Interactive comment on “Was Australia a sink or source of CO_2 in 2015? Data assimilation using OCO-2 satellite measurements” by Yohanna Villalobos et al.

The manuscript by *Villalobos et al.* (2021) evaluates terrestrial biosphere carbon dioxide (CO_2) fluxes in Australia for the year 2015. This analysis was conducted with a regional-scale inverse modeling framework which assimilated Orbiting Carbon Observatory-2 (OCO-2) retrievals of column-averaged CO_2 (XCO2). The main result of the study was the larger biospheric uptake of CO_2 in Australia during 2015 compared to years prior. The study suggests that the main biomes causing this larger update were the northern savannas, Mediterranean regions, and sparsely vegetative areas. Additional information is evaluated to suggest processes which caused these anomalous fluxes. The article is relatively well-written, results are novel and presented effectively, and overall conclusions are interesting. This study is also commendable in the fact it addresses an increasingly important frontier for using OCO-2 retrievals to derive sub-regional biospheric CO_2 fluxes. The study design and results are appropriate for the journal; however, in the current form, I can not recommend this paper for publication in Atmospheric Chemistry and Physics (ACP). As described in the comments below, many key features in the observations and modeling framework, which could have significant impact on the model results, are not described/evaluated in sufficient detail. It is a concern of the reviewer that these oversights could have influenced the model results which is heavily relied on in this study. I do however feel that if the authors can sufficiently address the major comments presented here that it could potentially be published in ACP in the future.

Minor Comments

1. Line 7. “the Mediterranean ecotype” instead of “Mediterranean”.

2. Line 12. “concentrations”.

3. Line 29. “(CO_2)”.

4. Line 48. “to the period”.

5. Line 110-111. “Haverd et al. (2020) ran…”.

6. Line 267. I think the authors want to refer to Fig. A1.

7. Figure 6. Please use the same y-axis values for all figure panels to avoid any unnecessary confusion.

Major Comments

1. To help provide some background/estimate about the uncertainty of global inverse model estimates of biospheric CO_2 fluxes in Australia when assimilating satellite and in situ data, in addition to the text provided already in the introduction section of this study, the authors could

access gridded results of the version 9 OCO-2 Multi-model Intercomparison Project (MIP) (https://www.esrl.noaa.gov/gmd/ccgg/OCO2_v9mip/index.php). This data set provides prior and posterior estimates of Net Biome Exchange (NBE) from up to 10 global models for the four-year interval of 2015-2018. This data set could have also been used to further compare to, and evaluate, some of the results of this study. This study does compare the results to 5 global inverse models. However, the OCO-2 MIP is a controlled experiment which can help with interpreting results due to specific processes (e.g., transport model, spatial resolution, a prior fluxes, observation modes, etc.).

One thing that should be taken into consideration, which has been demonstrated with OCO-2 MIP results, is that notable differences in terrestrial carbon flux estimates are derived depending on which transport model (e.g., GEOS-Chem or TM5) is used for the inversion. Using a single transport model (i.e., WRF) could result in biased biospheric CO_2 fluxes simply due to a specific model's transport errors. Using a model ensemble, such as that derived by the v9 OCO-2 MIP, can help better understand these potential biases. This is just a suggestion to the authors to provide a data set to help interpret results of this study and should not be considered a requirement for application here.

2. The authors state they use "fixed patterns" for initial and boundary conditions and then solve for scaling factors. Can more detail be provided about this? Boundary conditions can be very important for the accuracy of regional-scale inversion estimates for long-lived species. First, how large is the domain used in this study? This information would be good to present to the reader prior to discussing the boundary conditions. Are the boundary conditions provided as daily, monthly, seasonal, or annual averages? Are the scaling factors derived hourly or daily to reflect variability in the boundary conditions of CO_2? It is difficult to understand what exactly the authors did for this. Also, what is meant by the upper and lower areas of each quadrant? Please provide actual altitude or pressure levels which separate these areas.

It would be very helpful if the authors could provide some information about the sensitivity of the results of this study to the boundary conditions used in the modeling framework.

3. It sounds as if the prior biospheric CO_2 model fluxes did not cover the entire domain investigated in this study. Once again, it would be helpful for the reader to know the domain dimensions prior to this discussion. For the areas not covered by the BIOS3 product the authors incorporated monthly biosphere fluxes from Australia CABLE-POP global simulations. What spatial resolution is the global model provided at? How different are the global CABLE-POP results to the fluxes derived from the BIOS3 product?

4. Why did the authors decide to exclude small fires in their prior biomass burning emission inventory? The authors should provide some reasoning for this. Do small fires not contribute much to the overall biomass burning emission total for Australia? How were the GFED emissions scaled to an hourly resolution? Also, are biofuel emissions considered in the prior CO_2 flux estimate?

5. The description of how prior flux error/uncertainty estimates for all flux sources is missing. Also, what flux sources are constrained in the CMAQ simulations? Are all CO_2 sources and sinks allowed to be adjusted when assimilating OCO-2 data? What spatial and temporal resolution are these flux constraints calculated at? Does the domain include oceanic regions? Are these ocean fluxes allow to be adjusted? Given that Australia is both upwind and downwind of oceanic regions, these emission fluxes will have a large impact on XCO2 values over the Australian continent. This needs to be described in more detail.

