

## ACP review essd-2021-386, global carbon budget 2021

Thank you very much for the opportunity I was offered to be reviewer of this excellent study. This new global carbon budget study, is a useful and comprehensive study for the carbon cycle scientific community. I would also like to thank the authors for this exceptional effort.

Please find below a few comments.

1) Line 10, page 12 : ‘Short-cycle carbon emissions - for example from combustion of biomass - are not included.’ Which short-cycle emissions from combustion of biomass are you referencing exactly and why not including it?

2) Are the fraction of wildfire emissions included in your ELUC emissions?

2021 is projected in this study to be a La Niña year (with a reduction to the ocean sink, page 36), which has been linked to increase fires severity in 2019/2020 in the Northern Hemisphere due to severe drought. Was La Niña event and its possible impact on ELUC emissions also considered in the 2021 projection? The dry conditions for a non-El Niño year in 2019 are mentioned for “Final year 2020” (3.2.3) but not for 2021 projection (3.2.4).

3) What are the uncertainties estimates of the fossil fuel emission inventories you considered in this study?

4) Page 22, “Multiple inversions [...] were previously tested with satellite XCO<sub>2</sub> retrievals from GOSAT or OCO-2 measurements, but their results at the larger scales did not deviate substantially from their in-situ counterparts and are therefore not separately included”. Which results/studies are you referring to? What are the differences and what are the results at latitudinal scales?

The differences between these two sets of observations are particularly large at latitudinal/regional scale, but even if satellite measurements do not deviate significantly from in-situ data at the global scale, some differences are still present. With the MIP (Model Inter-comparison Project) ensemble, Peiro et al. (2022) found when using OCO-2 v9 that a small difference could be observed at the global terrestrial scale (largest sink from ~1PgC/yr to ~2PgC/yr for in-situ fluxes relative to posterior OCO-2 LNLG fluxes) and at the global ocean scale (largest sink for OCO-2 LNLG fluxes of about 1.5PgC/yr relative to in-situ). This was also observed, even if the difference was smaller, with OCO-2 v7 retrievals (Crowell et al., 2019).

Could this small difference, between these two sets of observations, impact your results if you did not include them separately? Do you think the results could have been different by not separating them, particularly when you looked at latitudinal scales (such as the tropics)?

You mentioned not including separately the three inversions tested with GOSAT and OCO-2 data (and only using in-situ data here, table A4) but in your discussion you mentioned that additional information could have been obtained with inversions assimilating satellite observations, is this not contradictory?

5) Which OCO-2 b10 retrievals (LNLG, LNLGOG...) did the CMS-Flux inversion use?

Did all inversions optimize biosphere and fires, Ocean, and fossil fuels fluxes?

6) The use of satellite observations from GOSAT and OCO-2 with CMS-Flux is new compared to Friedlingstein et al. (2020), where MIROC inversion was used instead. I saw no discussion (even in appendix) of the possible disagreement or agreement between the satellite and in-situ analysis with bottom-up fluxes used here; and how the results (accuracy, uncertainty, ...) could have changed here with a simulation assimilating satellite observations compared to the previous study of Friedlingstein et al. (2020) where no satellite observations were used?

This could explain the high uncertainty and fluxes ranges in the tropics observed with the inversions, for example (page 42), where the previous studies of Crowell et al. (2019) and Peiro et al., (2022), observed more net sources with OCO-2 inversions than with in-situ inversions.

On page 50, you mentioned “Additional information could also be obtained through [...] the introduction of inferred fluxes such as those based on satellite CO<sub>2</sub> retrievals”, but do not go further knowing you used an inversion with satellite CO<sub>2</sub> retrievals.

7) Page 25: 2020 has a global fossil CO<sub>2</sub> emissions 5.4% lower than in 2019. This was probably related to COVID, but it is not mentioned here, for some reason? China has not observed a decline in growth rate compared to other countries, do you have any assumptions/explanations why? In Friedlingstein et al, (2020), the projection of 2020 for China was a decrease in emissions which appeared to be less pronounced than other countries. However, here we don't see a decrease but an increase. How do you explain this difference for China between the two studies?

8) On page 28, the gross emissions are influenced by the temporary decrease in deforestation, which is one of the changes that could explain the decrease in net ELUC emissions over the last few years. However, have not forest wildfires been more intense in recent years? Also, in term of prevision,

studies show that fires will increase in intensity and frequency, so do we expect fires to have a larger contribution in the projection? If not, why?

9) On page 30, You mentioned the consequence of dry conditions from La Niña leading to fire emissions in Equatorial Asia. What about the large and severe fires in Australia which ceased in early March 2020? Additionally, you mention fires severity in the tropics, but the northern hemisphere like California experienced in 2020 the largest fires in Californian history. Why not mention it in this ELUC section (3.2)? I was only able to find this information by accessing the Land sink section (3.6, page 38).

*Technical comments:*

Figure 2, could it be possible to have a better quality figure?

Page 12, line 9, the meaning of UNFCCC is needed for those who do not know what it is (Like reviewer #1 mentioned, a lot of acronyms definition are missing).

Page 12, BP is mentioned without information on the abbreviation meaning.

Lin 9 page 12, 'UNFCCC Annex 1', could not find Annex1 in the manuscript, so if this is from the UNFCCC report, the reference is missing here.

Page 14, line 16: DGVMs is not defined.

Page 15, line 1, FAO is only defined page 178 but not in page 15.

Page 18, line 15: In table 4 and table A4, it seems there is 8 ocean based data-products and not 7.

Page 23, line 22, CH4 should be CH<sub>4</sub>

page 36, line 9, La Niña need an accent.