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Geoscientific Model Development Discussions

Interactive comment on "Current status of the ability of the GEMS/MACC models to reproduce the tropospheric CO vertical distribution as measured by MOZAIC" *by* N. Elguindi et al.

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The authors are grateful to the valuable comments by the reviewer. We think that this manuscript is suitable for GMD because it presents work that was carried out for a project whose main goal is to develop an operational data assimilation system for chemically reactive gases. The validation results presented in this study were used in developing such a system. We have made the following changes in the manuscript to better reflect this,

We changed the title of the manuscript to "A global evaluation of the GEMS/MACC ECMWF-IFS model coupled to the CTM MOZART with 4DVAR data assimilation" and the abstract to,

"Vertical profiles of CO taken from the MOZAIC aircraft database are used to globally evaluate the performance of the GEMS/MACC models, including the ECMWF-Integrated Forecasting System (IFS) model coupled to the CTM MOZART with 4DVAR data assimilation for the year 2004. This study provides a unique opportunity to compare the performance of three offline CTMs (MOZART, MOCAGE and TM5) driven by the same meteorology as well as one coupled atmosphere/CTM model run with and with data assimilation, enabling us to assess the potential gain brought by the combination of online transport and the 4DVAR chemical satellite data assimilation.

First we present a global analysis of observed CO seasonal averages and interannual variability for the years 2002–2007. Results show that despite the intense boreal forest fires that occurred during the summer in Alaska and Canada, globally the year 2004 had comparably lower tropospheric CO concentrations. Next we present a global validation of CO estimates produced by the MACC models for 2004, including an assessment of their ability to transport pollutants originating from the Alaskan/Canadian wildfires. In general, all the models tend to underestimate CO. The models perform best in Europe and the US where biases range from 0 to –25\% in the free troposphere and from 0 to –50\% in the surface and boundary layers (BL). Using the 4DVAR technique to assimilate MOPITT V3 CO data is shown to reduce biases by up to 25\% in some regions. However, even the IFS/MOZART coupled model with assimilation is not able to reproduce well the CO plumes originating from the Alaskan/Canadian wildfires at downwind locations in the eastern US and Europe. Suggestions regarding the improvement of the assimilation technique, such as the use of averaging kernels, are given. Sensitivity tests also reveal that deficiencies in the fire emissions inventory and injection height also play a role."

We have also added the following paragraph discussing issues regarding data assimilation to the end of section 4,

GMDD

3, C178–C184, 2010

“The fact that in many of the cases during the summer biases from the ASSIM simulation are better than from the CTRL simulation suggests that the assimilation is improving the simulation. However, it is still difficult to explain how valid this improvement is because there are many influencing factors, such as sensitivity of the MOPITT sensor, the method to assimilate CO MOPITT tropospheric columns into vertical profiles, and the 4DVAR procedure itself. Firstly, MOPITT CO data have been generally regarded as not very sensitive to the boundary layer. The MOPITT data products used for assimilation are based on thermal-infrared measurements near 4.7∼microns, and its sensitivity to the lower troposphere is debatable (Deeter, 2007). Hence, changes in the PBL can not be efficiently constrained by MOPITT data assimilation. However, recent studies have shown that MOPITT sensitivity varies considerably depending on the thermal contrast conditions (Deeter, 2007,Clerbaux, 2008). These studies show that to a certain degree, MOPITT is sensitive to the lower atmosphere in conditions where there is a strong temperature gradient between the surface and lower atmosphere. This may well be the case during summers in the northern hemisphere and it emphasizes the need to evaluate the impact at all seasons, and explore the difference of impact in using either day- or night-time data. Secondly, as MOPITT’s averaging kernels are not used in the assimilation process, the information on the vertical distribution of CO is lost and the low sensitivity in the lowest troposphere is not taken into account. This may allow the 4DVAR technique to change the concentration profile predominantly in the PBL where the CO variability is largest. Thirdly, in the 4DVAR context, a simplified model (but something still “physical”) is used to make sure that the profiles are modified in a way that their total column (i.e. vertical integral) better fits the MOPITT total columns. The freedom of the 4DVAR technique to change the concentration profiles depends on the assumed background errors statistics. The higher this error is the bigger are the allowed changes. The background error is often linked to the variability, and because variability is often high in the PBL the values in this region are more likely to be changed.”

In addition, we have made modifications throughout the manuscript (as suggested by

GMDD

3, C178–C184, 2010

the reviewers) that emphasize better the 4DVAR assimilation technique, such as a more thorough description in section 2.2.

Below is our response to your other concerns.

Regarding the possibility of splitting the paper in two: We agree that the manuscript is quite lengthy, however we would prefer not to split the paper in two mainly because we think a thorough description of the observations is necessary before we present the validation, particularly because as the reviewer pointed out there is very little information about the seasonal variations of tropospheric CO concentrations from in-situ measurements in the literature. We think it was necessary to present a description of the characteristic CO profiles over the cities in which the validation is performed and to put the year we validate, 2004, into context in terms of CO inter-annual variability. Therefore, we opted to shorten the manuscript and make it more concise and readable in the areas that the reviewer suggested.

