth system responses under net zero emissions. Can any information be gleaned from the analysis of this group of models about specific directions for future research and development for soil carbon, other than the obvious broad area of better representing permafrost? Also, for tropical carbon, it was suggested that missing processes are causing a high bias in carbon storage (Lines 445-453) – is there enough evidence to suggest whether missing processes or structural representation in the soil carbon components could be biasing the results in one direction? It would be elucidating to have a brief discussion of these implications.*

**Response:** We understand that the reviewer would like us to identify and suggest pathways for developing land surface components of ESMs in order for them to make more accurate projections. The analysis that we present here is focused on the spread and emerging consensus of carbon cycle behavior under positive, zero, and negative emissions. As such, we do not have the information needed to test specific processes within these ESMs against observations. These idealized experiments are not directly comparable to observed states of Earth as they lack representation of land use change and the actual time series of forcing factors including the anthropogenic emission of CO<sub>2</sub>. Thus we are largely restricted to describing the behavior rather than assessing if that emergent behavior is accurate.

It might be possible to identify how broad categories of model structure may lead to specific outcomes. However we are not able to find any obvious relationships. This is likely due to the fact that so many factors are changing between ESMs. We find no relationship between modeled carbon cycle behavior (i.e. any of the aspects in Fig. 2) with representation of nutrients, dynamic vegetation, complexity of soil carbon representation, or representation of fire (Figs. R2, R3, R4, R5). See further discussion in response to R2.4.

In addition to model structure we investigated if the magnitude of carbon stocks or change in carbon stocks was related to the magnitude of physical climate changes, for example near surface air temperature (Fig. R6). We did not find any clear relationships with physical climate that explain variability across the ensemble of ESMs.

While we did not find any robust association between model process representation and terrestrial carbon cycle behavior across the ESMs analyzed here (Figs. R2, R3, R4, R5), we appreciate that this topic needs further discussion within the manuscript. We have added the following text to the discussion (lines 360-378).

We investigated how process representation impacted terrestrial carbon cycle behavior across these 10 ESMs by examining carbon stock changes (Fig. 3, Table A1, A2). ESMs differ in their structural representation of key processes, including nutrient limitation, dynamic vegetation, the complexity of soil carbon representation, and representation of dynamic fires (Table 1). None of the structural differences that we investigated correlate with carbon cycle behavior across this ESM ensemble. As an example, it is reasonable to assume that representation of nutrient dynamics and nutrient limitation would lead to a moderated increase in land carbon sink under very large cumulative emissions, yet

ESMs with dynamic nutrients span the full range from the largest to the smallest land sink, as well as several in between the extremes (Fig. 3a). We find the same lack of correspondence for interactive fire: fire is active in five of the ten ESMs (Table 1), yet these ESMs neither cluster together nor separate from the others in the magnitude or sign of their carbon stock changes (Fig. 3). Regarding whether the spread is smaller among ESMs with more complex soil representations: the three depth-resolved ESMs (CESM2, NorESM2-LM, GFDL-ESM4) do not show tighter agreement than the single- or few-pool models; GFDL-ESM4 and NorESM2-LM sit at opposite ends of the initial soil carbon range despite both resolving soil by depth (Table A1). However, only 10 ESMs are represented in flat10MIP, each with a single realized combination of processes and parameters. In addition to structural differences, parametric uncertainty can create a wide array of behavior within a single model (Booth et al., 2012; Kennedy et al., 2025; McNeall et al., 2024). These two factors confound the problem, leading to a multitude of possible differences across the 10 ESMs. In short, we find it likely that far too many aspects vary across these models for us to find a robust attribution of model behavior to process inclusion. Systematic investigations of the role of specific processes and parametric uncertainty within ESM simulations through controlled experimentation are needed to isolate the importance of processes and process representation in controlling terrestrial carbon cycle behavior.

**R1.3** *Lines 68-70: The wording of these two sentences is a little confusing, making it sound like ZECMIP simulations were not emissions-driven, or at least leaving it a little ambiguous. I suggest minor rewording to make it clear that they were emissions-driven.*

**Response:** These lines have been removed as part of re-writing the introduction of ZEC.

**R1.4** *Line 95ish: I understand the necessity of a paper focusing on the land carbon response, but what happens with ocean carbon in these experiments (very generally / briefly)? Is there a companion paper focusing on the ocean that can be mentioned here?*

**Response:** We appreciate that the reviewer would like a description of the ocean carbon cycle response. An in depth analysis of ocean carbon cycling in flat10MIP is under development, but not yet available to be cited. We have modified the start of this section to indicate that an analysis is forthcoming (lines 78-79): "A high-level analysis of changes in global ocean carbon budget in flat10MIP has been documented in Sanderson et al. (2025) and a detailed regional analysis is forthcoming."

**R1.5** *Methods: Clear description of the protocol, and all panels in Figure 1 are helpful! My one suggestion is to summarize the metrics described in Section 2.1 in a table or graphically – I found myself drawing boxes and arrows on the figure to distinguish the timeframes discussed.*

**Response:** We appreciate this very helpful suggestion from the reviewer. We agree that a visual depiction of the phases and timeframes discussed in the manuscript would be useful for readers. To address this we have updated the experimental design figure (panels a-f of Fig. 1, repeated here as Fig. R1) to include horizontal shaded regions with labels naming each phase that is discussed. We have also added vertical shaded regions that indicate the time periods over which discrete differences are computed to represent the change over each phase. We also added this description to the caption and within the main text (lines 104-106): "We analyze both the full time series as well as distinct

time windows, or phases, within these simulations. Horizontal shaded bars in Fig. 1a-f indicate the duration and name of each phase, and vertical shaded bars indicate the time slices used to compute differences over those phases."

**R1.6** *Line 196: Please confirm how land use was specified in all experiments – my understanding was that flat10MIP applied constant preindustrial land use, but this statement makes it sound like NASA-GISS-ES may have used something else.*

**Response:** Land use was held fixed at 1850 conditions as per the protocol outlined in Sanderson et al. (2025). We have updated the relevant text for the description of NASA-GISS-ES as well as added this clarification to other model descriptions wherever crops or land use are mentioned.

**R1.7** *Line 206: Are the land models exactly the same between these versions of CESM and Nor-ESM?*

**Response:** We thank the reviewer for pointing out that this ambiguity could be clarified. CESM2 and NorESM both use the Community Land Model. However, the versions used in each ESM are not exactly the same although they are very similar. The main differences are likely to arise in soil carbon pools. We have clarified this point in the text (lines 499-500): "The land carbon cycle in NorESM2-LM (Seland et al., 2020) is based on the Community Land Model 5 (CLM5, Lawrence et al., 2019). As such, it the land component is very similar, but not identical, to the land component of CESM2. The main differences should arise in soil carbon."

**R1.8** *Lines 364-365: Only 2 out of 4 continue this pattern to 3000 PgC. The wording "Many" is misleading.*

**Response:** We thank the reviewer for pointing out that they found this wording misleading. We have changed to wording to reflect that most (8 of 10) ESMs continue to 2000 *PgC* of emissions. This line now reads (lines 222-225): "The accumulation rate slows for a few ESMs at higher cumulative emissions (Fig. 3), but many continue accumulating carbon on land at a near-linear rate all the way to 2000 *PgC* of cumulative emissions, and for the two longest running simulations they remain near linear all the way to 3000 *PgC* of cumulative emissions."

