

Interactive comment on “Carbon monoxide climatology derived from the trajectory mapping of global MOZAIC-IAGOS data” by M. Osman et al.

Anonymous Referee #1

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Reviewer’s comments on: "Carbon monoxide climatology derived from the trajectory mapping of global MOZAIC-IAGOS data" by M. Osman, et al.

General Comments

This manuscript by Osman et al. reports a novel use of MOZAIC/IAGOS (or 'M/I') measurements to construct a global, 3D, time-varying climatology for CO. The method relies on the HYSPLIT trajectory model and NCAR/NCEP reanalysis wind fields to 'project' measured CO concentrations to regions and altitudes lacking actual measurements. The resulting climatology is evaluated first by comparing CO climatology maps generated with forward and backward trajectories separately. The method is then validated by comparing M/I CO statistics at a number of airports with trajectory-mapped climatologies calculated after withholding M/I measurements at each of those airports.

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Finally, the CO climatology is compared with MOPITT satellite results, where MOPITT Level 3 CO values have been regridded to a 5-by-5 degree grid, matching the resolution of the trajectory-mapped climatology.

While the general method described in the manuscript is novel, the significance of the new climatology product is very unclear. The manuscript includes very little in the way of an error analysis that would permit an understanding of the limitations of the climatology. This analysis should be presented separately from the validation of the method. At a minimum, such an analysis should include (1) a clear description of the underlying assumptions of the method, and the impact of these assumptions on the accuracy of the climatology, and (2) a statistical analysis of the 'robustness' of the climatology (based on the variability of the CO values that are averaged together in each 'bin'). An important assumption that is only vaguely mentioned in the manuscript is that measurements of CO near source regions (e.g., urban regions surrounding airports) are directly useful for estimating CO concentrations large distances both upwind and downwind of the airport. This 'airport effect' would seem to result in a significant positive bias in the trajectory-mapped CO values. A statistical analysis of the variability of the trajectory-mapped CO values is necessary to distinguish regions where the CO climatology is 'statistically robust' from those regions where the uncertainty is very large.

The manuscript does not include a true validation section, which would involve comparing the trajectory-mapped CO climatology with an existing product with known error characteristics. The presented methods for evaluating the climatology are mostly qualitative. These methods have some value but are rather indirect and inconclusive. For example, since all airports are located near major urban centers, the biasing effects of urban sources of CO may well be similar at different airports. So, the trajectory-mapped climatology may generally represent CO profiles near urban regions better than in rural areas far from sources. If so, the experiments in which trajectory-mapped climatologies are calculated after withholding observations at one airport are still not indicative of the

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accuracy of the climatology away from urban regions. A more conclusive validation would be based on independent in-situ data from aircraft deployed during some field campaign, where the issue of local sources was known to be unimportant.

The comparisons of the trajectory-mapped climatology with MOPITT Level 3 products gridded at 5-degree resolution have only qualitative value due to several effects. First, the trajectory-mapped CO values are based on in-situ measurements near urban regions (esp. in the lower troposphere), whereas MOPITT Level 3 products at 5-degree resolution represent a mix of urban and mostly rural atmospheric conditions. This effect would very likely result in a positive bias in the trajectory-mapped climatology. Second, because of the significant variability of MOPITT retrieval performance with respect to various geophysical parameters (surface type, CO loading, thermal contrast), the validity of MOPITT averaging kernels averaged over large regions is unclear. All previous published MOPITT validation papers have exploited MOPITT Level 2 data averaged over much smaller regions.

Major Revisions

1. Most readers will find this paper unreasonably long and it should therefore be shortened. I believe the paper should be split into a 'methodology and validation' paper (based on Sections 1-4) and a separate 'analysis' paper (based on Sections 5 and 6). Given that some parts of the methodology need to be lengthened (see below), reducing the paper's length is even more important.

2. The paper should include a section specifically addressing errors in the trajectory-mapped CO climatology product. In addition to other sources of potential error (e.g., chemistry and trajectory errors), this quantitative error analysis should estimate the magnitude of systematic errors due to the fact that the M/I CO measurements in the lower-troposphere (e.g., 800 hPa to 1000 hPa) are likely biased towards CO concentrations observed over urban regions near airports. The error analysis should also provide information on the statistical robustness of the climatology product in relation to

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the variability of the trajectory-mapped CO values which are averaged together in each climatology bin. This statistical analysis should then be used to assess the expected geographical variability of the uncertainties of the trajectory-mapped CO climatology.

3. The purpose of the comparisons with MOPITT Level 3 products is unclear. Is the intent to use MOPITT products to validate the MOZAIC-based CO climatology, or is it the other way around? Statements on pages 18, 19, and 20 suggest that the trajectory-mapped CO climatology reveals biases in the MOPITT V6 product, despite the fact that this product has already been thoroughly validated against in-situ data obtained from NOAA aircraft and the HIPPO field campaign. True validation involves comparisons of a new product with an established product with known error characteristics. In this case, it seems that the error characteristics of the MOPITT product are better understood and better quantified than the errors associated with the trajectory-mapped climatology. Thus, it seems more reasonable to conclude that the MOPITT V6 product reveals biases in the trajectory-mapped CO climatology (than the other way around).

