Please find below the 3 reviews together with *our answers*. The main changes are

- Description and comparison to the SLIMCO2 product, which has a similar objective as our own
- Addition of an evaluation against NOAA Aircore profiles (Figure 4)
- Add an evaluation of the profiles against TCCON using the vertical weighting functions
- Add references to each of the TCCON stations
- Some added discussions as suggested by the reviewers
- Grammar corrections

We upload both the Track-Change and the clean version

Reviewer 1

Summary of the manuscript

This manuscript describes a compact climatology of CO_2 dry air mole fraction in the atmosphere, based on a fit to the CAMS CO_2 inversion product (version 22r1) from 2010-2022. The goal is to create a simple climatology that can be used as a prior for remote sensing retrievals of total column CO_2 (XCO_2), or as a simple truth metric against which to evaluate retrievals. Their focus seems to be primarily on the MicroCarb retrievals. The authors parameterize the mean seasonal cycle of CO_2 for 2.5x1.27 lon/lat grid boxes on the 79 CAMS layers as a double-harmonic with annual and semi-annual periods, plus a secular increase taken from the Mauna Loa (MLO) surface
CO2 record. The authors also derived vertical error-covariance matrices. The stated deliverables are a 25 MB netCDF file of fit coefficients and a 400 MB covariance file.

The authors examine the goodness of the fit vs. the original CAMS concentrations, as well as compare against TCCON XCO_2 and 80 AirCore profiles taken over central France. Overall, the performance is quite good and generally is a modest improvement over the commonly-used TCCON prior (Laughner et al., 2023). The authors describe their model's performance against some previous simple CO2 models, including the Simple Empirical CO2 Model (SECM) of Reuter et al. (2012) and the afore-mentioned TCCON prior, and find their approach generally has improved performance.

Overall assessment

Given the authors' goal of creating a simple CO2 climatology to be used primarily as a prior in retrievals, they have largely accomplished this goal. It is a technical paper with a limited audience, but it will be useful to some researchers and is therefore worthy of publication, once the concerns detailed below are addressed.

We mostly agree with the reviewer summary of the paper. We nevertheless feel that the paper also offers a quantitative evaluation of the components of the CO2 variability (trend, seasonal cycle, and interannual/synoptic variability) and an evaluation of the accuracy of the global CO_2 modelling, which can be of interest for a broader audience

General Comments

A central concern I have is that the authors of this manuscript seemed to have missed the more recent Simple cLImatological Model for CO_2 ($SLIMCO_2$), introduced in Noël et al. (2022) and described in detail in their Appendix A. SLIM is a simple climatological CO_2/CH_4 model whose primary stated purpose is to generate a priori profiles for satellite retrievals from FOCAL, and it is also used as a reference for bias correction. That is essentially the headline application claimed in the present manuscript. The approach of SLIM vs. the present work is remarkably similar:

- Both fit CO2 from a model. SLIM uses CarbonTracker (3x2 grid resolution) from 2003-2018, and the present work uses CAMS (2.5x1.7 grid resolution) from 2010-2022.

- The present work maintains the native 79 vertical layers of CAMS; SLIM fits each of 20 vertical layers, defined such that a straight average yields XCO_2 directly (i.e.; each layer has the same number of dry air molecules per unit area).
- Both assume a static, per-grid box, per-layer seasonal cycle. However, Bréon et al. parameterizes the seasonal cycle with just 4 numbers (a double sinusoid), while SLIM uses 36 numbers (piecewise linear with a ~10-day resolution).
- Both examine vertical covariances.
- SLIM is only evaluated against TCCON and the driving model (CarbonTracker); the present work is evaluated against TCCON, the driving model (CAMS), and additionally 80 AirCore profiles over central France. Futher, it appears to this reviewer that SLIM (Noël et al, 2022, Fig A5) generally compares better against TCCON than the present work (Fig 4), with the exception of Darwin and Reunion.

Therefore, they are nearly identical in approach and outcome, with only a few minor exceptions. To rectify this, the authors must clearly state how their work differs from the SLIM model, and why it is a valuable addition to the literature.

We fully agree that the SLIM method shows many similarities with what we have developed. We must confess that we were not aware of the SLIM development and we thank the reviewer for pointing it out. After contacting the authors of SLIM, we have made quantitative comparisons of the two products estimates of the climatological CO2. In the revised version, we fully acknowledge SLIM and discuss the increment brought by our modeling.