**R1.9** *Line 478: The figure reference here is incorrect, since there's not a Figure 11.*

**Response:** We thank the reviewer for pointing out this apparent typo. This is, in fact, not a typo, but an issue with the typesetting that makes Fig. 1 subpanel "l" (as in the lowercase letter L) appear as Fig. 11. We added a space between the two to clarify for now, and note that in final typesetting this will be formatted differently.

**R1.10** *Table 1: This is a very helpful summary table. But two additional factors could be at play – how many of the models use interactive fires? (Sounds like MPI, but maybe others?) Also, what is the maximum depth of the soil C pools? The number of pools doesn't seem to summarize this info, since for example NASA-GISS ha 9 pools but only represents the top 30 cm, but it could be helpful for interpreting some of the results.*

**Response:** Interactive fires:  
Fire is active in five of the ten ESMs (CESM2, GFDL-ESM4, NorESM2-LM, CNRM-ESM2-1, and

MPI-ESM1-2-LR). We do not find that the inclusion of interactive fire corresponds to a systematic difference in carbon cycle behavior across the ensemble (Fig. R4), consistent with our broader finding that structural differences do not map cleanly onto behavior (see response to R2.4). We added a sentence noting this to the discussion.

Soil depth:

Most of the ESMs in this study do not have any concept of soil depth for carbon. In the typical century-style carbon box model the depth of soil is unrelated to carbon unless the carbon is depth resolved (this is true for 3 models). Soil carbon pools are conceptual, and represent all of the carbon going into and out of the soil, but are not physically located at a particular depth or representing a particular depth. Depth is used to calculate water and energy storage, but soil carbon pools are typically agnostic to the depth.

## Reviewer 2 Comments

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**R2.1** *The manuscript “Land carbon response to positive, zero, and negative CO<sub>2</sub> emissions across Earth system models” reports on the carbon stock and flux responses of a ten-member model ensemble forced with schematic “flat10MIP” emission trajectories. Model spread is generally substantial, but common responses can also be identified, particularly under ongoing (positive) emissions and in tropical latitudes following sustained net-negative-emission forcing. This paper is relevant to the Earth system modeling community and, although it draws on schematic scenarios, has societal implications beyond.*

*I have three general comments on (i) the scope, (ii) the model description and comparison, and (iii) accessibility.*

**Response:** We thank the reviewer their effort and appreciate that they find the manuscript relevant for the Earth system modeling community.

**R2.2** *(i) The paper’s motivation, as described in the introduction, draws on the zero emissions commitment. Its introduction also discusses previous estimates and the recent “flat10MIP” trial. However, although this is the predominant motivation, the manuscript neither refers to a companion paper that provides an updated estimate nor does it provide such an estimate itself. Because the ZEC is sort of in the graphs of Figure 1, but is nowhere discussed (unless I have completely overlooked it), this is a bit irritating when reading the paper. I would prefer that the number be computed and discussed, or that a reference to a companion paper be added ahead of publication.*

**Response:** We appreciate that the reviewer found the paragraph explaining the motivation for the flat10MIP experiments confusing in the context of this manuscript where we primarily focus on the response of land carbon pools rather than ZEC specifically. We provided this explanation as context for flat10MIP more broadly, but we see why the reviewer could find this distracting. To address this we have removed the standalone paragraphs about TCRE and ZEC, as well as changed the presentation of flat10MIP to de-emphasize the motivation for these experiments associated with estimating TCRE and ZEC.

**R2.3** (ii) *The model description (Section 2.2) confused me because it seems arbitrary that some ESMs are described in great detail (e.g., including ocean components and sea ice dynamics), while for other ESMs, the land components are the only focus. This complicates comparisons between models because it is unclear whether a model does not represent a specific process described elsewhere or whether it was simply left out of its description paragraph. I suggest restricting the model description to essential ESM parameters, soil biogeochemistry, photosynthesis, vegetation dynamics, maybe water budget, and balancing the level of detail across all descriptions. For example, one aspect that should be added to all model descriptions is the atmospheric/land model grid size. Right now, this is only the case for some of the models. If the authors decide to detail the representation of natural disturbances or the spin-up (e.g. GFDL-ESM4), they would also have to do so for the other ones. On the other hand, Farquahar et al.'s and Collatz et al.'s work are presumably the fundamental basis for parameterizing photosynthesis in every one of the models (?), so this could be summarized in a common paragraph, for example, in 2.4.*

**Response:** We appreciate that the reviewer finds the level of detail is inconsistent across the descriptions of each ESM. First, we have decided to move these descriptions to the Appendix, so that they are available for reference but not part of the main narrative of the paper.

Second, we have moved towards unifying their descriptions, including removing discussion of components other than land. There is still more detail about some of the models which is not easy to unify, as some models contain more process representation than others.

We don't find the spatial resolution of the models to be particularly relevant to the findings of this paper. Those types of simulation details can be found in Appendix A1 of Sanderson et al. (2025), which is now noted at the top of the Appendix section containing the ESM descriptions.

**R2.4** *In turn, I encourage the authors to use the additional space to discuss model differences that could have mechanistic implications for explaining the large spread in more detail. Currently, the paper does not systematically identify specific takeaways that could prompt model development, especially regarding soil carbon dynamics. Is it only permafrost that sets the models apart? Is the spread across ESM with more intricate soil components smaller than between those with less comprehensive soil models? Or is there something else that could be a pointer for future development? Which process(es) would be most relevant to investigate for reducing model spread under flat10MIP-style experiments? A suitable spot for summarizing these sorts of differences could be Sections 2.3-2.5, and this could then be picked up in the results discussion. Also, I wonder if these Sections 2.3-2.5 could be grouped under a common subtitle, "commonalities and differences between model setup," or something similar, since this is their shared (and important) theme, though they do not currently discuss it in great detail.*

**Response:** We understand that the reviewer would like to see more direct attribution between model structure and model behavior. We have investigated a number of processes that differ across this group of ESMs including nutrients, dynamic vegetation, complexity of soil carbon representation, and interactive fire. These model structural differences are described in what is now section 2.2 (formerly section 2.3).

None of the structural differences that we investigated appear to correlate with carbon cycle behavior across this ESM ensemble (Figs. R2,R3,R4,R5). This should perhaps not be surprising. We have only 10 ESMs to analyze, each with a single combination of processes and parameters. We know from studies such as (Booth et al., 2012) that parametric uncertainty can create a wide array of

behavior within a single model. As an example, it is reasonable to assume that representation of nutrient dynamics and nutrient limitation would lead to a moderated increase in land carbon sink under very large cumulative emissions, yet ESMs with dynamic nutrients span the full range from the largest to the smallest land sink, as well as several in between the extremes (Figure 3a). In short, we find it likely that far too many things vary across these models for there to be a robust attribution of model behavior to process inclusion.

While we did not find any robust association between model process representation and terrestrial carbon cycle behavior across the ESMs analyzed here, we appreciate that this topic needs further discussion within the manuscript. See further discussion and the added text in R1. 2.

**R2.5** *(iii) The figures are clean and supportive of the findings discussed in the text. However, it seems to me that most figures are not colorblind-friendly, which should be checked. The text presentation is generally okay, but I got the impression that the manuscript was written quickly and has not received much proofreading. I encourage investing additional time in proofreading to remove punctuation mistakes, redundancies, and cluttered sentences (some of which are specified below, but the list is not extensive).*

**Response:** We have carefully proofread the manuscript and corrected errors.

We appreciate that the reviewer finds our figures clean. We chose to retain the color choices as they match the colors in (Sanderson et al., 2025). For the line plots we have additionally added distinct line styles and markers so that ESMs are separable without relying on color.

