Geosci. Model Dev. Discuss., 3, C98–C101, 2010
www.geosci-model-dev-discuss.net/3/C98/2010/

Geoscientific Model Development Discussions

GMDD

3, C98–C101, 2010

Interactive Comment

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.

Anonymous Referee #2

Received and published: 15 June 2010

General comments:

The paper presents 1) an analysis of CO profiles collected in the framework of the MOZAIC program, 2) a comparison between CTMs and the CO profiles, and 3) a qualitative evaluation of the improvements gained from assimilation of MOPITT into a CTM, and 4) a sensitivity analysis of simulated CO to a couple of model "components" including total amount and injection height of biomass burning emissions. While there are several interesting aspects in the paper, I also have some concerns which may prevent the publication of the paper in its current form.

First at all, I am not convinced that this paper belongs to GMD. I see more elements pertaining to model evaluation than to model development. ACP or JGR may be more appropriate. In addition, the Authors try to include too many different issues and as a result, the paper is rather long (and wordy in some cases) and hard to read. Some interesting points get too diluted, and some other are not discussed in great depth. While I am usually not in favor of increasing the number of papers, the Authors should really question themselves whether they should produce two papers, one describing the analysis of in-situ measurements, and one about the comparison with CTMs and possible improvements gained from the assimilation (this is only one suggestion, there might be other possible combinations to shorten the paper and make it easy to read).

Specific comments:

– Introduction: The introduction is too long. Please be more specific in stating the objectives of the studies as well as the results from previous works. One issue is that as the paper addresses too many questions, there are too many previous works to refer too.

– Section 2: The description of the different elements is not too well balanced in my opinion. The MOZAIC program is now well known and does not need to be described in details. It seems to me that it would be enough to mention only what is specific to this study. In the mean time, very little information is provided about the assimilation procedure for example, while this has been far less described than the MOZAIC program.

– Section 3 about MOZAIC CO profiles: This section is a bit long and not so well written. For example, there are 3 or 4 pages on the analysis of the MOZAIC profiles but there is also a paragraph which tries to already summarize these 3-4 pages (starting on line 9 page 405) – is this really needed? Also, a comment about the lines 24 to 28 on page 405: two papers are cited which produce different results in terms of biomass burning emissions emitted for one specific year. The conclusion of the Authors is that “Indiscrepancies (btw, is that an English term?) between the two estimates are attributed to differences in how the CO emissions from boreal forest fires are estimated". Well, it seems to me that stating that two emissions inventories are different because the emission estimates are done differently does not provide any very insightful piece of information.

Tables 2 to 5 could be documented in the supplementary material, rather than being included in this paper. However I would include plots showing the seasonal variations at the various sites and at various altitudes (following the format of the plots shown in the next section but for the actual monthly mean, not for the MNMB). As far as I can tell, the seasonal variations of CO concentrations from in-situ measurements at various altitudes are not often reported in the literature.

– Section 4: Again this section is quite long and a bit hard to read because the discussion mixes two issues, including how well the CTMs perform and to what extent the assimilation improves the results. It seems to me that the two points should be differentiated, which may make the section easier to read. Also, it would be great if there was a bit more information provided (or information provided in a more concise way) about the extent to which comparing the CTRL and ASSIM simulations provides some information about the processes which are most likely poorly represented in the model.

– Section 5: It is to be expected that the global models would have a hard time reproducing the individual enhanced CO layers observed in MOZAIC given the vertical and horizontal resolution currently used in the study. It is also expected that the assimilation of total CO column may not improve the representation of particular layers, especially if the information concerning the averaging kernels is not included in the assimilation. Therefore, the Authors should state more clearly the insights gained form the analysis of case studies.

– Conclusion: The conclusion is relatively well written, however I disagree with some of their statements to some extent. For example, the Authors say (pp 419-420): "Overall

GMDD

3, C98–C101, 2010

Interactive Comment

the ASSIM model performed better than the other models, however, the CO plumes were still much too weak in terms of concentrations and not always at the correct altitude in comparison to the MOZAIC observed profiles, showing that assimilation alone is not sufficient for compensating for other model inadequacies": It is not clear to me whether the assimilation alone is not capable to compensate for model errors and/or inadequacies, or whether this is due to the fact that there is little information – if any – about vertical profiles which is provided by their assimilation of the total CO column.

The Authors state: "While results from the sensitivity test indicate that in some cases using a higher injection height can improve the transport of the CO plumes downwind, in other cases the impact is not evident. This reflects the true variability associated with the injection height of emissions from boreal fires": It is not clear to me whether this reflects the variability of injection heights and/or the inability of the model to reproduce LRT of biomass burning in given situations.

Could the Authors comment and possible re-phrase these statements?

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