Further, in the introduction, the broader body of empirical / statistical XCO_2 reconstruction and gap-filling work (EOF-based reconstructions, machine-learning XCO_2 mapping, and CAMS-EGG4-based seamless products) should also be acknowledged, at least briefly, so that the compact-parameterization niche is clearly delineated.

Finally, the writing and grammar leaves something to be desired. There are run-on sentences, nouns with incorrect plularization, and just plain grammar errors. I strongly suggest the authors to run the manuscript through a spelling/grammar checker, or better yet through an AI LLM to check for spelling, grammar, and general language consistency and readability of the manuscript.

We thank the reviewer for this suggestion. In the revised version, we have used ChatGPT to detect such grammar errors and, hopefully, made adequate corrections

Specific Comments

- Abstract, Line 6: The opening sentence is incorrect as written. Atmospheric CO2 concentrations obviously can show strong local features, such as from fossil fuel sources. Please modify accordingly. You could just add “regional-scale” to the opening sentence, for example.

We agree and have corrected the opening sentence accordingly

- Abstract, Line 18: It need not be limited to XCO2; it could for instance be used for TIR (midto-upper troposphere) retrievals as from IASI, or profile retrievals.

We agree and have added the reference to CO2 profiles

- Section 1, Line 96: Please remove ", which is expected." It is not necessarily expected. For instance, Graven et al showed that the amplitude of the CO2 seasonal cycle in the northern latitudes increased by 50% above 45N latitude since 1960 (Graven et al, 2013, Science). Later studies tried to pin down the causes of this and found a number of factors involved, including temperature increases leading to GPP gains to exceed respiration increases (Forkel et al, 2016). Lombardozzi et al (2025) used modeling studies to show that agricultural nitrogen fertilization was the single largest factor explaining the seasonal cycle increase at ~45%. Finally, the seasonal cycle changes were not limited to the northern hemisphere. There appears to be an increasing seasonality of air-sea gas exchange in the southern ocean that has mediated a seasonal cycle increase in southern latitudes (Yun et al, 2022).

This limitation (increase of the amplitude of the seasonal cycle) was well noted in the first version of the manuscript in the discussion section, with a citation to Graven et al. We have nevertheless made a correction in section 2.2

- Section 2.2 : Please state over which years the fit is performed, and if it is fit for all 79 of CAMS vertical levels, or a more compressed vertical coordinate. This is not clear in the text, and is essential for reproducibility.

We have added this precision. The fits are performed over the 2010-2022 period and are done independently for each of the grid cells and the 79 vertical layers.

- Section 3.2: Why is the only AirCore data used that from France? This simply feels lazy on the part of the authors, especially considering that France is generally well-constrained by in-situ measurements, so CAMS can be expected to perform especially well there. Because all (or nearly all) of the AirCore data are available in the NOAA ObsPack dataset, this seems like an unnecessary shortcoming. I realize the authors list this in section 4.5 describing the paper's limitations, but it feels like a really important missing point from this manuscript, especially because there is so little already to differentiate this work from the SLIM model of Noël et al. (2022). I think it is important to expand the analysis to more locations around the globe with measured CO_2 profiles. I know for a fact that ObsPack even contains profiles over Uganda! ("co2_ugd_aircraft-insitu_1_allvalid").

We focused on the AirCore acquired in France, from a location close to the Trainou TCCON site because there are many such acquisition in the same location which makes it possible to compute valid statistics and variance-covariances. Following the reviewer suggestion, we have downloaded the NOAA AirCore profiles. We have added a figure to show the differences with CAMS and the climatology over these sites and added some discussion

- Section 3.3 (line 174): please provide some defense of ignoring the AK correction

The manuscript developed a climatological fit of the total column as well as vertical profiles. In the submitted version of the manuscript, we only evaluated the total column estimate which was compared

to the total column estimates from TCCON. In the revised version, we evaluate both the climatological total column and the vertically integrated climatological profiles. Le latter uses the AK. The statistics appear very similar.

- Section 3.3 (line 183): When you say "for this inversion", I think you mean "for the TCCON retrievals". This statement is unnecessary and probably wrong. TCCON operationally performs a scaling retrieval, and as the authors imply in the next sentence, the scaling factor is well constrained by the TCCON measurement. The only issue is the fidelity of the prior profile at levels in the atmosphere less well-constrained by the measurement; i.e, in the upper atmosphere where the averaging kernel is significantly less than unity. From Laugher et al. (2023), who did a very complete comparison to AirCore, they have very small differences in the upper atmosphere to AirCore, even less than this climatological fit to the CAMS inversion, so it seems possible to this reviewer that the use of this climatological prior would worsen TCCON XCO2 retrievals, rather than improving them.

