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Comment on acp-2021-16

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Community 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., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-16-CC1, 2021

This study uses a state-of-the-art regional atmospheric CO_2 flux inversion system to assimilate column-average CO_2 measurements from the OCO-2 satellite over Australasia. Monthly and annual mean flux estimates for Australia, as well as six eco-climate sub-regions inside it, are analyzed for the calendar year 2015. The results for the domain as a whole are compared to similar estimates from six global flux inversions (which, however, used only in situ data instead of the OCO-2 data). To help interpret the results, vegetation greenness information in the form of satellite EVI data, as well as rainfall products, were examined. Since the results are presented in terms of deviations from a carbon model prior, deviations of the GPP given by this model from GPP estimates derived from the MODIS instrument are also examined. The main finding is that the OCO-2 data indicate that the annual mean flux for Australia for 2015 was -0.3 PgC/yr, a substantial sink, as opposed to the small source of about +0.1 PgC/yr given by their carbon model prior or the substantial source of +0.6 PgC/yr given by the mean of the six global inversions. (Though see below my question about the latter number -- it may be closer to +0.1 or +0.2 PgC/yr if the monthly fluxes plotted in Figure 11 are correct.) This result suggests that the OCO-2 measurements cause the land biospheric flux for Australia to change from the source given by the in situ data into a robust sink.

The paper is well-organized and well-presented. It should not, however, be published in its present form, for the following reasons: 1) It is not clear from the work presented here that the main result, this shift of about a half PgC/yr or more from source to sink in the 2015 annual mean, is real and not simply an artifact of local biases in the OCO-2 data, or of the impact of the coverage of the OCO-2 data on the regional inversion (i.e., the partitioning of the OCO-2 information between fluxes in the interior of the domain versus fluxes outside the domain, interpreted here as errors in the boundary conditions). And 2) going beyond this shift in flux to look at the absolute values of both the prior and the posterior fluxes for the region, further uncertainty is introduced by potential errors in the transport model used here, the prior concentrations used (in particular, the CO_2 gradients produced by running the prior fluxes through the transport model combined with the global offset needed), and the impact of drift in the regional inversion (the fact that the constraint on the land + ocean CO_2 flux total obtained in the global inversions from the difference of the fossil fuel input and the atmospheric increase is no longer available in the regional inversion). One cannot rely on the errors obtained from the OSSEs done here to capture these error sources, unless the OSSEs somehow captured all the transport model, inversion method, and measurement bias errors along with the random measurement errors that are usually addressed (terms that were not mentioned in the text and that I doubt were addressed). This reviewer was not convinced that the sink obtained was not simply an artifact of one of these systematic error sources.

By focusing on the annual mean fluxes for this region, the authors are going after the toughest part of the estimation problem, more difficult than getting the seasonal cycle of flux correct, for example. It has been shown that errors in representing in situ measurements (especially those affected by continental air) in the global transport models result in measurement biases that get translated into annual mean flux biases in the global inversions. The early TransCom comparisons of regional fluxes from different global inversions regularly showed timeseries that moved up or down together with a similar seasonal and interannual variability, but were separated by offsets caused by these biases. Regional inversions are affected by the same sort of errors through the boundary conditions assumed (taken often from these same global inversions). The shift from the mainly surface-based in situ data to column-integrated measurements from satellites should reduce the bias problem simply by having a much denser and globally-distributed data constraint. However, counter-balancing this are two added problems: 1) biases in the retrieval of CO_2 from the measured radiances (these are on the order of a half ppm in the column at the scale of continents and seasons), and 2) the need to rely on transport models more to relate the information in the upper part of the column to fluxes far afield. Were it not for the retrieval bias problem, in particular, more satellite-based flux inversion results of the sort of study being done here would have appeared in the literature before now.

In this study, some explanation ought to be given of why only OCO-2 measurements collected over land were used, and not those over the ocean. On the surface, one would think that those over the ocean would be of great use in estimating fluxes for a small continent completely surrounded by oceans. We suspect that this is due to suspected biases affecting the OCO-2 ocean glint data. But if those data are affected by biases, how do we know that the retrievals taken over land that are used in this study aren't also affected by such biases? At a minimum, looking at flux inversions of both sets of data assimilated separately and comparing the difference between them would allow the authors to give some rough quantification of the impact of biases in the OCO-2 data. That would help place the magnitude of the shift to more uptake presented here in better context.

