Response to reviewer 1 ***(responses in italics)***

The global carbon budget provides an important contribution to both science and policy by updating annually information about emissions and sinks of CO2 and their trends. This year's budget is no exception, with valuable insights including recent increasing trends in AGR in spite of a small increase in EFOS and that cannot be explained by the current models, and highlighting persisting uncertainty in SLand in the northern extra tropics. A notable new development is the inclusion of the RSS correction, which results in a weaker, but possibly more realistic, land sink. The report follows previous years' structure and content, and is well written and well referenced. Nevertheless, I would like to point out a few aspects that can be improved or that need further clarification or discussion.

1) General comments
1.1) RSS correction
This is an important new addition to the budget, since the DGVM protocol used in previous budgets assumed no legacy emissions from land-use prior to 1850, implying a stronger land sink than what is plausible.
However, in many figures and tables, it is not clear whether the land component fluxes show include or not this correction, making it difficult for the reader to appreciate its impact in all relevant components of the budget.

The new method for estimating SLAND does not include legacy emissions from land-use. It now estimates the "natural" fluxes on a changing land cover while excluding the direct effects of the land cover change. Regarding figures - we only adjust the global SLAND values for the RSS. Where individual DGVM or ensemble mean DGVM data is shown, this is the raw model output. Figures 3, 4, 10 include the RSS adjustment when showing SLAND.

1.2) Next-year projections
This budget, like previous ones, provides an estimate of the values of emissions and sinks for the next year, in this case 2025. These projections are based in very different methods, that rely on many assumptions and which are not consistent with each other (the ESM projections being the most self-consistent). Since next-year projections have been now given for several budgets, it would be important to discuss how much confidence can be attributed to these projections: how have previous projections performed? Are the respective uncertainty ranges provided here realistic?

Thank you for the suggestion, this is now done in a new table (Table S7) where the projections of GATM, SOCEAN and SLAND are compared against the estimates from the following GCB release (ex. 2024 projection from last year GCB vs 2024 estimate from this year GCB).

Specific comments
Abstract
Lines 193-204: I understand the need to describe briefly the datasets used, but since the budget follows the same approach as previous years, I would recommend shortening this description, in order to rather highlight the new additions (for example the RSS correction)

and provide more depth on the new insights from this year's update (for example the very large value of BIM for 2024).

The general description is already very short: one sentence per component of the budget. However, we tried to reduce further more and introducted the new methodological changes.

Lines 205-213: it would be helpful to report percent increase for all budget terms.

The reason we only provided percent change for fossil CO_2 emissions is because this is the main anthropogenic driver of climate change, hence a strong policy interest in rate of change. We now do the same for land use emissions (ELUC), for completeness, but not for the other components of the carbon cycle (GATM, SOCEAN or SLAND) where such information is not highly relevant.
Lines 216-220: it would seem important to add here some insight about possible causes for the land BIM in 2024.

We do, the sentence reads: leaving a large negative BIM (-1.7 GtC yr-1), suggesting that the total sink or GATM is strongly overestimated in 2024.

Introduction
Please state briefly in the introduction what the novel aspects of this year's budget are (for example the RSS correction), compared to previous ones.

We already do this in the introduction of section 2 Methods (last paragraph). This seems more appropriate than in the general introductioon.

Lines 367-382: these sentences would probably fit better right in the beginning of the introduction, before stating the current budget's approach.

Not sure why it would be better to start saying the GCB is used by IPCC before explaining what the GCB is. We would rather keep this paragraph as the final paragraph of the introduction.

Results
The use of sign to indicate an increase/decrease "by" is not always consistent but it is needed for clarity. For example, in Lines 996-1006 +3.6 is given for India, but – sign in "EU27 by" and "USA by" is missing. Many other instances can be found throughout the text across all sections, I note some lines here, but a careful editing is needed. Lines: 1011, 1014-1015, 1028, 1039, 1042, 1048, 1055, 1057-61, 1078, 1205 (35TgC "up"), 1228, 1246, 1262, 1302, 1417, 1422, 1432, 1444, 1950-51.

Thank you for checking, we tried to be consistent in the revised manuscript, always providing the negative sign when there is a decrease.