**R2.6** *A question remained open for me: Partly (e.g. paragraph about GFDL-ESM4), it appeared as if land use (change) and harvest were considered a boundary condition for the simulations, which would complicate interpreting these otherwise idealized experiments. If so, model differences regarding land use and harvest would need to be clarified, and potential effects on the results discussed later. For example, it would be critical to know in which C pool harvests end up over short/medium/long term. However, land use/harvest shouldn't be different between models contributing flat10 experiments, should it?*

**Response:** These simulations do not include any transient land use change as is consistent with the flat10MIP protocol (Sanderson et al., 2025). We have unified the level of detail provided for each ESM and have clarified the description of GFDL-ESM4 (descriptions now in the Appendix).

**R2.7** *l. 3-4 Sentence could be more specific and linked to the subsequent sentence. What is the research gap that the flat10 experiments specifically fill?*

**Response:** We appreciate that the reviewer finds references to the original motivation for flat10MIP to be distracting in the context of this paper. To address this we have removed the sentence about the motivation for flat10MIP itself and reworded the following sentence as follows (lines 4-6) "Here we characterize the response of land carbon pools and fluxes from ten emissions-driven Earth system models (ESMs) under positive, net-zero, and net-negative CO<sub>2</sub> emissions using experiments from the 'flat10' model intercomparison."

**R2.8** *l. 25 Is this Friedlingstein et al. 2025, too? If so, the reference should be put here as well and not only at the end of the paragraph. Otherwise, the quoted figures need a reference.*

**Response:** We have added additional references to the Global Carbon Budget (Friedlingstein et al., 2025b) farther up in this paragraph as suggested.

**R2.9** *l. 26 Since the budget is broken down into contributions of individual compartments, it should be noted where the remaining 10% end up.*

**Response:** The remaining 10% is left unspecified as an “imbalance” in the Global Carbon Budget. To clarify this point we have reworded the sentence in question (lines 26-27) as follows: "Over the period 1959-2024 a 1 PgC/yr imbalance remains unattributed to specific fluxes in the assessed global carbon budget (Friedlingstein et al., 2025b)."

**R2.10** *l. 28 biologically? Also, “biogeochemically” might be more fitting and comprehensive*

**Response:** We have corrected this typo.

**R2.11** *l. 46-7 three times “large” - maybe there is a way to come up with a different expression?*

**Response:** We appreciate the reviewer found the wording repetitive. We have reworded this section.

**R2.12**

*l. 48 does this spread really apply to all models? Are there more recent examples from CMIP5/6 than from the CMIP3 era?*

**Response:** We are not aware of any experiments with current ESMs that are similar to Booth et al. (2012) that include emissions-driven carbon cycle responses to different assumptions about land parameters. We note that the model version from Booth et al. (2012) is very similar to one of the models in our study, HadCM3LC-Bris. Based on preliminary results that are not yet published as well as our expert opinion we expect that many current ESMs would exhibit similar within model spread in carbon cycle behavior.

To address this we have edited the sentence in question as follows (lines 47-49): "For example, projections made with the CMIP3-era HadCM3 ESM spanned ~450 ppm in atmospheric CO<sub>2</sub> in 2100 in response to modified assumptions within the land model (Booth et al., 2012), a behavior we expect would also occur in other ESMs."

**R2.13** *l. 49-53 This is a long sentence, could be split*

**Response:** We have split the sentence in question into two sentences.

**R2.14** *l. 55 The second phrase of the paragraph doesn't really fit to the remaining paragraph. It could be cut or moved to the previous paragraph.*

**Response:** We have removed the section in question.

**R2.15** *l. 67 The ZEC should have a unit.*

**Response:** We have removed the section in question.

**R2.16** *l. 82 The “intriguing behavior” should be specified.*

**Response:** We appreciate that the reviewer would like us to provide more detail about the behavior in low emissions scenarios. We have updated the text in question which now reads (lines 66-71):

Understanding the carbon cycle response to net-zero and net-negative emissions, and particularly the highly uncertain land carbon cycle response, has important implications for projecting future climate. The tendency for carbon sinks to become neutral or switch to sources under low emissions scenarios (Jones et al., 2016; Koven et al., 2022) is a key determinant of the coupled carbon-climate response to emissions, yet most model intercomparisons using emissions-driven ESMs have focused on regimes of increasing emissions, with only a limited number of simulations completed for lower or negative emissions scenarios (Asaadi et al., 2024).

**R2.17** *Intro in general: I figure it is important to specify that removals in this type of experiment are effective removals in the forcing and not made (spatially) explicit in any way in the ESMs. Spelling out CDR in an ESM may reveal different effects on (land) carbon uptake and climate-C cycle feedbacks depending on the specific method(s) being used for removals (e.g. Zickfeld et al. 2023 Nat Clim Change). Later on, I found a sentence touching on this aspect in the conclusions, but I consider it a core limitation of the experimental design that should appear in the introduction or methods.*

**Response:** We agree with the reviewer that it is useful to clarify the way in which carbon removal has been implemented in the flat10MIP protocol, however we disagree that this is necessarily a limitation of the experimental design. These are idealized experiments and the simplicity potentially enables greater understanding. Implementing specific CDR methods creates substantial additional complexity that makes the carbon cycle responses harder to interpret.

To clarify the way in which declining or negative emissions are implemented we have modified the text as follows (lines 97-100):

Declining and negative emissions within flat10MIP are implemented in the same way as positive emissions, namely that they are idealized and do not have a spatial pattern. This idealized approach enables a general understanding of the reversibility of the climate system, but does not capture the co-occurring impacts of any particular carbon dioxide removal method (Zickfeld et al., 2023).

**R2.18** *l. 104 “emissions are”*

**Response:** This typo has been corrected.

**R2.19** *Fig 1*

- 1. Indicating the time frames that are used to define the points of “net-zero emissions DeltaC” (Fig 2), “zero cumulative emissions DeltaC” (Fig 2, l. 420), etc. could help bridge between all the different panels figures.*
- 2. For me, the caption should explain the averaging timescale and method employed to obtain the thick curves compared to the presumably annual (?), unsmoothed data.*
- 3. It is obvious, but an x axis label “simulation year” or similar could be added*

4. panels c & f have a line label that is redundant with y axis labels and caption. Could be removed.
5. vertical lines indicate the timing of net-zero emissions etc, but I couldn't find it being specified in the text. Might have overlooked it, but it should be specified.

**Response:** We thank the reviewer for these comments and their suggestions for improving clarity of what is represented in our figures. In response we have made the following changes.

1. We have added vertical and horizontal shaded bars to Figure 1 (also repeated as Fig. R1) indicating the time ranges that are used for quantifying " $\Delta$  Emissions Phase" etc. See response to R1.7 above for more detail. This unifies the language across figures.
2. We modified the the caption so that the relevant sentence now reads "Time series of annual emissions (top row), cumulative emissions (second row), change in global mean atmospheric CO<sub>2</sub> (third row), and temperature (smoothed with an 11-year running mean)."
3. We added the x-label of "year" to all time series plots (it was erroneously missing due to a code error).
4. We removed the legend from the first and second rows as the reviewer found it confusing.
5. We have added the following sentence to the caption: "The vertical dashed lines indicate year the year of 1000 PgC of cumulative emissions, which corresponds to the year of net-zero emissions in esm-flat10-zec, as well as year 150 in the right hand column which is the year of net-zero emissions in esm-flat10-cdr."

