

Review of “A climatological description of atmospheric CO2 concentrations” by Bréon et al.

General Comment

This study presents a method to produce climatology of atmospheric CO2 distributions based on the atmospheric inversion model outputs (CAMS) constrained by the worldwide observations. The authors assessed their method against AirCore and TCCON measurements. They also discussed limitation and possibility for use of their product as a prior of a satellite retrieval.

My overall assessment is that this manuscript concisely documents technical methodology, but its scientific validation could be improved. With its concise and well structured descriptions, this manuscript would be a practical publication that supports a satellite CO2 observation mission and overall direction and discussion are sound. In this regard, I realized that this study is more like a technical paper. This is also because of limited novelty in terms of the method and scientific finding. In my opinion, additional validation is needed for this study to be considered for ACP.

In the revised manuscript, we have added some evaluation (against NOAA AirCore and TCCON, and provide additional scientific discussion

As the authors acknowledge in the manuscript, historical efforts have been made for mathematical representation of atmospheric CO2 variations (e.g., Thoning et al., 1989; Nakazawa et al., 1997; Pickers and Manning, 2015, reference therein). In addition, the Mauna Loa data have been used as the CO2 reference to demonstrate climatology of CO2 variations horizontally and vertically, because CO2 concentration at the site gives approximation of the global average, though slightly northern hemisphere biased (Sweeney et al., 2015; Umezawa et al., 2018). The authors take the approach similar to these earlier studies, but it does not seem to me that this study provide a new scientific insight into CO2 variations.

We strongly believe that the additions of surface observations, improvements in the atmospheric transport modelling and assimilation procedures have led to a significantly better descriptions of the atmospheric CO2. Besides, the recent development of AirCore offer new tools for evaluations that were not available for earlier analysis.

I also note that guidance to the conventional understanding from the earlier studies is insufficient, by which it is likely difficult for readers to understand underlying scientific consideration about why the authors took the approach in this study. For instance, the authors could explain why the site Mauna Loa is chosen in the scientific context with the aforementioned earlier studies. This is also the case for the sites American Samoa and Amsterdam Island mentioned in the manuscript.

In the revised manuscript, we add some discussion on the rationale for using these stations to quantify the global growth of atmospheric CO₂. We also discuss and quantify the potential improvement of using a different station for the southern hemisphere

Another component that could enhance this study is validation for the other regions. The AirCore dataset over France is an excellent contribution. As the authors explain, however, the data are from an area over which atmospheric CO₂ undergoes typical mid-latitude seasonal variation with a moderate amplitude. While the authors write that longitudinal variations in the atmospheric CO₂ are relatively small (in introduction), I think that, throughout the manuscript, they tend to simplify atmospheric variations and underestimate the spatial variability. Aircraft observation studies have reported large variations in atmospheric CO₂ in latitude, longitude and altitude (Sawa et al., 2012; Sweeney et al., 2015; Umezawa et al., 2018). The magnitude of variability in the northern hemisphere is also large enough (> 2 ppm) to be considered in this study. In the northern hemisphere, variability (or deviation from the climatology) is likely larger over eastern North America (Sweeney et al., 2015), over East Asia and South Asia (Umezawa et al., 2018) and over Siberia (Lloyd et al., 2002; Sasakawa et al., 2013) than over France. Considering this, comparison to the AirCore over France data would not provide validation at the continental to hemispheric scales. It should be also noted that the AirCore measurements were made over the area where the data density is high and CAMS are relatively well constrained. Nevertheless, discussion goes to use of the approach for satellite retrievals, where global coverage is the key. Here I see a significant gap to justify value of this study. Additional validation could be made by using available aircraft data: NOAA (Sweeney et al., 2015), CONTRAIL (Umezawa et al., 2018) and IAGOS (Filges et al., 2015).

In the revised manuscript, we use the NOAA AirCore that provide vertical profiles over different areas of the globe. The results are shown in Figure 4. In the Boulder/CO area, there are a number of profiles that is similar to those over France for a good statistical representativeness. There are also a large number of observations close to Lamont, but not over other areas of interest. The manuscript discusses these additional evaluations.

Note also that the TCCON offers a much better spatial representativeness

Regarding the limitation of this study in terms of spatial representativeness, title of this manuscript reads too general. I would suggest that it could be more objective specific. I have to point out that general scientific interpretation of the atmospheric CO₂ climatology has been discussed in more detail in earlier studies than in this study.