Fossil fuel
Lines 1026: please state briefly what information and assumptions the projections are based on.

This is done in the Methods description, section 2.1.2

Land use change
Lines 1068-1069: clarify which period is covered by vegetation biomass observations.

It was stated in the next sentence (1901-2012) but we slightly modified the text sentences to mention the time period in the first sentence.

Lines 1096-1099: it is the first instance when fluxes, rather than emissions are mentioned. It would be helpful for non-expert readers to state the sign convention here.

Thank you, this is done now
Lines 1103-1114: this is relevant for decision making, but it overlooks the fact that these models do not represent processes that can reduce the land sink, especially disturbance fluxes, as noted by Roebroek et al. (2023) among others. Please state limitations and assumptions underlying the values given here.

The numbers are based on bookkeeping models. These models are based on carbon stocks that are observed (direct observations or literature-based). This approach therefore accounts for the average cycle of natural disturbances (though it does not predict them specifically for individual years). Further, the bookkeeping models are adjusted over time based on the evolution of carbon densities from process-based models. This accounts for shifts in environmental conditions and therefore in principal for the associated changes in disturbance regimes, although it rightly can be pointed out that global process-based models lack detail of some specific disturbance regimes like insect outbreaks. Since this applies to any forest-related carbon fluxes, not just re/afforestation, we include a statement on disturbances in the methods section (S.2.2) rather than with the CDR estimates here.

Lines 1111: it is unclear if here you refer to a potential re/afforestation sink, or current CDR.

Thank you, we have clarified this in the revised manuscript. The new text reads as: "Carbon Dioxide Removal (CDR) in forests could instead be increased by permanently increasing the forest cover through re/afforestation. Currently, re/afforestation creates a removal of -0.6 GtC yr-1 from the atmosphere averaged over 2015-2024."

Lines 1151: clarify here you mean nature-based CDR, right?
Done, thank you

Lines 1160: are RECCAP2 the estimates that are shown in Figure 15?
No, the estimates shown on Figure 15 are from our study, estimated over the RECCAP2 regions, not the values reported in the RECCAP2 papers as RECCAP2 was not estimated over the same 2015-2024 period. We removed the reference to RECCAP2 here to avoid confusion.

Lines 1194-1196: I find this confusing, since BM models do not include fire emissions due to natural drivers such as El Niño droughts. Shouldn't this be rather part of the SLand discussion? And do DGVMs capture these emissions?

The Bookkeeping estimates do include impacts of droughts in so far as they increase land-use related fluxes, that is, when deforestation fires or agricultural burning spread anomalously because of the dry conditions, increasing deforestation areas, or because fires on drained peatlands are more wide-spread and intense. We have clarified this in the text. Most, but not all, DGVMs include wildfires (see Table S1).

Atmospheric CO2
Lines 1271: is r for pearson correlation?
Indeed, we clarified in the revised manuscript.

Lines 1295-1299: this is an important point. What would be the implications for 2023, then?
Thanks for the suggestion, this now added in the revised manuscript : "As the the satellite-based GRESO growth rate is more evently split between 2023 and 2024, so would be the BIM, -0.3 GtC in 2023 and -0.6 GtC in 2024 with GRESO, compared to +0.4 GtC in 2023 and -1.7 GtC in 2024 with the GATM from the surface based network"

Lines 1301: briefly clarify what the "GCB regression method" is based on.
Done, we added "described in section 2.4.3"

Lines 1303: give reference to "neutral ENSO year".
Done, thank you.

Ocean sink
Lines 1365: remove . after Tropical Pacific
Done

Lines 1382: is there a reason uncertainty ranges given using different notations (+- vs [ ])? If so, clarify.
We have a rule to give the range when there are less than 10 individual estimates. This applies for the fCO2-products here and also for the atmospheric inversions before 2015 and for the ESMs (see Tables 5 and 6). This is now mentioned in the introduction: We adopt a range of ±1 standard deviation (σ) to report the uncertainties in our estimates, representing a likelihood of 68% that the true value will be within the provided range if the errors have a gaussian distribution, and no bias is assumed. Note that when less than 10 individual data are available for an estimate (e.g. atmospheric inversions, f-CO_2 products), we provide the full range, as opposed to the standard deviation."