We have added a few sentences consistent with the reviewer comment at the end of section 3.3

- Section 3.4 feels out of place; it is not discussing the fidelity of the prior, but rather is mainly just showing how the seasonal cycle amplitude varies in location (lat/lon) and altitude, as taken from the CAMS inversion. There is nothing here about validating the model. It's fine but some explanation of why this is being discussed at the beginning of 3.4 would be useful.

Section 3 is "results" and section 3.4 is "Amplitude of the seasonal cycle". We do not understand the reviewer comment as the amplitude of the seasonal cycle is indeed a result, based on the analysis of the climatological fit of the CAMS modeling. No changes were made

- Section 3.5, first sentence: Please specify that Figure 2 is for a specific location in central France. It is likely not representative of global statistics.

We have added : "for a specific location chosen at the centre of the AirCore landings of Figure 1"

Technical Comments

- Year mismatches between text and reference list: "Friedlingstein et al., 2025" in the text vs. "2026" in the list; "Piao et al., 2017" in the text vs. "2018" in the list.

  Abstract (line 14): "the difference between simulated concentrations and in-situ … measurements are" → "is."
- Abstract (line 17): "may be therefore be used" — duplicated "be."
- Introduction (line 63): "Section 4 discusses the results and concludes" — missing period.
- Section 2.3 (line 106): "the sampling apparatus return to the ground" → "returns."
- Section 3.1 (line 133): "much more pronounced in the low atmospheric layers than in the atmosphere" → "than higher in the atmosphere."
- Section 3.1 (line 134): "different shapes depending of the atmospheric layers" → "depending on."
- Section 3.2 (Line 151): I think you mean "hybrid sigma" as opposed to just plain "sigma". Hybridsigma means P(i) = A(i) + B(i) Ps. Sigma means P(i) = B(i) Ps. Please check and fix accordingly.

- Section 3.2 (line 167): “provide a constrain” → “constraint.”
- Line 238: grid cell à grid cells.
- Line 277: levels à level
- Units (line 278): “25 Mo” (French Mégaoctets) vs. “25 Mbytes” (line 372) later — standardize to “MB.”
- Section 4.1 (line 258): “Mana Loa” → “Mauna Loa.”
- Competing interests (line 387): “no competing intest” → “interest.”
- Fig 8 caption: “line” à “rows”
- Consistency (lines 216, 18): “Hawaï” (line 216) vs. “Hawaii,” and “a-priori” (line 18) vs. “a priori” — standardize throughout.
- References: Taylor et al. (2022, ACOS GOSAT) and Lindqvist et al. (2015) appear in the reference list but are never cited in the body. Lindqvist et al. (“Does GOSAT capture the true seasonal cycle of CO_2?”) is directly relevant to Sections 3.4 / 4.1 and should be used there. Laughner et al. (2024, ESSD GGG2020 data) is listed, but the TCCON-data sentence cites only Laughner 2023—the 2024 data paper should be cited where the GGG2020 product is introduced.

We thank the reviewer for these technical comments. They have all been accounted for in the revised version.

In addition, we asked ChatGPT to identify grammatical errors and corrected them accordingly

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Key references consulted for this review

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Yun, J., et al. (2022), Sci. Adv., 8, eabq0220, https://doi.org/10.1126/sciadv.abq0220.

Reviewer 2

This manuscript presents a concise and operationally useful climatological description of atmospheric CO_2 variability. The core idea is to decompose the global CO_2 field into (i) the long-term growth rate derived from the Mauna Loa record and (ii) a spatially varying seasonal cycle represented by two harmonics. Using the CAMS high-resolution reanalysis (1979–2022), the authors derive only five coefficients per grid point and pressure level, and validate the resulting climatological product against 80 independent AirCore profiles over France and global TCCON column retrievals. The results show that the fit captures the dominant variability with typical errors of ~1–1.5 ppm in the free troposphere, with vertically correlated error structures, and it significantly outperforms the existing GGG2020 prior model (Laughner et al., 2023). Overall. The manuscript is well structured, the validation is thorough, and the discussion openly acknowledges limitations and possible improvements. The quality is high, and the paper is suitable for publication in ACP after addressing the following comments:

We thank the reviewer for this assessment

Major comments:

1. The core validation relies exclusively on 80 AirCore profiles from a single region (central France), where the CAMS reanalysis is tightly constrained by a dense surface network and may not be representative of the global performance. The authors themselves acknowledge this in the discussion. However, the authors can add at least one additional independent dataset from a contrasting region (e.g., NOAA AirCore flights over North America, or the regular Sodankylä profiles in Finland) to show whether the error statistics are broadly similar or not.