Specific Comments:

Introduction: We have shortened the introduction (from 1360 words to 860) and made it more concise and easier to read as the reviewer suggested.

Section 2: We have removed most of paragraph 1 in Section 2.1 describing the MOZAIC data and have added the following paragraph to Section 2.2 describing the assimilation technique;

'MOPITT V3 total column data (Deeter et al, 2003) are assimilated using ECMWF's 4D-VAR data assimilation system. The data are thinned to a resolution of 0.5 deg x 0.5 deg and are only assimilated over land between 65N and 65S. Averaging kernel information from the MOPITT data is not used, because it was not available at the time the GEMS simulations were run. The model equivalent of the observation is calculated as vertical integral. The background errors statistics for the CO assimilation were determined with the NMC method (Parrish and Derber 1992). For this, 150 days of 2-day forecasts

GMDD

3, C178–C184, 2010

Interactive Comment

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were run with the coupled system initialized from fields produced by the free running MOZART CTM, and the differences between 24-h and 48-h forecasts valid at the same time were used as a proxy for the background errors.'

Section 3: Following the reviewer's suggestion, we have shortened this section considerably and think that it reads better. Specifically,

Figure 1 and corresponding paragraphs 2 and 3 have been removed. The last paragraph on page 405 that summarizes the CO profiles and discusses the impact of the fall 2002 Russian fires has been removed. Although we feel this material would be a valuable contribution to the literature, we agree with the reviewer that it does not fit well in this manuscript. Following the reviewers suggestion, Tables 2-5 have been moved to the supplementary online material. Regarding the suggestion to include plots of the same format as in Section 4, we prefer not to add any more plots to the manuscript which we are trying to shorten. We hope that the profiles shown in Figures 1-4 and the Tables now given in the supplementary online materials will be sufficient to give the readers an idea of the seasonal variations of CO at various altitudes.

Section 4: We are very interested in assessing how much of an improvement a data assimilation system strategy offers for a semi-operational system, such as developed in the GEMS project. Therefore it is of particular interest to compare the biases of the stand-alone models to that of the coupled model with assimilation so that we are able to say in the end that the coupled model with data assimilation is able to reduce the biases by X. For this reason we prefer to keep the validation of the stand-alone CTMs and coupled model together in the same section. To make this point more clear to the reviewer and potential readers we have added the following sentences to the first paragraph in Section 4,

"Comparing the model biases from the CTMs and coupled system allows us to assess how much of an improvement the data assimilation provides. To differentiate between improvements due to better meteorology and transport in the coupled model versus

GMDD

3, C178–C184, 2010

Interactive Comment

the data assimilation, we analyze both a coupled IFS/MOZART simulation with full data assimilation (ASSIM) and a control run with no data assimilation (CTRL)."

Section 5: The extent to which the models were able to reproduce CO plumes from the boreal fires was not necessarily known before hand, and we think that the sensitivity studies we have performed provide some information as to processes that are currently not well represented in the models. The fact that using what we consider to be a better emissions inventory (Turquety) for the boreal fires results in a deeper CO plume downwind in most of the cases suggests that the models are indeed able to transport the emissions long distances downwind, despite their coarse resolution. Due to lack of resources we did not perform a resolution sensitivity test in this study.

Conclusions: Regarding the first point, we rephrase the line "...showing that assimilation alone is not sufficient for compensating for other model inadequacies." to read as "...showing that the method used for assimilation does not provide enough information about the vertical profiles and is therefore not sufficient to compensate for other model inadequacies'

and add the following lines to the end of the last paragraph to emphasize potential errors in the 4DVAR assimilation procedure,

'Finally, the fact that the depth of the CO plumes is not well represented in the troposphere, and that in some cases the CO appears to be over-compensated for in the PBL (i.e.∼Case∼1) suggests that some improvements could be made in the assimilation process. One possible shortcoming of using the MOPITT V3 data without averaging kernels is that the assimilation could be biased to the a-priori profile. Therefore, in the current MACC (follow up project to GEMS) reanalysis, that will cover the period 2003-2010, averaging kernel information is used for MOPITT V4 data. This allows one to separate the contributions of measurement and a-priori information in forming the total column and will hopefully lead to improvments in the CO analysis.'

Regarding the second point, we replace the sentence 'This reflects the true variability associated with the injection height of emissions from boreal fires" to the following,

" One possible explanation for this is the fact that in reality there is considerable variability associated with the injection height of emissions from boreal fires, depending on the intensity of the fire and the present synoptic conditions. Therefore a parameterization which is based on these factors would be most accurate. However, we can not rule out the possibility that there are other factors in the model, such as mass conservation in the advection scheme and numerical diffusion, which inhibit the long-range transport."

Interactive comment on Geosci. Model Dev. Discuss., 3, 391, 2010.

GMDD

3, C178–C184, 2010

Interactive Comment