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Comment on acp-2022-15

Anonymous Referee #1

Referee comment on "Interannual variability in the Australian carbon cycle over 2015–2019, based on assimilation of Orbiting Carbon Observatory-2 (OCO-2) satellite data" by Yohanna Villalobos et al., Atmos. Chem. Phys. Discuss.,
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The manuscript by Villalobos et al., presents a regional inversion of CO₂ fluxes over Australia, using OCO-2 observations and the CMAQ model. The study is well designed and the inverse modelling approach is sound and well described. Unfortunately, the paper lacks in at least two main aspects:

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The presentation and discussion of the results lacks concision and depth: the authors produced many figures, which are analyzed one after the other, but there is no real effort of hierarchization of the conclusions from these analyses, and their cross-implications are not well explored.

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Specifically, the links between climate anomalies and CO₂ flux anomalies are explored even before (and independently of) the robustness of the inversion results is assessed, which gives the impression that the authors try to fit their results in a pre-existing narrative, rather than verify if their results support it or not.

Despite these negative points, the base for the study is sound and the paper can probably be improved significantly through major revisions of the text of some sections, but without the need for producing new simulations. I give more specific comments further below.

Major comments

The presentation of the results is very lengthy, but I find it poorly organized, and it lacks a hierarchization of the importance of the results and of their interpretations:

- The manuscript presents the results, then uses them to explore links between climate anomalies and CO₂ flux anomalies, and only after that presents comparisons with independent data, and, at the very end, with results from other inversions. The scientific interpretation of the results is therefore done rather independently of their robustness assessment. Furthermore, this “robustness assessment” raises at least some suspicion on the results which, without invalidating them, makes it premature to jump into interpretations.
- There are many (17!) figures in these two sections, but a lot of repeated information from one to the other (e.g. Figures 6, 7, 8 and 16 all show prior/posterior flux anomalies + one additional indicator). Other figures are under-exploited (figures 10 and 11 show roughly the same thing), and information that needs to be interpreted in relation with each other end up on separate figures (Figures 13 and 14). Furthermore, the text is often just a very linear and lengthy description of the figures, and doesn't provide much added value (see specific comments below for examples): the text should guide the interpretation of the figures, not just describe them. This overall makes the paper rather hard to read, because the work of information filtering and hierarchization is largely left to the reader.

Regarding the content itself, there are also at least two major issues, that will need to be addressed in a revision:

- In comparisons with independent observations (surface-based and TCCON), the inversion tends to degrade the fit to independent data (especially at the surface sites). Some possible explanations are mentioned (retrieval biases due to clouds, possible transport model errors, non-representativity of the independent observations), but the implications of these on the interpretations of the results is not discussed. Furthermore, in comparisons with inversions from the OCO₂ MIP project, the CMAQ inversion is quite an outlier, which should further raise at least some carefulness regarding the scientific conclusions that can be derived from these results. On a side note, I missed a discussion on the validity of the boundary condition (whose influence at the observation sites is not even shown in the figures). Given the poor fit to oceanic observations (I545: “all the negative large posterior biases [...] are associated with [...] winds that come from the ocean”), it could very well be a large source of systematic error.
- A lot of focus is put in trying to highlight links between CO₂ flux anomalies and anomalies in weather/climate parameters (temperature and precipitations) and other relevant products (EVI, GPP). However, the link is not that obvious. For instance, in five of the six ecosystems studied, the inversions leads to a reduction of the correlation between C flux and EVI (which could in fact be interpreted as the inversion refuting that link, somehow). Correlations between rainfall and C flux are ≤ 0.16 in five of the six ecosystems, and correlations with temperature are also not very convincing except maybe in the “Cool Temperate” and “Sparsely vegetated” regions. The authors are relatively careful regarding the wording of their conclusions, yet, this link between C flux and climate anomalies seems one of the focal points of the paper (e.g. last

sentence of the abstract). I don't think that these poor correlations should necessarily be interpreted as a refutation of links between climate and flux anomalies, but maybe establishing this link requires more than just comparing time series: there's a reason why we need DGVMs!

Specific comments

- Section 3.1 is basically just a description of Figure 2: I don't need to read what I can already see in the figure, but I would need guidance on how to interpret it: the posterior biases are systematically positive: is that normal? What is causing that huge negative prior bias in November 2017?
- Section 3.2 is also just describing Figure 3. One question about this figure: how do the annual C budget compare between the prior and the posterior?
- Section 3.3.1: How is that "Results"? And again, this is just a (long) description, subplot by subplot and year by year, of the information shown in Figure 6. But what's the take home message of that? Similarly, Section 3.3.2 basically just describes Figure 7 and 8.
- Figures 6, 7 and 8 could easily be merged into one. This would also make it easier to see if maybe there is a combined effect of e.g. temperature and precipitation.
- Figure 10 shows correlations of the fluxes with temperature and precipitations at the pixel scale. The color scheme of that figure is terrible, it's impossible to distinguish a correlation of 0.5 from a correlation of 0.8. Also, the figure doesn't say if the correlations have improved or degraded compared to the prior, which would be required for a proper interpretation.
- Section 3.4 and 3.5: Again, there's no need to describe the figures so extensively, I can see myself that the fit is sometimes improved, sometimes degraded. But is it expected? Does it help understanding what you showed in Figures 2 and 3? What implications does it has for the relationships between flux and climate parameters that you looked at in Section 3.3?
- Figures 10 and 12 are quite hard to read (too much information with the error bars). Figures 10 and 11 are maybe redundant (and same for 12 and 13).
- Section 3.5, l518-524: "One possible explanation [...] vertical transport of the CMAQ model": why isn't this discussed more? This degradation of the fit to surface observations proves that at least something is going wrong in your inversion. Maybe it can be ignored, but then you need to justify this!
- Figures 14 and 15 need to be merged. It's really difficult to jump from one page to the other to understand the link between the two, and they are not that useful on their own (well, Figure 14 is, but then it should have been shown much earlier).
- In Section 4, there are a lot of comparisons with GPP from other data products (the prior CABLE BIOS3, the DIFFUSE model, MODIS data) and with other inversions: this is useful, but again, it needs to be connected together and to the rest of the manuscript and to broader research questions. There are efforts in that direction, but it needs to be more refined. For now, it still comes up quite a bit as a long list of comparisons, analyzed one by one, rather than as pieces of a larger puzzle. What's are the scientific questions that the three subsections try to address? How robust are these discussions, given what has been seen in other parts of the paper?
- Appendices: I think most of it is superfluous (what's the added value of showing 60 plots of spatial distribution of the OCO2 soundings vs. showing e.g. one example month or year?)