The comparison of the a posteriori CO_2 concentrations obtained here to independent data is a strength of the manuscript. However, the systematic differences of 1 ppm or more over half the year that are obtained when comparing to the three local TCCON sites, as well as the half-ppm difference of opposite sign obtained in comparison to the in situ data at Cape Grim, do not give one confidence that the annual mean fluxes are correct. Differences of half to 1 ppm are not negligible and are the sorts of differences that a half or full PgC/yr error in the annual mean for the region might cause. Not knowing whether the flux estimate should be pushed more in the direction of the TCCON or Cape Grim data is also obviously a problem (i.e. we don't know whether our validation data themselves are reliable).

For this paper to be publishable, the authors should either back off from the emphasis on the absolute flux levels and focus instead on variability, or else add the sensitivity studies necessary to quantify the uncertainty in the annual mean fluxes related to the systematic errors mentioned above. Even if the first approach is taken (focusing only on variability, meaning here the seasonal cycle), some quantification of the impact of the systematic errors on the flux variability would still be required. One approach that might help with this would be to look at the comparison of OCO-2 flux estimates from a suite of different global inversions collected as part of the OCO-2 flux inversion model intercomparison project (MIP). Fluxes obtained for the Australia or Australasia region from those runs could be collected and the spread used as a measure of some of these sources of uncertainty (the different estimates in the suite having been obtained from inversions that used different transport models, prior fluxes, and inversion methods). The key issue of biases in the OCO-2 data could perhaps be examined also using these MIP results, as well as by doing an inversion using the OCO-2 ocean glint data inside the regional domain. Another approach might be to try to use a larger domain (with the inversion run at a coarser resolution) to see how much the annual mean flux result changes. All this is unfortunately a lot of work, but it seems to be needed to get an idea of how robust the sink obtained here actually is. It should be noted that this applies to not just this study, but to regional inversions of satellite CO_2 retrievals more generally.

Finally, I would offer my opinion that regional inversions of satellite CO_2 data are not the ideal way of handling these data, since much of the information contained in the column pertains to fluxes emitted outside the domain (and conversely, much of the information useful for constraining fluxes inside the domain, especially their annual means, is contained in satellite measurements taken outside of the regional domain). A global inversion that uses all the data together, perhaps with a high-resolution model nested over the region of interest, would be a better approach. All this is due to the full-column nature of the satellite measurements -- regional inversions do much better when the data are taken close to the surface and reflect local fluxes more directly. If the authors are intent on using a regional inversion for this problem, they should do a better job of quantifying the uncertainty in their flux estimates due to their treatment of the boundary conditions.

Detailed comments (for line numbers indicated):

4: Does a negative sink indicate a source? Be careful to distinguish between fluxes (which can be + or -) and a source or sink, which should be unsigned (the sign of the flux being indicated by whether it is called a source or sink)

15-16: I'm glad you bring out this difficulty in matching both column and surface data in the presence of transport model error -- this is often ignored.

115-124: It is not clear which fields EDGAR estimates are being used for -- just the aviation and marine transport emissions missing from ODIAC? Please clarify in the text.

232-233: The explanation given here for the a posteriori cost function values being too high by a factor of two is correct IF the errors are unbiased and gaussian. Another possibility that ought to be mentioned is that they are not gaussian and independent but rather have errors correlated in time and space (including flat biases) that render the statistical assumptions made in deriving the estimation method invalid and lead to a higher cost function than expected.

233-234: "To compensate, the posterior uncertainties estimated in Villalobos et al. (2020) were increased by $\sqrt{2}$." I think you mean prior uncertainties instead of posterior, no? Also, please indicate whether the uncertainties on the measurements were increased in addition to those on the prior fluxes.

264: the prior overestimates the measured CO_2 when?

265: Are these posterior-prior differences that you refer to FLUX differences or differences in CO_2 concentration? If the latter, are they X_{CO2} or something else? [Figure B1g does not relate to either, so I can't tell...]

267: The figure pointed to here (B1g) only shows the number of OCO-2 data points over the domain in July 2015. It is not at all clear how that relates to the posterior-prior differences referred to in this sentence. Why are you pointing to that figure? Do you mean A1g?

272: "...most of the increased in the posterior flux" By "increase in the posterior flux", do you mean a shift towards more positive values, reflecting the greater prevelence of red values in SE Australia in A1h & i? Or an increase in the uptake from the previous month (blue values in A1)?

273: "(Supplementary Fig S1, panel g, h)" The flux changes are shown in Figure A1 -- do you mean to point to that figure, instead?

