

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 introduced 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 CO₂ 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 introduction.

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 evenly 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-CO₂ 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 than 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-qcb-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