**R2.20** *l. 115 Because "negative emissions" is also being used synonymously with "carbon removals", using the term "positive emissions" instead of just "emissions" here would be unambiguous*

**Response:** We have changed the phrasing in this location to "positive emissions" as suggested by the reviewer (positive emissions was already used generally throughout the manuscript).

**R2.21** *l. 115 I would specify that the "average around" refers to a time point. The sentence is quite long, and initially I got confused by the way it is phrased at the moment.*

**Response:** We have changed the phrasing to indicate an approximate time as suggested by the reviewer.

**R2.22** *l. 134 representation (no "s")*

**Response:** We have corrected this as suggested by the reviewer.

**R2.23** *Section 2.2 Is there a reason why the length of model descriptions vary widely between the different models? Are GFDL-ESM4, GISS-E2.1-G-CC2 and CNRM-ESM2-1 specifically relevant for the study? Or do these model versions just differ the most to the citeable literature?*

**Response:** Please see response to R2.3 where we address this question.

**R2.24** *l. 246 The "q10" dependence should be briefly explained and referenced.*

**Response:** We have updated the sentence as follows to specify that q10 is an exponential

dependence of respiration on temperature (lines 535-536): "Soil carbon is represented in a single pool, with an exponential “q10” dependence of decomposition on soil temperature (e.g. Lloyd and Taylor, 1994)."

**R2.25** *l. 261 I reckon “MATSIRO” should be outside brackets?*

**Response:** We intended to put the abbreviated name of the submodel within the parentheses as it is just provided for reference.

**R2.26** *l. 271 Where does the litter end up in the models that do not report cLitter? Looking at F2, in comparison to the other models, it looks like the soil pool?*

**Response:** The carbon that would typically be allocated to a litter pool is instead included within the soil carbon pool. We have reworded this section as follows (lines 121-123): "Three of the ESMs analyzed here report only vegetation and soil carbon pools (NASA-GISS-E2.1-G-CC2, UKESM1.2, HadCM3L-Bris) where litter carbon is implicitly included within soil pools."

**R2.27** *l. 275 In the table, it says “4 for each 20 layers” also for GFDL-ESM4. Is the GFDL land model similar to CLM?*

**Response:** The GFDL-ESM4 model is quite different in nearly all respects compared to the Community Land Model which comprises the land component of CESM2 and NorESM ESMs. GFDL-ESM4 does discretize their soil carbon pools by depth (Sulman et al., 2014, 2019), but otherwise the assumptions made about soil carbon differ from CLM.

To clarify this point we have added the following text (lines 132-134): "Although both GFDL-ESM4 and the ESMs with CLM5 resolve soil carbon pools by depth they remain distinctly different models with independent development history."

**R2.28** *l. 294 Missing comma: In GFDL-ESM4, plants*

**Response:** This has been corrected.

**R2.29** *Section 3.1.1 This section lacks a clear storyline and appears quickly drafted, making it hard to read. I suggest giving it another round of a makeover. Maybe a brief introductory statement could help (potentially by reordering parts of the section) and avoiding too many sentences that build on “have”.*

**Response:** We thank the reviewer for pointing out that the motivation and storyline for this section was unclear. We have re-written much of this section, in particular the first two paragraphs (starting on lin 164).

**R2.30** *l. 235-8 Is there an indication that spin-up is really the dominant reason for model spread? I would expect the models to be properly spun up before the start of the transient experiments. Otherwise, it would be really hard to draw any meaningful results, for example, from comparing positive and negative emission phases/asymmetries.*

**Response:** The ESMs should be spun up prior to starting any of the simulations analyzed here as specified by the protocol for flat10MIP specified by (Sanderson et al., 2025). However, the NorESM

team indicated that their soil carbon pools were not entirely spun up and specified a drift correction that was applied prior to analysis.

**R2.31** *l. 235-40 Does this conform with the results in the transient phases of the scenarios?*

**Response:** (We assume this comment refers to lines 335-340).

We appreciate that the reviewer would like to see if this hypothesis bears out in the results from the other phases of the experiment. As with many differences between the ESMs we do not see a clear relationship between the initial carbon stocks and the loss or gain of carbon across the 10 ESMs (Fig. R7. We have added a sentence on this point as follows and included a new figure in the appendix (also repeated here as R7) (lines 198-198): "However, across this 10 member ensemble we do not see a robust relationship between initial carbon content and amount of carbon gained or lost during different phases (Fig. A1)."

**R2.32** *Fig 2 I would maybe avoid "phase" in the caption because the three types of scenarios shown in the figure do not follow after one another chronologically.*

**Response:** We removed the word "phase" from the caption as suggested by the reviewer.

**R2.33** *l. 325-7 Something is wrong with this sentence*

**Response:** We have edited this typo. The sentence now reads (lines 182-183): "It is likely the models used here, which are largely very closely related to CMIP6 versions, exhibit a similar mix of skill across regions and processes."

**R2.34** *l. 349, 357 missing commas (consider checking throughout the manuscript): During the .. phase, ..; As cumulative emissions increase, it..*

**Response:** We have added commas as suggested by the reviewer and have also proofread the manuscript.

**R2.35** *l. 372 This is not directly obvious, so I'd suggest adding a reference.*

**Response:** We have added references to support this point as suggested by the reviewer. The lines referenced now read (lines 231-234): "nutrients are not the only constraints on vegetation growth, and GPP in the tropics in particular is limited by hot temperatures and moisture stress in combination with faster vegetation turnover (e.g. Pau et al., 2018; Green et al., 2019; Zarakas et al., 2024; Friedlingstein et al., 2025a)."

**R2.36** *l. 383-5 The second half of this sentence is confusing. Consider splitting it up.*

**Response:** We have split the sentence into two as suggested by the reviewer.

**R2.37** *l. 390-7 What should those notes tell the reader, how do they relate to the two hypotheses? I don't really get the focus/intention of this paragraph.*

**Response:** We understand that the reviewer would like further explanation of the connection

between the recent papers assessing historical land sink and our analysis of flat10 simulations. We have edited the text to address this as follows (lines 253-256):

However, the behavior of the ESMS we analyze here are relevant for interpreting estimates of land carbon sinks reported by the Global Carbon Project (Friedlingstein et al., 2025b) as the land model components of many of the ESMS participating in flat10MIP are also used to make estimates of the historical period and included in Global Carbon Project assessments.

**R2.38** *l. 395 MPI-ESM1-2-LR*

**Response:** We have corrected this typo.

**R2.39** *l. 401 global carbon stocks*

**Response:** We have corrected this typo.

**R2.40** *l. 402-4 This is misleading: 5 out of 10 models showing tropical C losses is not “most” and vegetation C decline in 6 out of ten models is not “all” (referring to Fig 2i).*

**Response:** We appreciate that the reviewer perceived a discrepancy between Figure 2 and the statement "Carbon is lost in tropical latitudes in most models, and vegetation carbon declines in tropical latitudes in all models" during the Net-zero phase. In table A2 we can see that total carbon is lost in 7/10 models. The total carbon change was not directly represented in Figure 2, and the direction of the vegetation change was slightly ambiguous because the bars were stacked and therefore overlapping. To address this we have updated this figure to plot negative values below the zero line and positive values above, with a black line showing the total carbon change. From this updated Figure 2, readers can infer that vegetation carbon declines at cumulative-zero emissions in the tropics in all models.

We have updated Figure 2 and text referring to Figure 2 to reflect these changes.

