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Comment on acp-2022-458

Anonymous Referee #1

Referee comment on "Assimilation of S5P/TROPOMI carbon monoxide data with the global CAMS near-real-time system" by Antje Inness et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-458-RC1, 2022

This manuscript provides a thorough accounting of the procedure, and changes in performance, of the CAMS NRT system when including TROPOMI observations of carbon monoxide (CO). The authors show that by including the TROPOMI CO observations the CAMS CO columns increase by on average 8%, which result in a much improved match compared to independent observations, as compared to a version of the CAMS system where only MOPITT and IASI observations are assimilated.

The authors furthermore assess more closely the impact of TROPOMI assimilation for fire plumes. While the total column can well be constrained, the TROPOMI data does not provide further constraints on individual plumes that are transported across continents, at altitudes above 500 hPa. This suggests (even though not written explicitly) that modeling aspects, such as accurate emission estimates and transport, remain essential to be able to capture such events.

The manuscript is well written, and provides a good description of the current state, and technical details, on the TROPOMI CO assimilation configuration in CAMS.

However I find it rather an accounting of the technical aspects, which would indeed be valuable as a technical report, but less so as a scientific contribution as could be expected in ACP. For that, a more critical assessment of various aspects would be expected. The authors should be more rigorous and selective in the figures that they wish to present, to make their points.

For example, the manuscript contains a long section on the monitoring of TROPOMI data in the CAMS system. While this is valuable, and well-understood from an operational perspective, the value for the reader is not clear, and does not justify a lengthy reporting. I suggest to condense, or even remove this section, or alternatively move this into supplementary material, also because reported performance statistics over the given time period are difficult to interpret/reproduce, and not generally interesting, given the changes in the CAMS system and observation version.

Also various figures do not contain any novel information, and could possibly be moved into supplementary material (or removed completely), e.g. Fig. 5, while Fig. 6 and Fig 10 contain also very similar information; as well as Figure 12, 17, 19, 20, and redundancy in Figures 22-25).

Also, discrepancies, e.g. between TROPOMI and MOPITT and IASI, as well as between TROPOMI observations and GFAS fire estimates are indicated, but the implications of these discrepancies are so far essentially disregarded. Such implications are however very important for generating long, consistent time series of TCCO in future reanalyses, and for the accuracy assessment of GFAS fire CO emission estimates, e.g. in terms of its timing, and magnitude. I think exactly such important messages cab be drawn from this study, but appear lacking.

So, In an updated version of this manuscript I challenge the authors to provide a more critical of such aspects, and reduced reporting particularly on the monitoring aspects.

These elements should ideally also be reflected in an updated version of the abstract, which could well be shortened and made more to-the-point, and the conclusions section.

More detailed comments

pp5, line 19: You describe why the IASI data is bias-corrected, and not MOPITT. (for historical reasons). Given that IASI appears more consistent with TROPOMI, wouldn't this study be an argument to change this configuration, and apply bias correction to MOPITT instead?

pp5 line 30 and 34: 150 days were used for generating the BGE, both in the old and new configuration. Given the different lifetime, and specifics of CO for different seasons over the globe, it seems to make a difference for which period the 150 days were chosen (summer or winter).

pp6, line 2: “constant in time, globally averaged (…) profile” This seems a gross simplification of the BGE information, given that the tropopause height varies strongly over the globe, and that CO emissions and lifetime features different uncertainties depending on season and region. Could you comment on this? To what extend is such a simplification applied here expected to contribute to overall errors in resulting CO analyses?

pp10-11: The authors provide a thorough, and useful accounting of the quality of the various TROPOMI CO data streams. My take on this is that the TROPOMI CO data, as used in the CAMS system, is actually biased high by a value of 6.5% (NDACC) to 9% (TCCON) (pp 11, line 9), and even larger positive biases over Antarctica. Is this correct? If so, how does this relate to percentual differences of monitoring experiments as reported later on in the manuscript (pp 13, line 14-15: departures of 9-11%). This also to provide further justification of the statement given on pp 17, line 1-2 (“this suggests that (…) CAMS is biased low and not that TROPOMI CO is biased high”) and pp 18, line 1-2

(“In ASSIM TROPOMI TCCO is used without bias correction”). It seems that the definite justification for all this is only provided in sec. 4.2.3, when independent evaluation is introduced.

