

## RC2 Anonymous Referee #2

Thanks for opportunity to again review important document. I will certainly recommend publication. Document has gotten longer, while time to review - esp this year - has gotten shorter. Too much to ask, even of strong advocates? Authors and journal need to consider alternate approaches! I evaluate mostly marked changes. I read full text to understand context for changes. Line number references refer to full 2022 document. Basic question, perhaps not answerable here but also not addressed: Why do atmos CO2 concentrations (e.g. Figure 1) rise continually apparently without any influence from substantial changes documented in Efos etc. as in Figure 3?

*Atmospheric CO2 increases (GATM) as long as emissions are larger than sinks, see Table 6. This is the case for the whole historical period. Unclear which "substantial changes" documented in EFos the reviewer is referring to. The small decline in fossil fuel emissions in 2020 (-5%) was too small to stop the increase of atmospheric CO2. The world still emitted 10.5 GtC in 2020, which is much larger than the land and ocean sinks, hence the observed increase in atmospheric CO2.*

Technical issues/changes:

Line 159: "synthesise" - ESSD editors will know correct spelling

*We use British English. We will change if requested by the editor.*

Lines 169 to 171: Efos = 10.1 + Eluc 1.1 = Etotal of 11.1? As in prior years, small offset must represent rounding error? Scientific readers should understand uncertainty limits (1 sigma) but casual readers will see this as math error? Also, because of qualification due to cement carbonation, does this total include or not include the carbonation term? One case Etotal = 11.0, in the other 11.2, both within uncertainty limits but both will confuse some readers?

*Rounding errors indeed. For 2021, Efos = 10.07 (9.85 including the carbonation sink), Eluc = 1.08, Total = 11.14 (10.93 including the carbonation sink). The total reported now includes the carbonation sink (as in Table 6 for 2021), this is clarified now.*

Line 176: "50% above pre-industrial" this means pre-industrial = 270 ppm? We know this but casual readers may not. Likewise at lines 221. Not defined (277 ppm) until line 239.

*Done, and updated to 278ppm as in IPCC AR6.*

Line 191: "pre-COVID" not well defined (and, in any case, not determined by carbon community). Insert '2019' so that readers will understand changes on your terms.

*Done*

Lines 190 to 199: this paragraph seems slightly confusing? Efos for 2021 known (reported), now [quantified] as slightly below Efos 2019. (Why not specify Efos 2019 here?). Efos increase expected for 2022 [estimate growth], to above Efos 2019 (now Efos 2019 itemized but why here rather than earlier?). Then sectors then regions, but all as Efos 2022 estimates. Some readers may feel that authors jumped past solid 2021 data?

*We hope it is clearer now. The headline highlights the current year (2022), without quantification. Then the paragraph first reports the 2021 value, and second reports the 2022 estimate. Both are compared to 2019, the pre-COVID-19 conditions.*

Lines 203 to 211: now, appropriately, Eluc but on decadal rather than annual terms; tell us why? In line 202, authors revert to annual estimate. If not in fact significant, say so while omitting the numbers?

*As explained in the text, line 201-202 refers to the fossil fuel emissions over the last decade from the 28 countries where emissions decreased.*

Lines 206, 207: “highlighting substantial mitigation potential” this seems like IPCC-speak. For casual readers (of Exec Summary!) call out the desired change: less logging (deforestation)?

*Agreed, changed as : highlighting the strong potential of halting deforestation for emissions reductions.*

Line 207: “sequestration of 0.9” a quantified report or an estimate, not clear.

*Estimate from the bookkeeping models as all numbers reported here.*

Line 208: other ‘land-use’ transitions?

*Indeed, now added for clarity.*

Line 209: now reader confronts 6 decades (1959 to 2021)? Need consistent time-scales or explicit refocussing.

*Indeed, we now report the top 3 countries for the last decade (2012-2021)*

Line 215: rounding errors again induce confusion. 2021 Efos + cement at 11.1 while 2022 increase to 11.1? And what happened to carbonation?