**R2.41** *l. 408 I'd suggest “constant” instead of “flat”.*

**Response:** We have changed the wording as suggested by the reviewer.

**R2.42** *l. 431-3 This sentence is also confusing.*

**Response:** We have modified this sentence. It now reads (lines 293-294): "In contrast, at the point of cumulative-zero emissions carbon pools have had a similar scale of response, but the perturbation is relative to their lower carbon pre-industrial state."

**R2.43** *l. 443 Would the vegetation models actually be able to represent substantial delays ( $\geq$  decadal) of fertilization effects, given that modeled photosynthesis and NPP respond immediately to CO<sub>2</sub>? Or would the authors expect any such delay from observations/first principles?*

**Response:** We agree with the reviewer that the wording here was confusing. We expect the response of fertilization, as in enhanced photosynthesis, would respond quickly to CO<sub>2</sub> and would not be delayed in time. That being said, increased carbon stocks could potentially enable greater photosynthesis through greater leaf area, which could persist longer. To address this comment we

have rewritten the sentence in question as follows (lines 304-306):

This suggests that the effect of hotter temperatures, and fast response to declining CO<sub>2</sub> concentrations, experienced over this period outweighs any enhanced photosynthesis associated with the persistence of increased carbon stocks that may support greater leaf area from the period under which CO<sub>2</sub> concentrations were increasing.

**R2.44** *l. 447 punctuation: world. In particular,*

**Response:** This typo has been corrected.

**R2.45** *l. 449 As far as I know, at least the MPI model operationally contains a component for fires. Has it been disabled for these simulations? If enabled, I don't see its response being particularly strong compared to the other models, so I wonder if this aspect would actually play a substantial role.*

**Response:** Fire is active in MPI-ESM1-2-LR via the SPITFIRE scheme and was enabled in these simulations, as it is in five of the ten ESMs (now indicated in Table 1). We agree with the reviewer that the fire response is not visibly strong in MPI relative to other models (Fig. R4). Across the ensemble we find no systematic difference between ESMs with and without interactive fire, so we have reframed this passage to present fire as one of several disturbance processes whose representation varies across models rather than as an asserted driver.

**R2.46** *l. 465 Is there a possibility to verify this based on GFDL-ESM4 papers/model documentation? Then, this could be turned into a definite statement.*

**Response:** We have modified this statement to make it more definite as follows (lines 328-329): "The third ESM (GFDL-ESM4) shows low initial stocks of high latitude soil carbon owing to a lack of permafrost carbon despite vertically resolved soil carbon pools."

**R2.47** *l. 468 word order: both due to*

**Response:** We have edited the wording as suggested by the reviewer.

**R2.48** *l. 512-4 This sounds a bit as if the ESMs were more or less deliberately "mistuned". "Accurate" land carbon stocks are an important tuning objective of ESMs, as documented in model description papers. I agree with the second part of the sentence; perhaps the first half could be rephrased or specified?*

**Response:** We appreciate the reviewers concern about possible misinterpretations of this section. We have rephrased this sentence which now reads (lines 396-397): "The considerable uncertainty associated with the best estimates of land and ocean carbon stocks contribute to the lack of convergence on pre-industrial carbon stocks across ESMs."

**R2.49** *l. 524 CO2 removals are relevant for net-zero emission scenarios too, maybe even for low-emission stabilization. Therefore, net-zero emission scenarios (such as esm-flat10-zec) also contain implicit removals, so this should be added.*

**Response:** We understand that the reviewer would like us to include both the zec and cdr

simulations in our discussion of the missing representation of actual carbon dioxide removal processes. The esm-flat10-zec simulations don't specifically contain any negative emissions or anthropogenic CO<sub>2</sub> removal. The emissions go from positive 10 *PgC/yr* to zero and are never negative. Scenarios for net-zero rather than zero emissions could exist, but within the framework of flat10 experimental design they are identical. To address this point we have edited the text as follows (lines 407-411):

The esm-flat10-cdr simulations impose the magnitude of the carbon dioxide removal as a boundary condition of the emissions-driven scenario through a negative emissions flux - they do not directly model the processes driving carbon dioxide removal some of which could have additional climate impacts due to physical changes to the land surface (i.e. Swann et al., 2012; King et al., 2024). Similarly, esm-flat10-zec simulations represent either zero or net-zero emissions, the latter of which implicitly includes carbon dioxide removal.

## Figures

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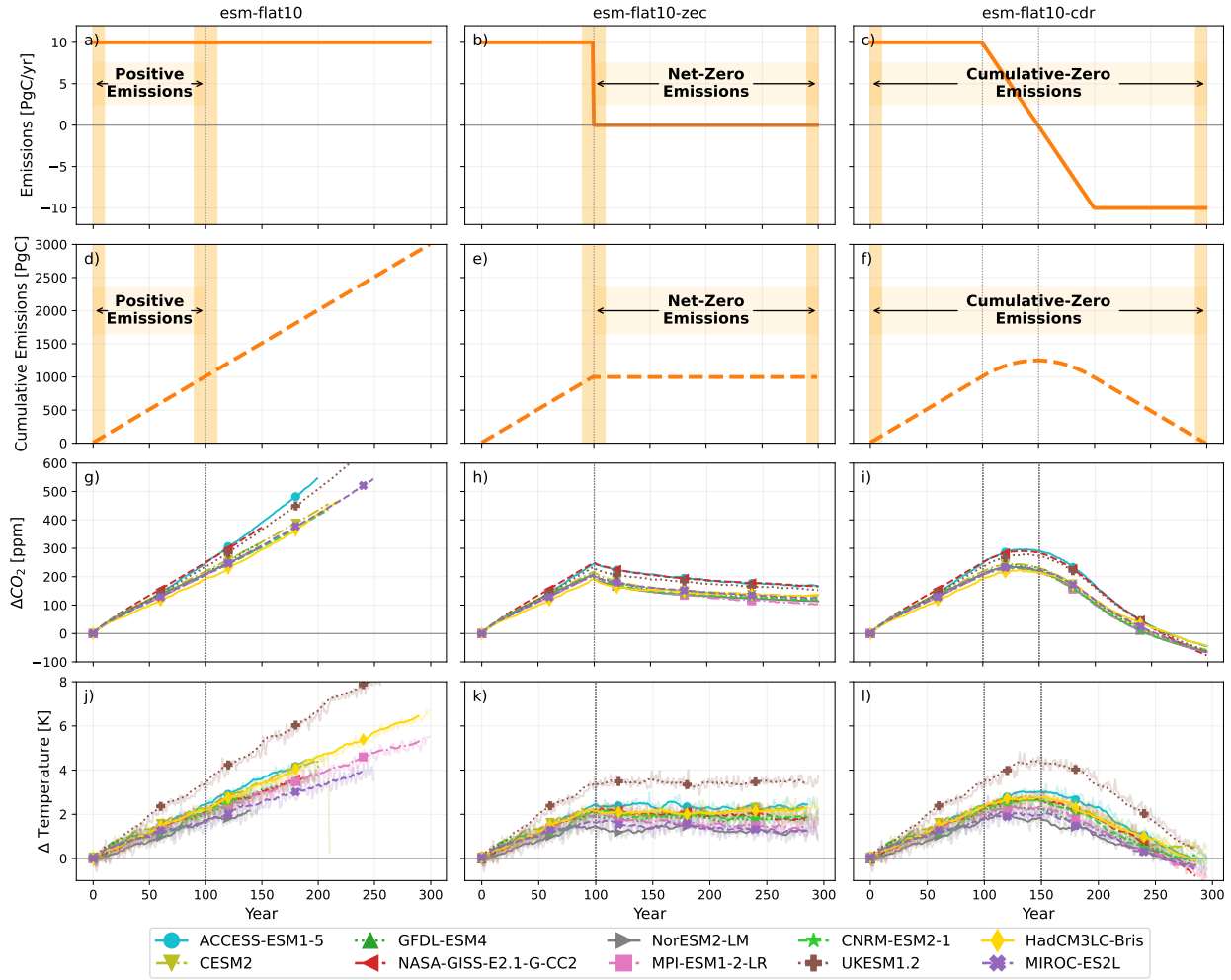