Lines 1415-1420: it would be more informative to have this point being discussed along with discrepancies between models and observation-based products. Here makes comparison more difficult.
Thank you for the suggestion, but we would rather keep it here, at the end of the section, as a summary of the SOCEAN estimate and comparison with the independent approaches from O2/N2, inversions and ESMs.

Lines 1443: briefly explain what predictors are used by the FFNN.
This is described in the Method section (section 2.6.4).

Lines 1489-1491: since there seems to be a linear relationship between the two, would it make sense to apply a correction to fCO2? I do not have a strong opinion, but I am curious if the authors have considered this.
This is indeed an interesting thought and we equally have considered this option. However, to-date there are a couple of caveats: Firstly, we were unable to include all fCO2-products in the analysis (in part because some use a setup that does not allow for a model testbed analysis). Secondly, we only have a subset of models available for the subsampling exercise and we thus are not certain that the relationship would hold if we would expand the model testbed. Finally, we still see a considerable spread in the regression line and we believe further evidence is necessary before we can recommend a trend correction. To make this more clear, we added to the revised manuscript following line 1491: "This relationship, however, remains uncertain and its sensitivity to the GOBM testset needs further investigation"

Land sink
Lines 1497-98: does the RSS correction affect only the mean values (bias), or are there carry-over effects that could result in different IAV and trends? Please clarify that the RSS correction was only applied to the DGVM multimodel mean, not to the individual DGVM estimates (as far as I understand).
The RSS is calculated annually and so could impact interannual variability and does impact trends (as there is an increasing trend in RSS). Indeed, the RSS correction is only applied as a final step to correct the estimate of SLAND from the DGVMs multi model mean.

Lines 1505-1506: give value of the residual sink here too.
Done, thank you

Lines 1560: is it unclear if the values given here include the RSS correction, please clarify.
Yes, this includes the RSS correction. We have added a sentence to clarify: "(including the RSS correction) "

Lines 1571-1573: how about fires?
We agree. Added text "and fires". Also fire is discussed in detail in the paragraph below.

Lines 1574-1586: do the models representing fires estimate a weaker sink?
Yes, the fire models simulate a weaker sink in 2024 and a larger negative anomaly. We have added the following text: "Fire enabled DGVMs do simulate a larger reduction in SLAND in 2024 compared to non-fire models (0.6 vs 0.3 GtC), indicating the importance of representing fire extremes when explaining reductions in the land sink."

Partitioning of the fluxes
Lines 1683-1687: it is worth mentioning that, however, the northern land flux does not seem to contribute much to the BIM in 2024.
The 2024 anomaly is discussed in the following section (Tropics) as the tropical lands explain most of the anomaly, as seen in the inversions, with implications for the BIM

Lines 1717-1721: are there implications for the BIM value in 2024 that can be drawn from this comparison?
Thank you, we added a sentence providing implications for the BIM in 2024: "This discrepancy between the DGVMs and inversions estimates of the global atmosphere-land flux largely explains the negative BIM in 2024."

Lines 1753-1795: this is a very informative addition. However, RECCAP2 provides estimates of regional budgets using additional datasets (possibly with higher confidence that global products). Therefore, it would be worth comparing the estimates here with the values provided by RECCAP2 for each region, or at least highlight those regions where results from RECCAP2 indicate large discrepancies between global datasets and refined regional products.
That would be fantastic if doable, however, the RECCAP2 analysis does not cover the same period (2015-2024). hence a like to like comparison is not possible.

Closing the global carbon cycle
Lines 1873: th should be superscript
Done, thank you

Lines 1876: with “historical” do you mean “pre-industrial”? Does this refer to the RSS correction?
We do mean historical. This is the RSS correction that accounts for the reduction in forest cover over the historical period, hence reducing the SLAND estimate relative to the DGVMs simulations that assume fixed pre-industrial land cover. We added "RSS in the text to make it clear.

Lines 1891-1893: is this only because of the RSS correction, or are there other reasons?
This is due to the combined effect of the RSS correction on SLAND, the correction on SOCEAN and the revised ELUC estimate.