*Sorry, the cement carbonation sink had been omitted by mistake. Corrected now.*

Line 217: release of IPCC AR6 WG1 in 2019? Formal citation = 2021? Please clarify?

*The IPCC AR6 WG1 was approved in August 2021, hence a 2021 publication date, but the data IPCC used for the remaining carbon budget stop in 2019. This is clarified now.*

Line 219: cumulatively for each of the next 28 years?  $28 \times 0.4 = 11.2$ . What (again) about carbonation? In the noise?

*Sorry, 0.4 is simply the current emissions level (10.9 GtC) divided by 28 years, in order to quantify the linear annual emission reduction needed to reach zero in 2050. Clarified now.*

[Speaking personally, these numbers assume we continue in a stable economic and social system? Very unlikely? Can authors not inject some note of enhanced uncertainty?]

*Not clear what the reviewer suggests here. We do not want to speculate on the future of the socio-economy of the World. This is beyond the scope of the Global Carbon Budget.*

Line 223: following from prior paragraph, this growth rate will need to trend toward zero? But, despite quantified decreased Efos for 2020, atmos concentrations of CO<sub>2</sub> showed no downward deflection?

*Indeed and this is to be expected from the global carbon cycle budget. See our response to the initial comment from Reviewer 2.*

Lines 224 to 230: Decadal records vs annual predictions? Can authors justify annual predictions in view of 3x uncertainty obs to models?

*Not sure we understand the question. The methodology describing the 2022 prediction and its uncertainty is described in section 2.4.2*

Lines 235, 236: If annual 1 Gt changes in Sland do not impact global atmos CO<sub>2</sub> concentrations, same also true for Socean? What are we missing as global atmos CO<sub>2</sub> concentrations continue to rise regardless of changes in Efos, Sland, Socean?

*Same response as before, we are not “missing” anything. Atmospheric CO<sub>2</sub> continue to rise because of large anthropogenic CO<sub>2</sub> emissions (EFOS). Nevertheless, we rephrased the sentence as follows: “Year to year variability in the land sink is about 1 GtC yr<sup>-1</sup> and*

*dominates year-to-year changes in the global atmospheric CO<sub>2</sub> concentration, making small annual changes in anthropogenic emissions hard to detect.”*

Line 254: parentheses rather than commas (e.g. “since the year 1750 (the pre-industrial period) and”)? Authors and editors will know.

*Done, thank you.*

Line 302: “characterising” looks strange to my eye but authors and editors will know.

*We use British English. We will change if requested by the editor.*

Lines 363 to 369: this paragraph implies that authors have applied carbonation corrections consistently to prior data? Not clear to this reader.

*As explained in that section, we take the average of the two studies available (Cao et al., 2020 and Guo et al., 2021).*

Line 403: information also for peat drainage <by combining> three independent datasets for peat drainage?

*Done, thank you*

Line 545: for widely-diverging GOBM and obs-based estimates, should this be median rather than average? Or, authors already selected against extreme values?

*We treat GOBMs and data-products as different types of data, hence we calculate the average for each data stream before taking the average of the two averages. We only reject one “extreme” data product (Watson et al), as explained in Appendix C.3.1).*

Line 632: here, carbonation corrections applied only since 2021 in atmos inversions? Somewhere this reader would like to find a statement about when corrections applied, over what time periods, or - as minor - not applied.

*As explained in the text, small differences in Fossil datasets used by the inversion models could have occurred, hence they are all adjusted to ensure agreement across inversion models and also with the estimate of EFOS in this budget*

Line 697: here reader finds/learns that carbonation correction applies since “1960s”. Need this clarification earlier to resolve previous issues.

*This is not what we wrote. We only report here the magnitude of the cement carbonation flux which increased “from an average of 20 MtC yr<sup>-1</sup> (0.02 GtC yr<sup>-1</sup>) in the 1960s to an average of 200 MtC yr<sup>-1</sup> (0.2 GtC yr<sup>-1</sup>) during 2012-2021. The actual dataset goes back to 1931.*

Line 789, 790: “emissions from organic soils contribute over proportionally to interannual variability” Something wrong somewhere?