In the results section the authors do state that they "do not allow much freedom for ocean fluxes" implying that ocean flux prior uncertainties were set to be small values. This could greatly impact posterior land flux estimates due to the fact Australia's XCO2 values will be significantly impacted by ocean fluxes.

6. Details about how the observational error/uncertainty matrix was calculated (e.g., transport error, model-data mismatch, etc.) needs to be described as well. Are individual soundings of OCO-2 retrievals used for comparison to the model, or is there some temporal/spatial averaging? The authors point to a past study for these methods, but the way OCO-2 data is treated in this study is important enough to be presented in this manuscript.

7. How do the authors extrapolate the vertical CO_2 profile in CMAQ above 50 mb? Will some of the offset in the model-data XCO2 comparison be due to the fact the model does not account of this part of the vertical profile? Satellites (e.g., OCO-2, GOSAT) have sensitivity to this stratospheric CO_2 and will contribute to the overall retrieved XCO2 values.

8. A major result presented by the authors is that "either prior uncertainties or observational uncertainties were too high" in the model setup used in this study. Can the results of the posterior fluxes be trusted due to this? Just because the posterior CO_2 concentrations compare better to the observations, compared to the prior, does in no way mean that the posterior fluxes are more accurate. The authors need to expand upon this and provide evidence of why the posterior fluxes are realistic.

9. When the authors compare monthly and annual terrestrial CO_2 fluxes, do these include biomass burning emissions? Were fires anomalous in 2015 compared to prior years? Were the fires adjusted significantly due to the assimilation of OCO-2 observations? This is an important point because if prior fire emissions are not treated correctly in the inversion, and the fires were significant different during this year, the inversion could bias the land sink high or low. Something similar could be said for oceanic fluxes. The authors should present values, and spatial maps, for the source attribution of prior and posterior CO_2 fluxes for the domain. Since the description about what fluxes were constrained in the inversion, and how the individual source prior errors were attributed, it is difficult to understand and trust the results of the study.

10. Are the 0-50 mm rainfall anomalies for southeast Australia in July 2015 significant? What is the fractional increase in rainfall for this region this equates to? There are regions of Australia in the same year that received up to 700 mm more rainfall in a respective month. Also, depending

on when the rainfall was occurring in July, it might not even have much effect on the EVI values and could be impacted by months prior. It appears June 2015 had a slightly higher anomaly in rainfall in the same region. The authors are quick to attribute the increase in biospheric carbon uptake to increased EVI and rainfall, which may be true, but more analysis/explanation would help.

10. What data is used to derive the six bioclimatic classes used in this study? The authors point to a past study, but the information is needed here.

11. One of the most striking and surprising results is the large increase in carbon uptake in the sparsely vegetated (mainly desert) region of Australia. The authors state this "might be associated with an underestimation of the GPP by CABLE-BIOS3". This is true of course, but why would a sparsely vegetative region, which had decreased vegetation (negative EVI anomalies in Fig. S1) and experienced a negative anomaly in rainfall for much of the year (see Fig. S2), have such large values of carbon uptake? The March – September 2015 posterior carbon uptake values in the sparsely vegetative regions in Figure 6 are larger than any other biome in Australia. Is this not counter-intuitive and highly unexpected? Are the larger values due to very large carbon uptake, or is simply due to the large spatial extent of the biome? Could this be due to the choices in prior flux or observational uncertainties which are known to be incorrect (as stated by the authors earlier in the manuscript)? The comparison between CABLE-BIOS3 GPP and MODIS GPP is helpful but does not explain why the carbon uptake in this sparsely vegetative region is so large. This is a very interesting result, but it needs to be explained and interpreted more thoroughly.

12. There are a couple experimental setups that could just as likely lead to these results instead of it actually occurring in nature. The first thing that needs to be expanded on, and potentially investigated more, is the impact of boundary conditions on the inversion. Small errors in the boundary conditions can have large impacts on regional-scale inversion models. This is evident in Figure 7, as very large adjustments in posterior land fluxes had only small impacts on the XCO2 values (typically ~0.25 ppm) at the TCCON locations. These same results could be simulated if you had ~0.25 ppm or more errors in boundary conditions and had too small prior error uncertainties or did not adjust boundary conditions correctly. Can the authors provide evidence this is not the case? Knowing that observational and prior error uncertainties were not set correctly, how can the mean posterior fluxes values, and spatial distributions, be expected to be accurate enough to make the claims in the results of this study? Also, the authors clearly state that prior uncertainties are too stiff for ocean fluxes, how do we know that inaccuracies in the ocean prior aren't being redistributed to posterior land fluxes? All three of these concerns could easily lead to similar results/conclusions presented in this study.

13. Could this study have assimilated in situ data to infer CO_2 fluxes in Australia? It would be interesting, and perhaps provide more confidence in the results presented here, to see if in situ data assimilation results in similar conclusions compared to inverse model estimates using OCO-2 XCO2 data. Are there any other sources of in situ measurement data in Australia besides the data applied in this study for model evaluation?

14. Have the authors conducted the inversion using OCO-2 XCO2 data for other years than 2015? It would be interesting to see if the model results had any inter-annual variation. Was 2015 selected simply to see if the El Nino had impact on Australia carbon fluxes? If 2015 was in fact an anomalous year, it would be interesting to see if the model framework would not simulate the larger biospheric uptake for later years (e.g., 2017). This could help increase the robustness of the conclusions of this study.