In the revised version, we have added a figure that shows the comparison of the climatological product against all NOAA Aircore profiles acquired in North America, Finland and Australia. We discuss the comparisons which appear consistent with the results obtained over France

2. The authors note that the inter-hemispheric exchange time (~1 year) suggests using American Samoa or Amsterdam Island data for the southern extratropics, yet the main product still relies solely on Mauna Loa. Here, I would suggest the authors make a sensitivity test: where the Mauna Loa trend is replaced by the American Samoa record for grid points south of, say, ~20S, and comparing the resulting XCO2 errors against TCCON stations such as Wollongong, Lauder, or Reunion.

In the revised manuscript, we analyze the difference between the smoothed Mauna Loa and American Samoa time series and discuss in quantitative term the impact on the climatology when using one versus the other. The differences are on the order of a few tenths of ppm with a trend component (increasing difference due to increasing emissions in the northern hemisphere. The impact on the climatology is visible but small with respect to the other uncertainties.

3. In Section 4.5, the authors mention “For the comparison against TCCON, we did not account for averaging kernels. While acceptable for a first-order assessment, this simplification may slightly bias the interpretation of TCCON comparisons. A more precise evaluation of the precision of CAMS and its climatological fit should account for the variations linked to the

averaging kernels." But I am still curious about the effect of the AK. Can you make a sensitivity test and add a figure as well?

In the revised version, we have modified Figure 4 to show both the statistics obtained when using the unweighted total column (as in previous version) and the statistics when using the AK for the weighting. The differences are very small.

Minor comments:

1. P12, 3.5 Vertical Covariance -> 3.6 Vertical Covariance

Corrected

2. TCCON citation (data DOI) is missing. Although the acknowledgement includes the TCCON, please check with the TCCON community/PIs if this is enough.

We have been in contact with the TCCON community to make a proper acknowledgment that is acceptable to them. We have added a reference for each of the TCCON sites individually

Reviewer 3

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.

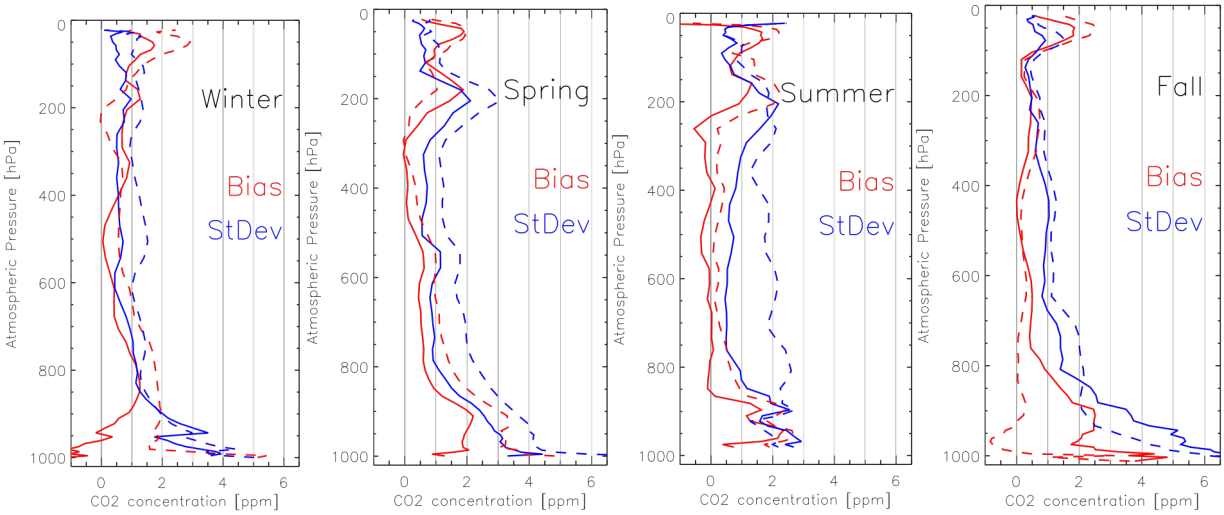
We do not understand this comment. Our analysis demonstrates the CAMS and climatology products reproduces closely the Aircore observations, at least over France. Thus, there is little doubt that both products do reproduce the seasonality described by the Aircore profile. Conversely, the Climatology does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

Reference

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