Figure R1: **Global mean timeseries emissions, cumulative emissions, temperature, and CO<sub>2</sub>.** Time series of annual emissions (top row), cumulative emissions (second row), change in global mean atmospheric CO<sub>2</sub> (third row), and temperature (smoothed with an 11-year running mean). Columns show each of the three experiments: esm-flat10 (left), esm-flat10-zec (middle), and esm-flat10-cdr (right). Each line in the second and third rows represents one ESM participating in flat10MIP. Colored circles indicate the year of peak CO<sub>2</sub> or temperature during the esm-flat10-cdr experiment. The vertical dashed lines indicate year the year of 1000 *PgC* of cumulative emissions, which corresponds to the year of net-zero emissions in esm-flat10-zec, as well as year 150 in the right hand column which is the year of net-zero emissions in esm-flat10-cdr. Shaded horizontal bars on the first two rows indicate the phases referred to throughout the main text, and vertical shaded bars indicate the time periods used to compute the change over each phase.

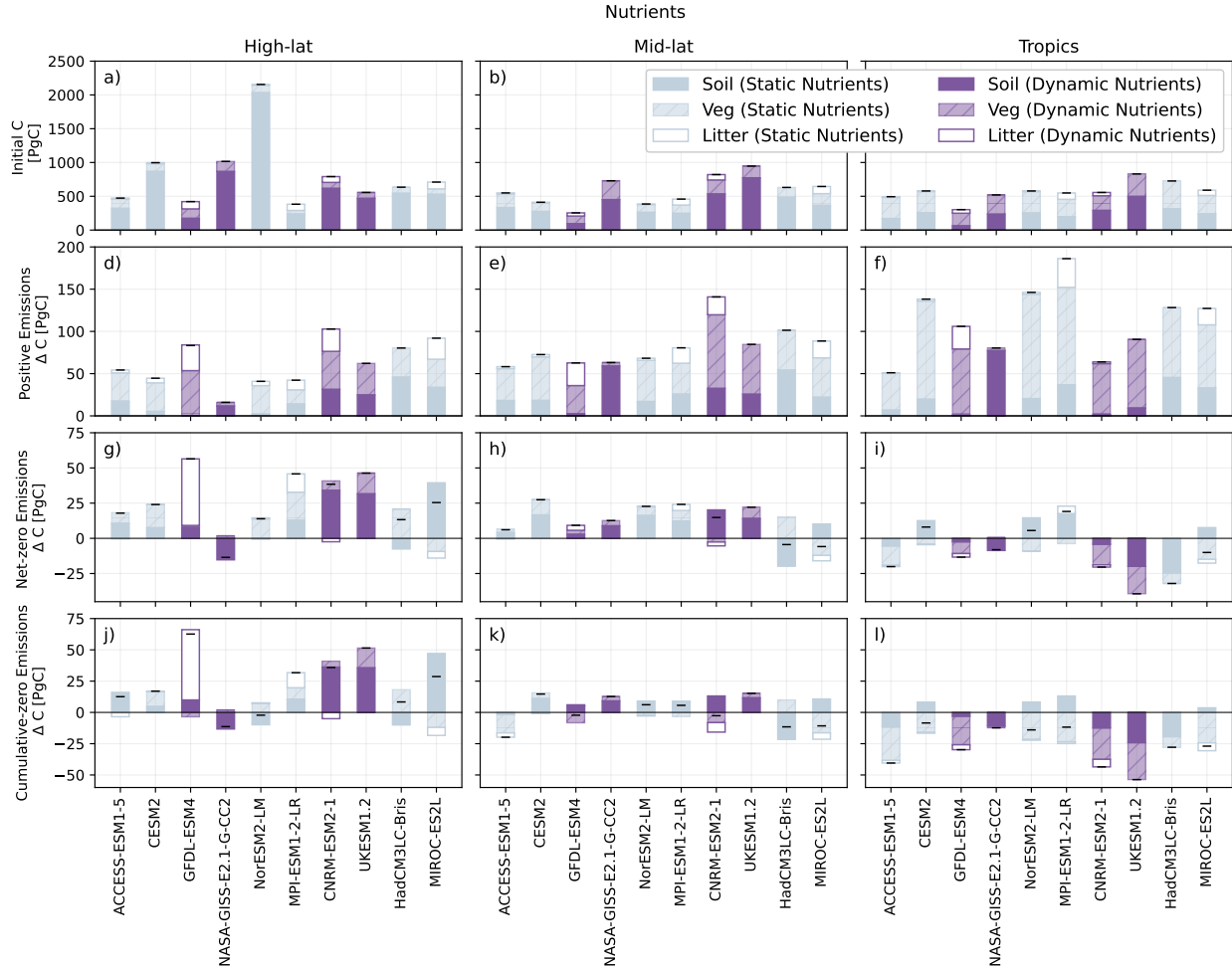


Figure R2: **Carbon and  $\Delta$ carbon at different latitudes colored by nutrient dynamics.** Bar graph showing initial carbon stocks (first row), and change in carbon stocks after positive emissions (second row), after net-zero emissions (third row), and after negative emissions emissions (fourth row) in each of the ESMs participating in flat10MIP. Each column shows the initial stock or change in C for soil carbon (solid dark color), vegetation carbon (lighter color), and litter (white with outline) high-latitudes ( $> 50^\circ$ ) in the left column, mid-latitude regions ( $> 20^\circ$  and  $< 50^\circ$ ) in the middle column, and tropical regions ( $< 20^\circ$ ) in the right column. Positive values are plotted above the zero line and negative values are plotted below the zero line. The total C (sum of all bars) is indicated with a black bar. This figure shows the same data as main text figure 2, except that here colors indicate models with either dynamic or static representation of nutrient limitation.