273-275: "...which again lines up with a higher than usual increase in land productivity." The largest signal I see in SE Australia in Aug-Sept 2015 in Fig A1 h&i are the (red) positive flux anomalies next to the coast, which seem to overwhelm the (blue) negative anomalies further inland. These cannot be explained by the higher EVI values in your argument.

274: "Positive EVI anomalies in this period was not as strong as in July."
To my eye, the strong EVI anomalies seen in July (Fig S1g) more or less continue at the same magnitude, or very close to it, in the following two months (S1h & i) -- and yes, perhaps waning a bit in September.

274: By "these findings", do you mean the reduction in EVI? That makes sense, but the sitll-strong uptake in the south from your inversion does not agree with it... Maybe you could reword this to be clearer?

277-278: "Spatial patterns of the EVI anomalies during these months are expected because rainfall is one of the most important drivers of ecosystem dynamics and productivity."
Agreed, however, the agreement between your EVI plots and your rainfall plots does not really support this well. In particular, in the August-September time frame that you are discussing here, most of the EVI anomalies in SE Australia are positive (green) at a time when the rainfall anomalies are switching to negative (dry). The two areas in southern Australia in September that have negative EVI anomalies (orange) are both located in areas of positive rainfall anomalies. Basically, the argument you make in the text (which is reasonable) is not well- supported by the figures you point to.

294-306: In this whole discussion of the savanna fluxes, it surprises me that you don't point at all to your EVI or rainfall maps in the supplementary material. The strong rainfall anomaly in January over the savanna in the north supports your argument, as does the positive EVI values there in the same month. However, these favorable growth conditions quickly change to unfavorable ones in February (strong negative EVI values (red) broadly across the savanna in the NE, and dry conditions starting in March) and stay unfavorable all the way through September, a period when your inversion is giving you an anomalous uptake there the whole time. To make these two lines of evidence agree, you would have to argue that the benefit of the wet conditions in January allow increased growth and carbon uptake that laata all the way through the dry half year that follows.

307-312: Similarly for the warm temperate region, your finding of reduced C uptake in the posterior does not agree with the EVI data, which show increased greenness across the region for most of this time.

313-321: For the cool temperate region, there does not seem to be any indication in the EVI or rainfall data of any strong driver for these shifts to C release that you obtain in your inversion. For this region, as in those above, your posterior flux estimates don't seem to correlate well with the EVI or ranifall metrics. At least you bring in the MODIS GPP metric and discuss errors in your CABLE-BIOS3 prior, which is relevant because you discuss your results in terms of a shift away from the prior.

325: Here, the rainfall and (especially) EVI maps do support the greater uptake in August. I would point also to the EVI map, as that is more convincing than the rainfall map.

350: I could not find the indicated Table A1 in Appendix A -- where is it?

345-353: This whole discussion of the biases with respect to the Darwin TCCON site seems a bit beside the point. The broad view is that there are very large seasonal differences between the TCCON estimates and both the prior and posterior estimates from the inversion, and that assimilating OCO-2 data does not significantly reduce them. Some discussion of why this might be the case and what it means is warranted. The OCO-2 data themselves are bias-corrected using the TCCON data, so why shouldn't they drive the result to agree with the TCCON data? (One answer: there are still site-to-site biases between OCO-2 and TCCON, some of which we may be seeing here.) Could the difference be due to errors in the TCCON retrievals, maybe? If the positive bias is due to improper treatment of fire carbon emissions in the prior, as suggested, why wouldn't the posterior estimate correct these strongly (the inversion assimilating column CO_2 data, which should be less sensitive to the vertical location of the emissions)? The TCCON data also see any fire emissions in the column, again being less sensitive to the emission level.

357-364: Again, in the discussion of the Wollongong site, the key point is not captured: strong systematic differences between TCCON and the OCO2-driven inversion remain, even after the OCO-2 data are assimilated. Why? A 1 ppm difference extending across over half the year is not a "small" difference in terms of what it implies about flux errors.

362-364: There is little suggestion in the figures that are pointed to that the large difference in August at Wollongong is related to data density -- the winds are mostly coming from the west then, where the OCO-2 data are located (i.e., over the continent).

378: Why would winds blowing off of the ocean necessarily lead to negative biases? Are you saying that there is some problem with your prior CO_2 fields over the ocean? What is causing that?