Specific comment

Section 2.3: If this is the first publication after the methodological paper (Membrive et al., 2017), presentation of the monthly AirCore data in more detail might interest readers. Given

that the data plays a central role in this manuscript, it would be interesting and helpful to see the variation characteristics (e.g., seasonality) and comparison to the CAMS and climatology.

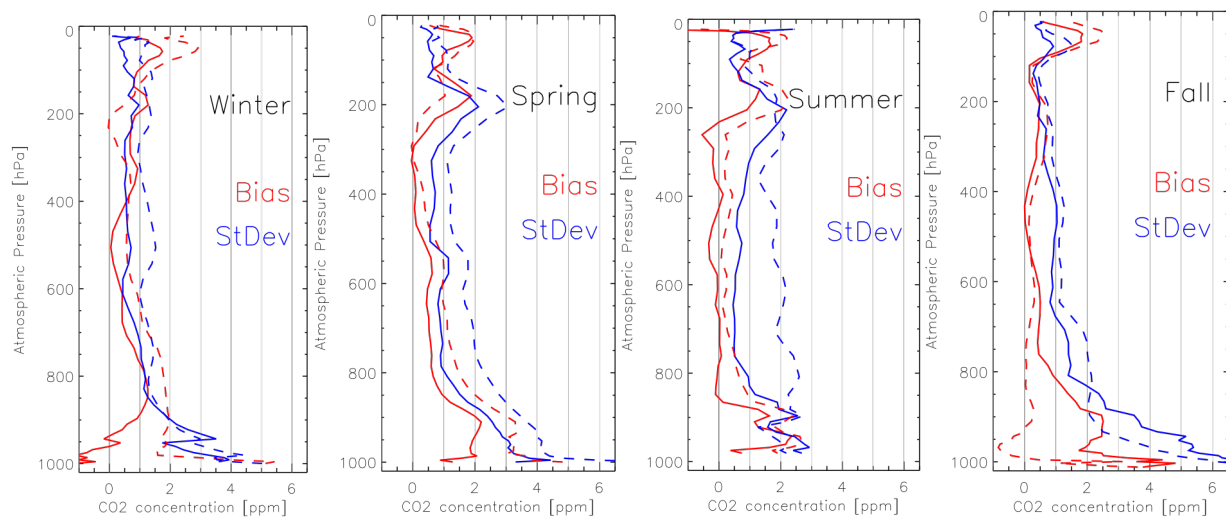
We do not understand this comment. Our analysis demonstrates the CAMS and climatology products reproduces closely the Aircore observations, at least over France. Thus, there is little doubt that both products do reproduce the seasonality described by the Aircore profile. Conversely, the Climatology does not reproduce the synoptic variations that are described by CAMS.

P7 L156: “STDDEV” is not consistent with that in Figure 3 (“StDev”).

Corrected

Figure 3: I am curious to see how the bias and standard deviation vary with season.

Please find below Figure 3 separated by season. We do not feel there is a signal strong enough to deserve to add this figure in the manuscript, in particular since the number of samples in each season is (obviously) smaller than for the full year, which leads to significant statistical uncertainty



Section 3.5 and Figure 7: I am afraid that the term “error” could be misleading and I would use a more neutral term like “difference” or “deviation.” The CAMS output and the climatological fit behave differently by definition. A small value is obtained by coincidence. In this sense, Figure 7 shows areas where the climatological fit has difficulty for follow-up use (where “errors” are high), as the authors also discuss later.

We have changed the term “error” to “deviation” when appropriate, as suggested by the reviewer. We agree that large RMS value of the difference indicates that there are variations (synoptical or inter-annual) that cannot be reproduced by the climatology. We feel it is interesting to show a quantitative evaluation of these non-climatological deviations

P12 L231: As in my earlier comment, it would be interesting to see season-specific comparisons.

See answer above

P16 L323: The use of Mauna Loa as a reference site has a history. A bit more explanation about the site's advantage in latitude, altitude and geography could be mentioned somewhere in the manuscript. This comment applies to American Samoa and Amsterdam Island, but I wonder what kind of improvement is expected by replacing Mauna Loa with American Samoa or Amsterdam Island.

We have added some additional discussion on the potential use of American samoa for a better description of the CO2 concentration variations in the southern hemisphere

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