Lines 1894-1898: the discussion of the disagreements in AGR between the surface network and satellite-based GRESO should be linked here. What is the effect of these disagreements on BIM for 2024?
Nice suggestion, we added a sentence saying: We note that using the GRESO atmospheric growth rate of 6.8±0.2 GtC/yr for 2024 (Section 3.5.3) would reduce the BIM to about -0.6 GtC.

Lines 1910-1913: I understand a full attribution is out of scope in this already long report, but Bastos et al. (2021) showed that the BIM strongly correlated with differences between inversions and global models, so maybe something could be said about the BIM based on Figure 14? At first glance some of the peaks in BIM in Figure 4 seem to correspond to large differences between inversions and GCB estimates of the total and land fluxes shown in Figure 14, especially in the tropics.
Indeed, this is probably beyond the scope of this paper. The interannual variability of the BIM is likely to be due to errors in the variability of SLAND, although errors in GATM cannot be excluded, as highlighted for 2024. The decadal to long-term BIM is unfortunately much harder to attribute.

Lines 2024-2032: for traceability, it would be useful to have a table reporting major corrections/updates to the data and their implications.
We have a table describing major changes in our methodology (Table 3 for this year, and Table S9 for previous years).

Lines 2057-2082: there are several satellite-based biomass products that now allow to derive at least some components of ELUC, for example Xu et al. (2021). Are these planned to be incorporated in future budgets, if not, what are the limitations?
Satellite-based biomass products are only available for the recent decades, and they only estimate above ground biomass changes, not total carbon. Also, by definition, these estimates do not separate ELUC from SLAND. Directly using these products in GCB is unfortunately not possible yet.

Lines 2011-2116: provide values for the estimates by atmospheric inversions, oxygen-based estimates and Randerson et al. (2025) for comparison.
The estimates from atmospheric inversions and oxygen are provided in Table 7 as stated in the sentence. The Randerson et al estimate is not decadal, it only covers the 2000-2019 period.

Figures and Tables
Figure 13: do the DGVM estimates shown here include the RSS correction? Please clarify or, ideally, include both the uncorrected and corrected estimates.
The DGVM estimates shown here are for the S3 simulation to estimate the net land sink and so do not need the RSS correction. The RSS correction only applies to the S2 simulation for estimating SLAND. The 'GCB' estimate shown in the figure is the sum of DGVM SLAND and BKM ELUC, which does include the RSS correction. In caption we include "The 'GCB' central mean estimate includes the SLAND and SOCEAN global adjustments".

Figure 14: does the gray shade for DGVMs correspond to the land-flux with or without RSS correction?
Figure 14 shows the S3 simulation, i.e. the net land flux, not SLAND. Therefore, no adjustment is required as the S3 simulation varies all drivers together.

Figure 15 has very poor quality, please increase resolution.
We have the .pdf (vector) version available.

Data availability
I agree that making the data available will contribute to greater understanding and new scientific insights of how the carbon cycle works, as stated in Section 7. However, in that spirit, why are gridded data provided openly for atmospheric inversions only, and not for the other datasets underlying the budget, especially those used in the main figures? Providing all gridded data openly would contribute to speed up understanding of uncertainties and knowledge gaps, by making it accessible to the broader scientific community. Furthermore, this hampers reproducibility of some of the main results of this paper (Figures 6, 11, 12, 15), and does not seem to fully align with ESSD core principles as stated in the journal's page.

The data files for all figures are freely available at (https://globalcarbonbudget.org/datahub/the-latest-gcb-data-2025/). Additionally, the land and ocean gridded data are also fully open and available via that website.

Supplement
It is difficult to extract from the supplementary information the key aspects of the protocol used for DGVM and BM models (starting year, period covered, forcing, adjustments, etc.), since they are referred to in between lengthy descriptions of specific aspects of the datasets. These lengthy descriptions are useful since they contain very relevant information, but I would suggest summarizing the modelling protocol and adjustments performed in a table (similar to Table S2 for GOBMs).
The protocol for the GOBMs slightly differs across models, in particular for the spin up phase, hence the table S2 is needed to report these model specific set-up. For DGCMs, they all follow the same protocol described in the SI, with the simulations S0-S3 describing starting year, period covered and forcing. We don't feel that a table is necessary as it would show identical entries for all DGVMs.