*Sentence clarified.*

Line 784-809: sorry, but the authors lost me completely in this discussion of Eluc. I suspect authors could rewrite at half the length with twice the clarity. Not useful as written.

*Thank you, we will rewrite this section in the revised version of the manuscript.*

Line 815: “ to a substantial part for export” not clear what authors mean here? Land cleared of forest, converted to cropland, but crops then exported?

*Clarified: “export of agricultural products”*

Line 829: NGHGI defined here but acronym used several time previously?

*The acronym is defined before in section 2.2.1. but we also define it at the first instance it is used in the result section.*

Line 863: “relatively wet dry season” I know what you mean but highly awkward as written  
*Sorry, we can't really think of a better way to describe a dry season that is relatively wetter than average.*

Lines 901 to 903: why not these lines (about unprecedented atmos CO2 concentrations) in exec summary?

*This is taken from IPCC AR6 WG1 chapter 5. It is there for context but we don't see this as a result from the global carbon budget that would belong to the executive summary*

Page 87-88, Table 3: Excellent, should be required in any repeating global estimate in ESSD.

*Thank you. This table is always part of the GCB paper*

Page 94, Table 6: Legend includes a disclaimer about rounding to 0.1 GtC. Such a disclaimer should occur at top of manuscript text?

*Not sure what the reviewer means by “top of manuscript text”. We feel this is the right place to mention rounding numbers, as done for table 5 and table 8*

Page 97-98, Table 10: Interesting approach to show major uncertainties in one table. Appears much more orderly and organised here than this reader found in text?

*Indeed, it seems appropriate to show uncertainties for each component of the budget in one table.*

Page 107, Figure 9: Why can't these panels appear as large and clear as panel in Figure 10?

*Thanks for this. The submitted high definition file has larger panels - as in Figure 10.*

Page 110, Figure 12: Thought-provoking. In last year's version, Efos - Gatm occurred as a broad range and doubled line diagonally across the graphic, allowing this viewer to assume annual variations. In present version, Efos - Gatm follows a single linear line without variation. I need to think whether invariant Efos - Gatm across the ranges of Sland and Socan is even possible? Shouldn't individual Efos - Gatm points vary as BIM?

*Indeed, last year, we had the grey range which shows the uncertainty on EFOS-GATM (note that this has nothing to do with some combination of individual SLAND and SOCEAN). We didn't show it here, but it has been added in the revised version.*

Page 137, Figure B4: Good that authors include but suggests very weak relation atmospheric inversions to airborne measurements. Problem with obs or models or both? I understand why co-authors want inclusion in this notable effort but this suggests a substantive piece that could be eliminated, treated elsewhere, relegated to a supplement, etc. Really no change, and certainly no improvement, from prior version! This version could reference all of Appendix B by citation to previous rather than inclusion in every iteration?

*This figure is not showing relations, but biases in mole fractions. Biases are generally within 0.5ppm which is considered quite impressive for a comparison to independently gathered observations in a different part of the atmosphere. Such an “anchor” is deemed highly necessary in inverse modelling, and also used to evaluate the OCO-2 satellite inversions that the reviewer specifically highlighted in the review. We furthermore note that this figure is already part of the supplementary section, and the inversion evaluation is discussed in supplementary section C.5.2, not in the main text. We updated the reference to this discussion in Section 2.6.*

Page 140, Appendix C, lines 59 to 69: About carbonation. Text here describes (and, to certain extent, repeats information already provided) carbonation process. Final sentence

describes where carbonation processes fit in budget. Nothing here, however, about time span for applying these corrections, or of impact of leaving them out? Other sections of Appendix C, Methods, quite necessary, particularly when external economic or social factors force change in annual estimates. Likewise for Appendix D. But, Appendix B?

*Cement carbonation: there was indeed some repetition with the description in the main text (section 2.1). This has been updated now. Time span is also mentioned now (since 1931)*