Sec. 3: Please consider to condense this section, or move this to an appendix or so. The only relevant figure is maybe Fig. 7, except that it might be better to present the data for a more consistent model configuration and time period (e.g. only for the full year of 2021). Then also the changes to the CAMS model system (incl. Table 2) could be moved there.

pp20, line 2-3: “MOPITT departures are increased in ASSIM”. I think this is a very important finding that deserves more attention than currently given. Despite the arguments given earlier about the different sensitivities of MOPITT and TROPOMI, I find it worrying that the math with MOPITT decreases, given that I expect that MOPITT TIR retrievals have been thoroughly validated, and improved, over many years (what is their reported uncertainty statistics?). Does this study, and particularly Figure 11, imply that MOPITT TIR retrievals of TCCO are biased low? What are implications for CAMS when developing a new reanalysis product? Your judgement as part of the discussion and conclusions section would be much appreciated.

pp 24, line 7: “leads to changes in surface ozone”: Please remove this statement if not further substantiated. Even though O3 is undoubtedly modified, It is in my view much more likely that the changes seen are a direct effect of CO assimilation.

Furthermore, when presenting evaluation of CO against surface observation networks, please provide information on the quality and representativity of these observations - this is to my knowledge not straight-forward, which makes interpretation of results in Figure 18 difficult, if not impossible.

Sec. 4.2.4 (Boreal Wildfires), particularly Figures 19-20: I believe these figures, and their discussion, even though interesting as an assessment on its own, are out of scope wrt the topic of this manuscript. When presenting these results from the GFAS system in a manuscript, I rather expect a scientific judgement, e.g. with respect to their quality, and/or impact in the CAMS system, particularly as TROPOMI CO data really enables such quantification. This connection is currently essentially missing, which therefore does not justify the figures shown here. To the least the authors should be more selective.

pp 27, line 5: "These high values are better captured in ASSIM than CTRL". What are the implications of this finding for the GFAS emission amounts reported above?

pp 29: "horizontal mismatch". Considering that the modeled plume is present one day too late, could it possibly also be a mismatch in time of the GFAS emissions in this configuration?

pp 29, l16 "can be considered a success": I quite do not agree with this statement. Of course TROPOMI CO is very successful in correcting the total column, much better than was the case in the CTRL configuration, but from the given evidence it appears simply not able to significantly improve the placement of the plumes in the right altitude, which was the topic of exactly this analysis.

pp32, line 1 (46 Tg, 17 Tg, 9 Tg) - please consider to remove the statement on quantification of CO emissions here. The current evaluations actually suggest that these are under-estimates (but unclear by how much)

Conclusions section: Please provide an overview of open issues, and current limitations.

Technical corrections

pp1 line 28: “already … already” please rewrite removing one ‘already’.

pp3 line 16: “published” change to “reported”, remove “but not in a peer reviewed publication”.

line 18 “additional work”, please specify “work” here, ore rewrite this sentence. “Additional work” does not justify a manuscript per se.

line 39: “CAMS-GLOB-ANT”

pp4, line 1 “CAMS-GLOB-BIO”

pp6, line 6/7: please provide a more generic reference, this is not really readable. Alternatively consider to move this technical information, which is not essential to the subject of TROPOMI CO assimilation, towards an appendix or so (see also generic comments above)

pp 7, line 8: IAGOS: Provide here the explanation of this acronym, and not in line 12.

line 15 “in this paper” -> “here”

line 28: “station”->”stations”

pp 9, Table 3: Check ordering of rows: one date stamp (2019-08-06) should come after 2019-07-03?

pp 13, line 15: “medium”->”median”