4. The comparisons with MOPITT total column values (Section 4.2) appear to be based on incorrect assumptions regarding the MOPITT layering scheme, and should be repeated and reanalyzed. Specifically, the assumed layer boundaries (at the midpoints between the MOPITT retrieval levels) do not agree with the layering scheme discussed in Deeter et al., (2013) and in the MOPITT V5 User's Guide.

Minor Revisions and Technical Corrections

p. 2, l. 16 - 'comparison' should be 'comparing'

p. 2, l. 22 - Are the results really conclusive that MOPITT is biased, or are the authors really just stating that there is some bias between MOPITT and the trajectory-mapped climatology?

p. 2, Abstract - Should be some brief statement about the limitations of the climatology, e.g. primarily for Northern Hemisphere.

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- p. 5, l. 9 - The meaning of the sentence "Background CO levels are found ..." is unclear.
- p. 5, l. 13 - There is insufficient evidence for this claim.
- p. 6, l. 25 - What are these 'obvious advantages'?
- p. 6, l. 28 - This statement is premature, since no data have yet been presented in the manuscript.
- p. 7, l. 13 - typo in 'transformation'
- p. 7, l. 20 - There is no clear reason for dividing the MOPITT-related material into Sections 2.4 and 4; they should be combined in a single section.
- p. 8, l. 21 - what fraction of the airports (or MOZAIIC profiles) are located in the Southern Hemisphere?
- p. 9, l. 1 - This is incorrect; MOPITT retrievals do not rely on thermal contrast between the surface and atmosphere (but do rely on a temperature gradient within the atmosphere).
- p. 9, l. 6 - Which MOPITT product is exploited in this paper, the TIR-only, NIR-only, or TIR/NIR?
- p. 9, l. 19-26 - MOPITT validation results vary widely from one version to another; only V6 validation results should be listed here since results for other versions are irrelevant.
- p. 10, l. 4 - Why were the M/I cruise data not used?
- p. 11, l. 3 - A thorough discussion of the potential effects of source regions on the trajectory-mapped climatology is needed here; It is not conclusive that qualitative comparisons of maps based separately on backward or forward trajectories prove that source regions have an insignificant effect.
- p. 12, l. 19 - Add reference to Worden et al (Atmos. Meas. Tech., 6(7), 1633–1646, doi:10.5194/amt-6-1633-2013) regarding the variables which affect MOPITT averaging

kernels.

p. 13, l. 22 - Here the manuscript lacks important details. Exactly how were the CO concentrations at the 'missing' MOPITT levels (above the maximum MOZAIC aircraft altitude) determined? How many levels in the vertical grid are actually affected by this? Note that the described strategy of using MOPITT a priori profiles is inconsistent with methods used in MOPITT validation papers and might lead to unphysical discontinuities in the CO profile. Does the chosen method of filling in these high levels affect the results?

p. 14, l. 17 - Why exclude airports in the Southern Hemisphere from the validation study?

p. 15, l. 25 - All of the listed airports are located in the Northern Hemisphere. Should include several from the Southern Hemisphere, where the climatology might be more challenged.

p. 16, l. 15 - For readers' convenience, online M/I CO maps should be reproduced in the manuscript (with permission) to compare against Fig. 6.

p. 17, l. 22 - Both the shapes and magnitudes (or areas) of the averaging kernels are significant.

p. 17, l. 7 - It is unclear if the analysis of Fig. 2 is included just as an example, or if it supposedly illustrates overall bias in MOPITT. Any conclusions about MOPITT retrieval bias should compare findings to MOPITT validation papers; possible reasons for any discrepancies in validation results (relative to published results) should be discussed.

p. 18, l. 10 - Which MOPITT product was used: TIR-only, NIR-only or TIR/NIR?

p. 18, l. 10 - Emphasize that standard MOPITT L3 retrievals have been regrided to 5-degree resolution for this analysis.

p. 18, l. 27 - Are the authors suggesting that the trajectory-mapped CO climatol-

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ogy can be used to validate MOPITT, or are the MOPITT data being used to validate the MOZAIC CO climatology? This sentence observes that there is a difference between the two products ("reveals significant biases between MOPITT and the trajectory-mapped ..."), while the first sentence in the next paragraph ("MOPITT seems to underestimate ...") suggests that the trajectory-mapped CO climatology can be used to determine biases in MOPITT products.

p. 19, l. 5 - This is incorrect; all previous MOPITT validation work was performed with MOPITT Level 2 products, not Level 3 products. This may be related to the significant discrepancies in the validation results reported in this manuscript compared to previously published MOPITT validation results.

p. 19, l.9 - It is not true that all published MOPITT validation results have been based on NOAA flask sampling.

p. 19, l. 10-19 - Comparisons of MOPITT L3 data (w/ 5-degree resolution) and M/I profiles for one airport (Frankfurt) do not provide convincing evidence of a general negative bias in the MOPITT retrievals, especially given that MOPITT V6 validation results have been previously reported for a large number of NOAA sites and for the HIPPO field campaign (Deeter et al., 2014).

p. 19, l. 12 - It is unclear if these comparisons are based on standard MOPITT L3 data, or the regridded 5-degree resolution L3 data.

p. 20, l. 12 - It is unclear why MOPITT retrievals in the lower troposphere would only yield a lower-bound (although it is true that such retrievals are often highly constrained by the a priori).

p. 21 - References are needed for the text and equations presented in Section 4.2.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 29871, 2015.