Line 857: I suggest adding a subsection header for the RSS correction here (or highlight as a paragraph), making it easier for the readers to find the relevant information.
Done

Response to reviewer 2 ***(responses in italics)***

General:
This manuscript presents the 2025 update of the Global Carbon Budget as part of the established "living data" series. The paper maintains the high standards of transparency, community coordination, and documentation that characterise previous releases, and the resulting dataset will be of high value to both the research and policy makers.
Relative to the 2024 release, the manuscript introduces several methodological changes, most notably the RSS correction, the use of transient carbon densities in land-use change emissions, and the inclusion of satellite-derived atmospheric CO2 growth rates, and explicit data from Japan. These updates lead to noticeable changes in the land-ocean partitioning of CO2 uptake and in the behaviour of the budget imbalance. Overall, these changes are well documented in the Methods section.
The Results & Discussion highlights several key outcomes of the 2025 update, e.g. revised estimates of land and ocean sinks and updated uncertainty ranges. Satellite-based CO2 observations provide enhanced spatial and temporal coverage relative to surface networks. The 2024–2025 El Nino is highlighted as a major driver of elevated atmospheric CO2 growth rates and weakened sink performance. Emissions trends continue to diverge regionally, with declines in some industrialised regions and continued growth in developing economies. The remaining carbon budgets are updated, reinforcing the urgency of rapid emissions reductions. Remaining challenges include large uncertainties in land-use change emissions and ocean uptake, underscoring the need for expanded observations, improved regionalisation, and continued methodological development.
Overall the manuscript should be published of course. I nevertheless recommend some revisions primarily to improve clarity in distinguishing method-driven revisions from data-driven signals, and to tone down some interpretive statements in the Results and Discussion.

A central issue throughout the Results and Discussion is that several prominent changes relative to GCB 2024 arise primarily from methodological corrections (e.g. RSS corrections, revised ELUC treatment, updated GATM estimation), rather than from new observational constraints. While this distinction is carefully described in the Methods, it is not always carried through consistently into the Results and Discussion. I recommend that the authors more explicitly flag when differences relative to GCB2024 are driven by revised assumptions or corrections, especially when discussing the important differences relative to GCB2024 (particularly in ocean uptake, land–ocean partitioning, and budget imbalance)

Thank you raising this point. At the beginning of the ocean sink and land sink results sections, we now remind that the reported estimates of SOCEAN and SLAND are corrected, as described in the method section. We also report the values of the uncorrected estimates are for the last decade (2015-2024). Likewise for the BIM estimate for the last decade, we also report the BIM estimate with uncorrected SOCEAN and SLAND.

The exceptionally large atmospheric CO2 growth rate in 2024, combined with a strongly negative budget imbalance, is a key result of this update. While the manuscript discusses this clearly, parts of the Discussion imply causal attribution (e.g. sink overestimation or sink failure) that is not uniquely supported by the budget closure alone. Given that multiple

budget components (SLAND, SOCEAN, and GATM) were revised simultaneously, and that correlated uncertainties remain substantial, attribution of the imbalance to any single process is inherently ambiguous. Statements linking the 2024 imbalance to specific sink behaviour should therefore be softened or explicitly framed as hypotheses rather than conclusions.

Thank you for this suggestion. The 2024 budget imbalance is large: -1.7GtC. This is too large over one single year to be due to an underestimation of the anthropogenic fossil or land use changes CO_2 sources that do not show such large year to year variability. Similarly, the ocean sink does not vary significantly from one year to another. The culprits must be the land sink or the CO2 growth rate. Given the discrepancy between the land sink estimate from the DGVMs and atmospheric inversions, we have strong reason to believe the imbalance comes from the land sink estimate (as now discussed in section 3.8.1.1). Nevertheless, we added a mention of the uncertain CO_2 growth rate in 2024, revising the statement in section 3.9.1 and writing : "For 2024, the combination of our estimated anthropogenic sources (11.6 ± 0.9 GtC yr−1) and partitioning in atmosphere, land and ocean (13.3 ± 0.9 GtC yr−1) leads to a large negative BIM of -1.7 GtC (Table 7), indicating that, despite the lower than average SLAND in 2024, the land sink is still too large to explain the record-high atmospheric CO2 growth rate of 2024. We note that using the GRESO atmospheric growth rate of 6.8±0.2 GtC yr−1 for 2024 (Section 3.5.3) would reduce the BIM to about -0.6 GtC."

