Response to reviewer 2

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.

Thank you

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?

CO2 emissions from combustion of biomass are included in the land use change (ELUC) part of the carbon budget (see section 2.2).

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).

Wildfire are not included in ELUC as ELUC only treats with human induced land use and land cover changes. Natural fluxes such as wildfires are part of the land sink (SLAND). However, deforestation and/or degradation fires are inlcudd in ELUC and those are also subject to year to year variability induced by climatic conditions (more human fires during drier years).

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

Unclear what this means, since we consider one inventory with its uncertainty reported in section 2.1.

Page 22, "Multiple inversions [...] were previously tested with satellite XCO_2 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?

We thank the reviewer for the elaborate question, and for bringing the Peiro et al paper to our attention. It is an impressive analysis that we can impossibly match, or repeat, with our set of model outcomes. Nevertheless, we would like to reply to some of the points made. The sentence on XCO2 results not deviating from their in-situ counterparts on the larger-scales of GCP is based on our own analysis of the resulting flux ensemble for GCP. This analysis is part of the annual written synthesis by the modelling team, which is not published by itself. Instead, it serves as the basis of our presentation of the inverse GCP flux ensemble in the paper. In the GCP paper we typically only discuss the ensemble and refrain from highlighting specific models, or specific model-model differences. In the mentioned elaborate synthesis, we found the fluxes on tropical-NH-SH scale for the models with more than one solution to be similar enough to not warrant introducing them as a separate member in the GCP ensemble. From that perspective, we agree with the contradiction in the discussion that the reviewer points out. To make our point less ambiguous we suggest changing the last statement to: "... Additional information specifically on smaller scales not currently analysed in GCP (but see Peiro et al., 2022) could be extracted from inversions using satellite data." Note that the reverse request: to analyse the differences between inversions with- and without satellite data in more depth, is beyond the scope of the GCP inverse exercise. This research is much better addressed in specific studies that systematically investigate XCO2 constraints on carbon fluxes across more models. In GCP, the time period analysed exceeds the XCO2-era by far and also only a few models explicitly include/exclude these data for the past years. Also, GCP does not have protocols for how to use XCO2 nor which retrieval versions to assimilate, in contrast to OCO-MIP. To try to come to other conclusions here based on our set would not be feasible, nor credible.

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?

The information requested here is provided in Table A4, where we list the specific data constraints and inversion details of each model. In short, the models do not all optimize the same variables and in fact none of them have estimated fossil fuel fluxes.

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_2 retrievals”, but do not go further knowing you used an inversion with satellite CO_2 retrievals.

Indeed, we do not separately discuss the introduction of CMS flux and its impact on the ensemble, and we do not analyze any changes in the flux distribution that CMS-flux might have caused through the use of OCO-2 data. The reason for this is twofold: (1) that in all the analyses we performed before our synthesis, the CMS-fluxes are never an outlier or remarkable change from previous fluxes we presented in 2020, or before. It's land/ocean distribution, NH-Tropics-SH

distribution, its agreement with the global growth rate, and agreement with aircraft data all fall within the current (and previous year's) range of fluxes in GCP. (2) is that we typically do not discuss individual models, or model-model differences in the paper, but instead focus on the budget constraint suggested by the ensemble. The remark on page 50, of the possible extra information to gain from inversions with satellite CO2 retrievals will be changed as indicated in our reply to the previous remark. Thank you for pointing this out.

Page 25: 2020 has a global fossil CO_2 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?

Indeed, the decline is due to the COVID-19 pandemic, we clarified this again here. The difference between Friedlingstein et al 2020 and the estimates here is that the former were our projection based on 4 different approaches. Here we report the actual 2020 estimate based entirely on reported emissions from countries. Our projection for 2020 emissions from China was too low compared to their actual emissions.

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?

In general, wildfires are considered in the natural land sink term unless they are associated with a land cover change (e.g. deforestation), or clearly attributable to human intervention (tropical forest degradation/peat fires). Andela et al., Science, 356, 1356-1362 shows a deadline in the global area burnt over recent decades, albeit with large regional variation. This was attributed to agricultural expansion and intensification. Therefore it is not a priori clear the direction of the near-future trend in biomass burning. Although Zeng et al. suggest an increase in emissions despite a reduction in burnt area, DOI: 10.1126/sciadv.abh2646

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).

The severe fires between end 2019-early 2020 are indeed mentioned in the manuscript on P39 line7. Wildfires, ie those that do not result in a change in land cover (e.g. deforestation fires) or are clearly attributable to human intervention (tropical forest degradation / peat fires) are considered in the natural land sink and simulated by several of the ensemble of DGVMs.

Technical comments:

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

Done, hope it's better now.

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).

Done, thank you

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

BP used to be an acronym, but it hasn't been for many years. But we now write " BP energy company" to make it clear.

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.

UNFCCC Annex 1 countries is a list of 43 countries, classified by the UN as "industrialized countries and economies in transition". We clarified the sentence.

Page 14, line 16: DGVMs is not defined.

Done, thank you

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

Done, thank you

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

Indeed, there are 8 ocean based data products but only 7 are used to calculated the ocean carbon sink. This is clarified now.

Page 23, line 22, CH4 should be CH_4

Done, thank you

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

Done, thank you