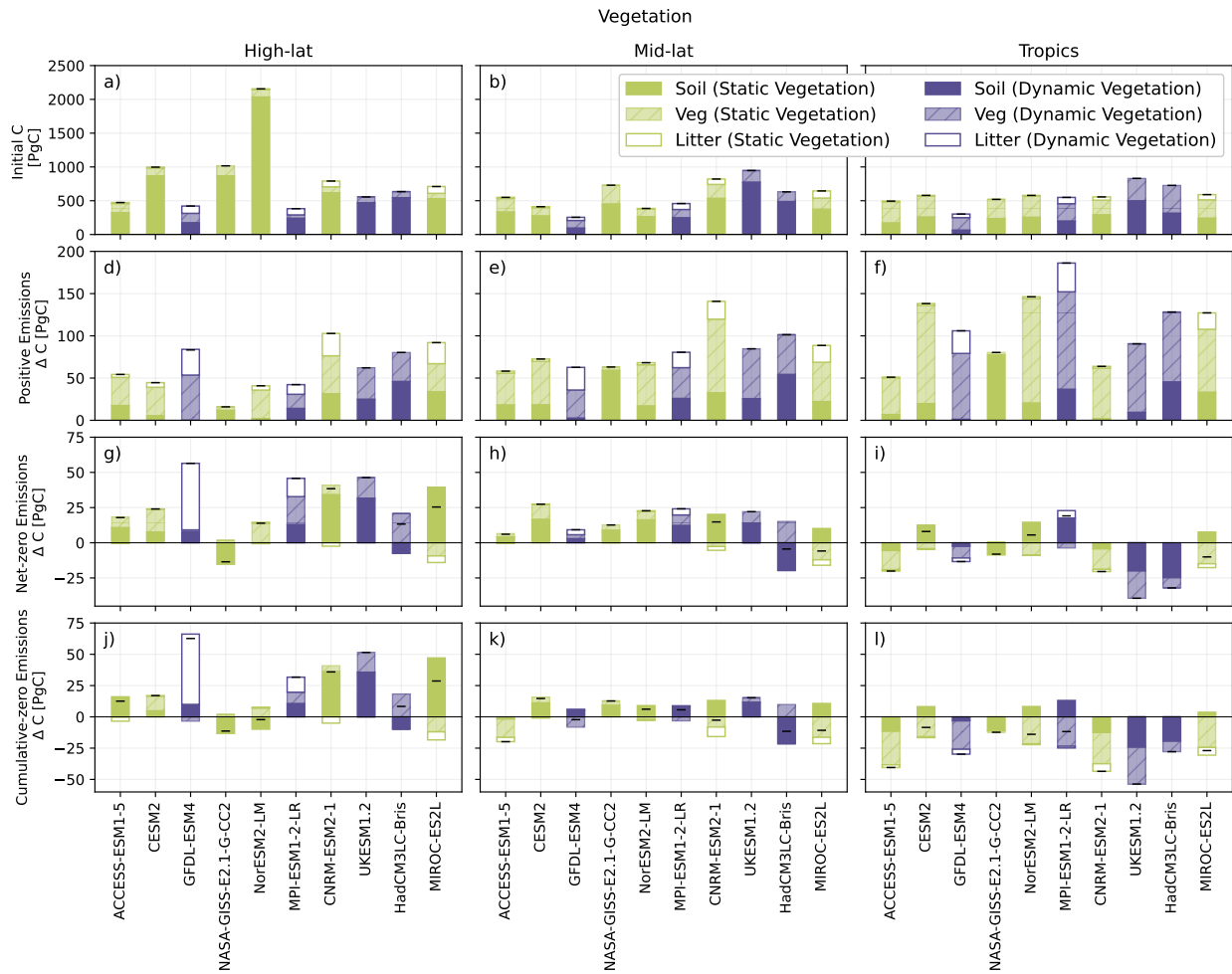


Figure R3: Carbon and  $\Delta$ carbon at different latitudes colored by vegetation dynamics. As in Figure R2, with colors that indicate models with either dynamic or static representation of vegetation dynamics.

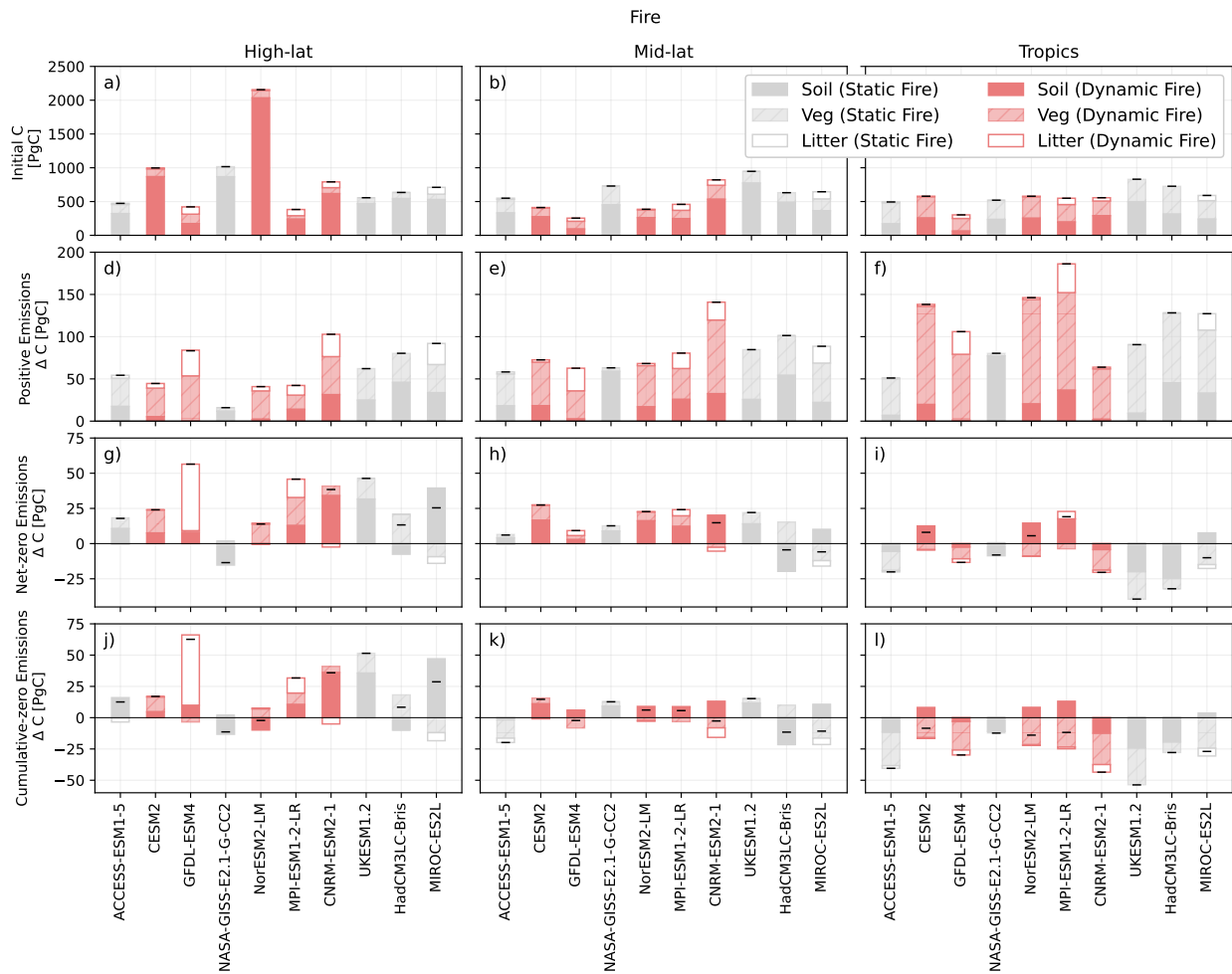


Figure R4: **Carbon and  $\Delta$ carbon at different latitudes colored by dynamic fires.** As in Figure R2, with colors that indicate models with either dynamic or static representation of dynamic fires.



