Referee Report

Manuscript review for AMT / EGUsphere

Manuscript: “A climatological description of atmospheric CO_2 concentrations”

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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 CO_2 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.

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.

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.

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.

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

- 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).

- 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.

- 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").
- Section 3.3 (line 174): please provide some defense of ignoring the AK correction
- 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.
- 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 3.5, first sentence: Please specify that Figure 2 is for a specific location in central France. It is likely not representative of global statistics.

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". Hybrid-sigma 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 → grid cells.
- Line 277: levels → 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" → "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.

Key references consulted for this review

Graven, H. D., R. F. Keeling, S. C. Piper, P. K. Patra, B. B. Stephens, S. C. Wofsy, L. R. Welp, C. Sweeney, P. P. Tans, J. J. Kelley, B. C. Daube, E. A. Kort, G. W. Santoni, and J. D. Bent (2013), Science, 341, 1085–1089, https://doi.org/10.1126/science.1239207.

Forkel, M., N. Carvalhais, C. Rödenbeck, R. Keeling, M. Heimann, K. Thonicke, S. Zaehle, and M. Reichstein (2016), Science, 351, 696–699, https://doi.org/10.1126/science.aac4971.

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Noël, S., M. Reuter, M. Buchwitz, et al. (2022), Atmos. Meas. Tech., 15, 3401–3437, https://doi.org/10.5194/amt-15-3401-2022.

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