380-384: It is argued that the difference at Gunn Point is due to a high bias in the in situ data, a single high reading possibly related to fires. However, in Figure 9a, the absolute values for both the in situ and estimated CO_2 levels are given, and the July in situ value seems to be in line with seasonally-expected values, lying in between the June and August values. It is the estimated CO_2 value which is the outlier. So the explanation given does not agree with this.

402: "...savanna, Mediterranean and sparsely vegetated ecosystem drove this higher posterior uptake." I would suggest removing the Mediterranean region from this list. The tropical region actually had a larger shift from the prior, yet you don't include it. Only July saw a significant change from the prior in the Mediterranean ecoregion...

411: The annual mean for the ensemble of global models that you give (+0.63 PgC/yr) does not seem to agree with the monthly numbers you give for the ensemble mean in Figure 11b. If you add up the monthly numbers in 11b and divide by 12, you get something more like +0.1 or +0.2. Which is correct, the annual mean you give or the monthly numbers plotted in 11b?

415: It is not clear where the seasonal amplitude of 0.22 PgC/yr for the global models comes from. Based on what is plotted in Figure 11b, it should be more like 2.7 PgC/yr. Are you sure you didn't accidentally divide by 12? The 3.46 number for the inversion results also looks a bit high, based on what is shown in Fig 11b.

423-424: "Monthly biases of simulated concentrations compared to TCCON sites at Darwin, Wollongong and Lauder generally improved using posterior rather than prior fluxes."
I would have to disagree -- based on what you have shown in your plots, the improvement is only very slight. Mostly, the seasonal biases that are there in the prior remain there in the posterior.

424-428: To me, this seems like an overly rosy view of the results. It seems to me that there are persistent biases between the results of the inversion and the independent data: the CO_2 values are too low across the year when compared to the three TCCON sites (except during the austral Spring for the two sites over Australia), and too high when compared to the Cape Grim in situ site. What causes these biases, and why are the inversion results lower than TCCON in the column, but higher than Cape Grim near the surface? For a paper trying to pin down the absolute level of annual mean flux for this region, these systematic biases with respect to the available independent data are a big cause for concern. A 1 ppm difference in the column average is not a small bias, nor is a 2 ppm difference at a background site like Cape Grim.

432-443: The potential of higher-resolution inversions noted here is well taken. However, the overall result of an annual mean uptake for 2015, as well as the factors affecting how it was obtained, have more to do with broad-scale features of the inversion that would not be substantially changed by going to finer resolutions. First, the data used (column data only over the land areas, mostly over Australia) -- local biases in this, perhaps related to the near-desert nature of much of the area, might factor into the result. What would your inversion give if you included the OCO-2 ocean glint data as well? Yes, that data might be affected more by retrieval biases, but you could solve for that in the same manner that you correct for biases in the boundary fluxes coming into the domain. Second, the short span of the inversion -- how important are errors in the initial condition to the result you obtain? If you vary your prior for the period leading up to the start of 2015, how do your annual mean results change? Third, and probably most important, what is the impact of using only a small amount of column-integrated satellite CO_2 data in a regional inversion of this sort on constraining the local fluxes? Most of the CO_2 information in the mid- to upper-column comes from fluxes emitted outside of the regional domain -- only the lowermost part of the column contains information on the local fluxes. How do you ensure that errors in modeling the upper part of the column (in terms of your prior fluxes and CO_2 fields, and your atmospheric transport) do not overwhelm the actual local information that you do have, resulting in spurious flux estimates? Sensitivity studies are crucial for handling these sorts of issues, and you really have not done any of those here. Also, given the importance of transport model errors in inversions of this sort, getting an idea of the model-related errors is important. One approach you could take to address this is to see where your annual mean results for Australia fall within the suite of inverse modeling results given by the OCO-2 flux inversion MIP (see Crowell et al, 2019 for the OCO-2 v7 MIP and Peiro et al. for the v9 MIP, the latter having just been submitted to ACPD).

448-449: "In general, they compared well with TCCON data when wind directions coupled our estimated fluxes to these observations." This is a generous interpretation of your flux estimates. In my view, the systematic errors with respect to the three TCCON sites and with respect to the background site at Cape Grimm open legitimate questions as to the reliability of the annual mean flux estimates obtained here.

453-454: This focus on the Mediterrenean ecoregion, when it played a relatively minor role in the greater uptake estimates obtained, seems misplaced.

455-456: "We also noted an increased seasonal cycle of flux, also suggesting greater productivity than the prior estimate." Is the increase in the seasonal cycle amplitude obtained here in comparison to the global models' still significant once the error in the global ensemble mean number (the 0.22 PgC/yr) noted above is corrected?