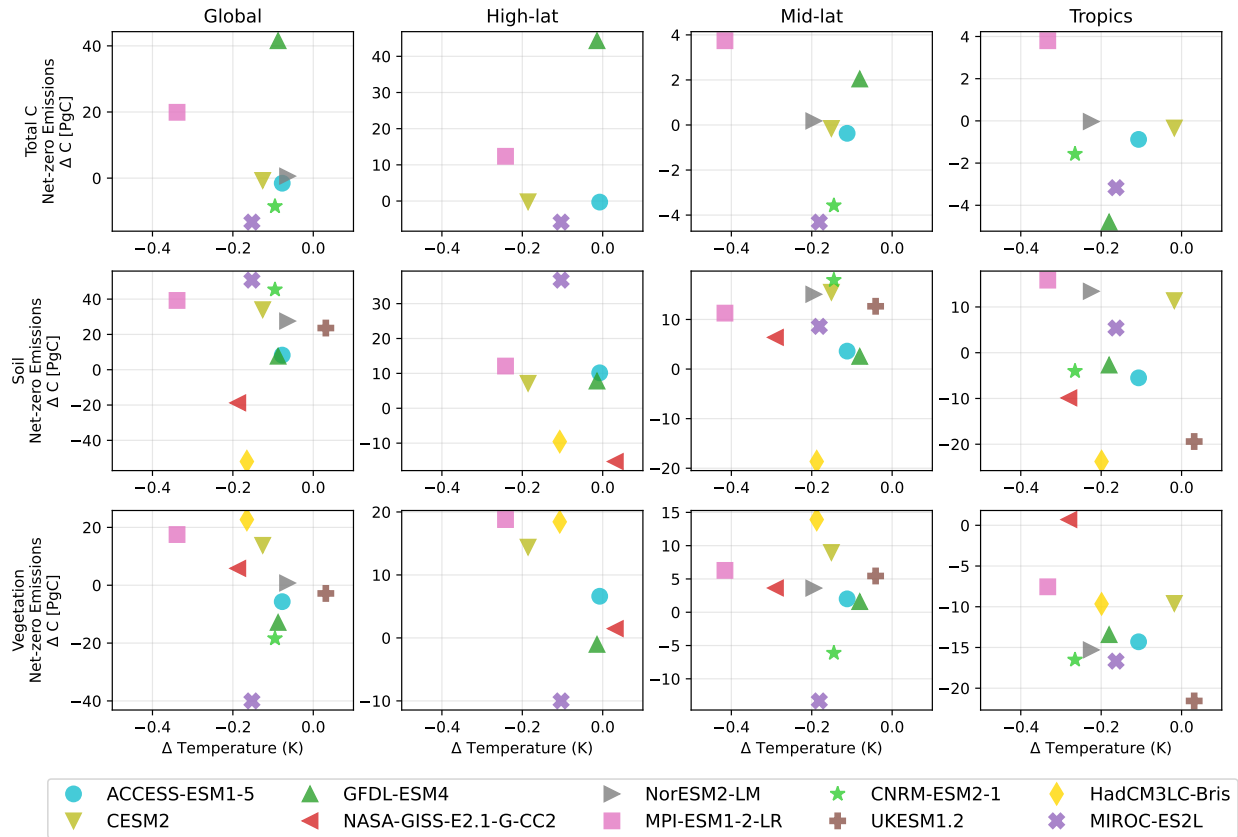


Figure R6: **Change in Carbon during net-zero emissions and change in temperature.** Change in carbon in units of *PgC* for total C (top), soil (middle), and vegetation (bottom) during the net-zero phase plotted against the change in near surface air temperature over land in units of *Kelvin*. Columns show results averaged over land for different regions including global average (left column), high-latitudes (second column), mid-latitudes (third column), and tropics (fourth column). Symbols and colors denote different ESMs.

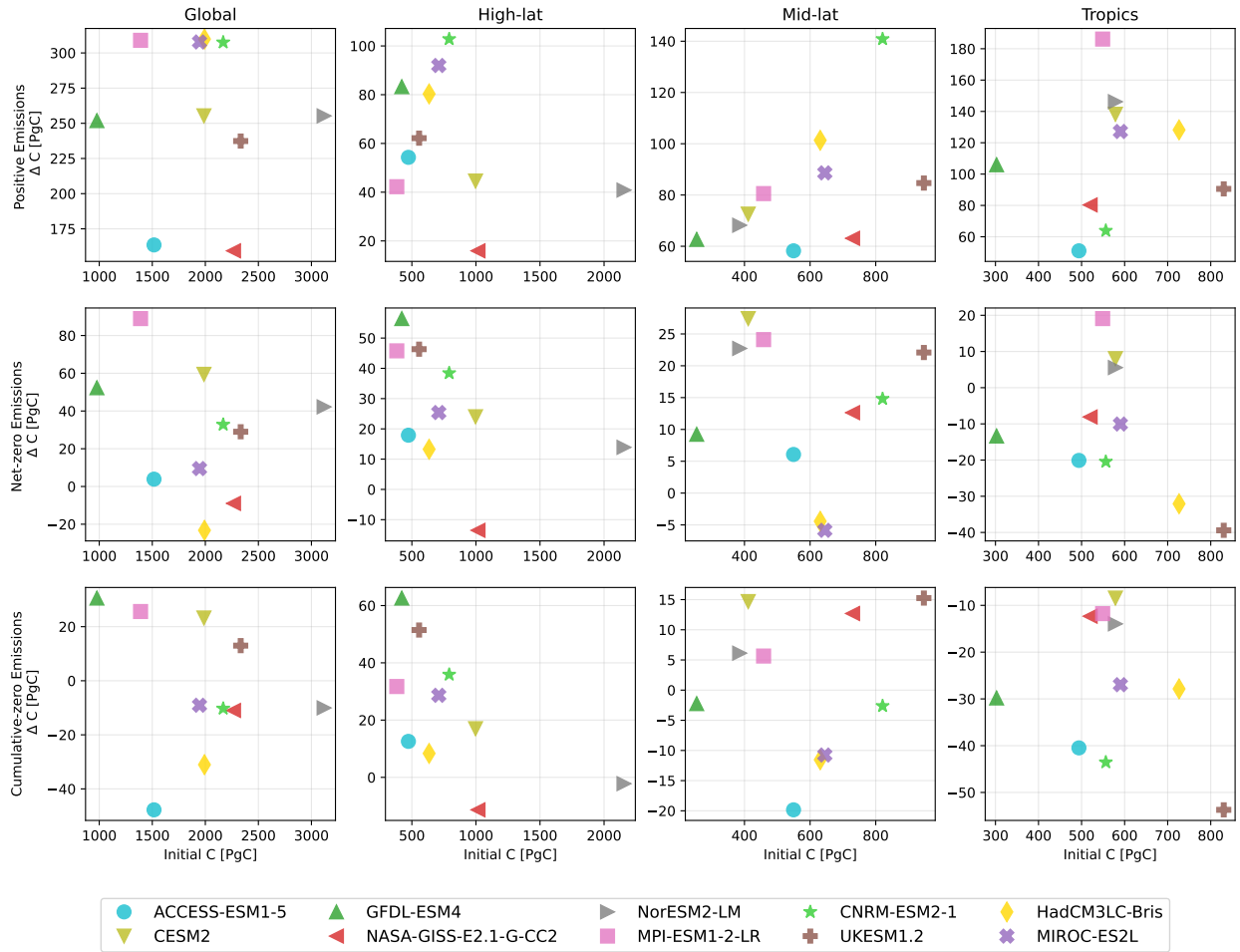


Figure R7: **Change in carbon during different phases as a function of initial carbon.** Change in carbon in units of  $PgC$  for during the positive emissions phase (top), net-zero phase (middle), and cumulative-zero phase (bottom) plotted against the initial carbon in units of  $PgC$ . Columns show results averaged over land for different regions including global average (left column), high-latitudes (second column), mid-latitudes (third column), and tropics (fourth column). Symbols and colors denote different ESMs.

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