Minor:

Please explicitly remind us readers that changes in ELUC relative to GCB2024 arise from revised model assumptions (transient carbon densities), rather than new land-use activity data.
Changes in ELUC this year results from the revised transient carbon densities indeed, but also from having 3 bookkeeping models this year vs 4 in GCB2024. The land cover forcing is updated every year (to the most recent year), with potentially some, more minor revisions of previous years. This is explained in section 3.2.2 when the ELUC results are presented, with a reference to figure S1 and S3 for attribution of each change.

When discussing the suspension of SOCAT-based products and applied corrections, a brief summary sentence reiterating the rationale would help non-specialist readers.

There is no direct relation between the suspension of some ocean datasets in SOCAT and the correction applied to the SOCEAN estimate. They are both listed in Table 3 as both being methodological changes in this year budget GCB2025 relative to last year GCB2024.

Minor inconsistencies remain in the use of terms such as “uncertainty,” “spread,” and “variability.” These could be harmonised for clarity.

Thank you, we tried to be as consistent as possible. We use uncertainty when quantified with a standard deviation; range when quantified from less than 10 data streams (ESMs, fCO2 products). Variability is used to quantify year to year changes.

Specific Comments (please be aware that the line numbers refer to the manuscript with tracked changes which I kindly received from the editors)

· Lines 641–642 (Abstract): The large negative budget imbalance in 2024 is interpreted as suggesting that “the total sink or GATM is strongly overestimated”. From a mass-balance perspective, an equally plausible interpretation is that land carbon losses are underestimated. Processes such as drought-enhanced respiration, disturbance-related emissions (e.g. fires, peat), and post-disturbance legacy fluxes are not fully constrained and would lead to an apparent negative imbalance. I suggest rephrasing (briefly in the abstract and in more detail in the main text) to explicitly acknowledge underestimated sources or land carbon losses as an alternative or additional explanation.

At the abstract level, we believe it is OK the say the sink is overestimated. The sink is the net result of carbon gains and losses, with gains being larger than losses both for the land and the ocean. Saying the "net" flux (the sink) is too large does not imply the gains are overestimated. It could indeed be the losses that are underestimated. This is discussed in more details in sections 3.7.3. Post disturbance legacy fluxes are unlikely to play a major role for one year only.

· Line 1047: The sentence “Global emissions and their partitioning among the atmosphere, ocean and land are in balance in the real world” is potentially misleading. I suggest rephrasing

We are not sure why this could be misleading. The sentence described the mass balance (conservation of carbon globally). We now write "mass balance" hoping this was the issue.

· Lines 1222–1236: Please add a reference to the description of how aviation and shipping emissions are estimated and projected.

For the historical estimate, the reference is given: Andrew and Peters (2025). For the projection, they are estimated in this GCB paper. The method is briefly explained in section 2.1.2 with a more detailed description in the SI, section S.1.5

· Lines 1745–1747: Please add a reference to support the statement introduced here.

We are not sure which statement the referee refers to. The statement line 1745 has 6 references already: (Gitz and Ciais, 2003; Sitch et al., 2005, Pongratz et al., 2009; Gasser et al., 2021; Obermeier et al. 2021; Dorgeist et al., 2024).

· Lines 2626–2627: Please provide quantitative values (absolute and/or relative) for the reported increase in CO2 emissions due to peat fires in tropical Asia.

We have added the estimates: "Emissions are further increased by peat fires in equatorial Asia (GFED4s, van der Werf et al., 2017), contributing substantially to emissions in individual years, typically related to dry conditions during El Niño (0.2 GtC in 2015; 0.03 GtC yr-1 averaged over 2015-2024)."