Figure 4 vs. Figure 11: why are the blue lines on these two plots different? Aren't both of them the net land biospheric flux for Australia given by the regional OCO-2 inversion for 2015?

Editorial comments:

6: I would put a comma after "southern Australia", for clarity

7: it seems odd to refer to "Mediterranean" by itself over Australia -- I would continue to call it "the Mediterranean ecotype" if it were me...

7: define "EVI" here

13: replace "which" with "that" (?)

14: add a comma before "though"

15: standardize your usage of "in situ" throughout document. Earlier you had a hyphen in it. I like it without the hyphen and italicized, but check the ACP preference on Latin words and go with that. No hyphen, in any case...

16: I'd replace "to match" with "in matching"

24: You are missing an end parenthesis for this beginning one

28-29: add commas before "along" and after "CO_2 data"

30-31: add commas around "launched in 20xx" (two places)

33: put "and none in Australia" either in parentheses or between commas

39: "are run at" ?

41: "run at 1 deg"; also, change the comma before "for example" to a semi-colon

43: change "the inversion" to "an inversion"?

44: add a comma before "since"

47-48: please indicate whether the anomaly that Poulter et al found was for the year of 2011 indicated in previous sentence, or some other year. You could just add "in that year" after "-0.66" to do so.

50: "Ma et al. (2016) found that this carbon uptake rapidly diminished..."   It is not clear here whether you are trying to say that Ma et al got a smaller estimate of uptake than the two previously-cited references, or whether they found a similarly large uptake that somehow rapidly diminished across the 2011-2012 time period. Please reword this to clarify.

Footnote on page 2: On this footnote, you could note that when you refer to uptake or emissions, you will still use the signed flux values, so that the reader is not confused by, say, a negative uptake value.

58: remove comma before "and"

59: define "EVI" here and in the abstract

60: replace "against" with "and"

64: add a comma before "we"

68: remove comma before "involves"

70: add "likelihood" after "maximum a posteriori"? Otherwise, does not make any sense.

71: get rid of the parenthesis before "Rayner" and move it before "2019"

76: italicize the second "x"

77: the "b" should be super-scripted, not sub-

Figure D1, caption: You should indicate what quantity these uncertainties apply to. I think you mean them to apply to the annual mean flux estimate across 2015, but you need to indicate this.

95: add a comma before "x" (in addition to the one after it that is already there)

97: When you say here that you solve for scaling factors on the boundary conditions, this conflicts with what you say in the following sentence, where you say that you solve for additive offsets to the boundary conditions. Please clarify this to indicate which of these two options you actually use.

102: add a comma before "so"

111: change to "ran the CABLE model in the BIOS-3 setup"

111: "hourly" does not need to be capitalized

162: instead of interpolating to the "CMAQ vertical profiles", I think you mean to the "CMAQ vertical levels", no?

163: would be useful if you wrote out this equation here in the text instead of forcing the reader to go to Conner et al to get it

165: rewrite this for clarity. They aren't daily averages but rather 1000-1400 local time averages, correct?

168: "in situ"

172: again, use of "daily" to describe a subset of the full day is a bit confusing -- please reword

185: "data that comes from the land" -- it would be better to reword this to "data that was collected over land", because we the origins of the air in the column can "come from" over the ocean quite easily...

213: remove the parentheses around "S76 and S78"

215: Add comma after "Table 3"

Figure 3 caption: What are the units? Also, do the number of measurements in each month change substantially?

237-239: move the phrase "which is about 0.06 PgC y^{-1} (mostly constant

for each month in 2015)" after "over Australia" -- as it reads now, it sounds like this refers to the annual posterior flux, not the fossil fuel.

258: change "are shown" to "is given"

273 & 275: do you mean "h and i" instead of "g and h"? Those are the two panels showing August & Sept.

293: "are shown"

294-295: change to "strengthens to a posterior of -1.05 ± 0.11 PgC y^{-1}".

316: change to "to a posterior of 0.47 ± 0.05 PgC y^{-1}".

331: "suggests also an"

334: "strengthened"

Figure 8a: the title should read "Darwin" not "Wollongong", I think.

374: add a comma after "sites"

380: "OCO-2"

400: "Section 4" -- the "s" should be capitalized because it is a proper name.

412: add a comma before "which"

417: switch "which" to "that"

Figure 11 caption, line 4: correct "ensemble monthly the mean"