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Atmospheric Chemistry and Physics Discussions

Interactive comment on "How much CO was emitted by the 2010 fires around Moscow?" *by* M. Krol et al.

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We thank the reviewer for the comments. We will reply to the main comments below, and indicate how we will change the manuscript in reply to the comments. Comments are repeated in italic.

1. The sensitivity studies demonstrate that the assimilation is strongly driven by the IASI measurements, as it exhibits hardly no dependence on the a priori inventory used. I find, however, that the range of the a posteriori emission strengths is quite narrow (22-27 Tg CO), and I suppose that this is due to a combination of the very large amount of available observations and of presumably small errors assigned to the IASI observations during the assimilation. By assigning larger errors to IASI observations, e.g. a two- or four-fold increase, and carrying out a sensitivity inversion could help to de-

termine whether the assimilation results remain robust towards the choice of the IASI errors.

We already inflate errors on IASI observations by a factor 50. We now performed a sensitivity with an inflation factor of 150 and we find that the results hardly change. Indeed, the background part of the posterior cost function (the part that quantifies the deviation of the posterior emissions from the prior) remains smaller than 5% in all inversions, signalling that the cost function is dominated by the deviations from the IASI data. As we replied to reviewer 2, the derived range (22-27 Tg CO) does not reflect all possible sources of error, such as transport errors. As we pointed out in the manuscript, these errors are more difficult to assess compared to errors resulting from the prior emissions, or the vertical emission height. In the revised manuscript we will highlight this, and we will also show the results of the extra sensitivity simulation.

2. Konovalov et al.(2011) reported that 2010 fires in Moscow region and surroundings emitted around 10 Tg CO, i.e. more than 85% of the total annual anthropogenic CO emissions. Further, they estimated that 30% of total CO fire emissions in European Russia originates in peat fires. Is the present study capable to make a rough estimate of the contribution of peat fires to the derived CO emission from IASI?

We did not specifically optimize for peat versus non-peat CO emissions as was done in Konovalov et al. (2011). We therefore cannot really differentiate between CO from peat fires and other fires. However, we refer to detailed studies that show that the region east of Moscow suffered from heavy peat burning. Given the large increment calculated for the emissions in that region, this probably signals that peat emissions are underestimated in the prior. Konovalov also contacted us and mentioned that our interpretation of the diurnal variations in his manuscript is based on a misunderstanding. We will correct this interpretation in the revised manuscript.

3. The Russian fires emitted massive amounts of smoke and aerosols into the atmosphere. However, there is no mention about aerosols in the paper and their potential *impact on the IASI retrieval. This could be a strong source of uncertainty and to address it would benefit the paper a lot.*

This is an interesting issue. There is no direct broad-band absorption of smoke or aerosols in the CO spectral range. But aerosols might impact on the temperature retrieval, which is needed for the CO retrieval. So, if there is a an effect on the CO retrievals, the effect is indirect. Moreover, the lifetime of CO is quite long. We take into account a large amount of IASI observations that track the CO after emission, and after the CO has been lofted to altitudes at which IASI has larger sensitivity. So, local effects cannot be ruled out, but we think that the impact on the results will be minor.

4. The CO column is measured with 10% accuracy or better (p.28709, l.17) The errors are then inflated by a factor of 7. Please explain how this factor comes up and what is finally the IASI error used in the assimilations. The fact that the assimilation is strongly constrained by the IASI observations could be due to IASI errors assumed too low in the assimilation system. Furthermore, it is highly recommended that error bars for IASI and MOPITT columns are plotted in Fig. 2, or better in a separate plot.

First, the error inflation (factor of 7) should account for spatial and temporal correlations in the mismatches between model and observations. This error is very difficult to estimate, since it also depends on the varying correlations in the modeled fields. A fixed error inflation is a simple pragmatic choice. We think that a detailed discussion of these errors is beyond the scope of this manuscript. Moreover, we performed now an extra inversion with larger inflation of the errors (factor 12) and the results appear hardly sensitive. Second, plotting the errors is figure 2 will be very difficult. The main reason is that the plotted columns are averages over a large geographical domain. If we would assume random errors on the individual IASI of MOPITT columns, the error on the mean will roughly scale with $1/sqrt(N)$, with N the number of observations in the average. Nevertheless, we will calculate the mean error and plot the error in the revised manuscript if meaningful.

5. p.28711, l.1-5: Please provide details about the a priori direct anthropogenic CO, and biogenic emissions, as well as the CO produced from anthropogenic and biogenic precursors as simulated by TM5 for the regions under investigation (R1 and R2).

We agree that the method section is very short. We will provide more information in the revised manuscript.

6. Apart from the comparisons with MOPITT, the findings are not confronted to independent validation datasets, especially from ground-based measurements. Aren't really any data available?

We found two datasets. First, we now compare to the actual CO measurements of the MSU station in Moscow. This comparison confirms that the posterior emissions still heavily underestimate the observations in Moscow. We discussed possible reasons for this already in the manuscript. Second, we now compare to measurements from the HUMPPA-COPEC-2010 campaing that was conducted in Hyytiälä, Finland. The plot (will replace figure 5) is attached. We think that the comparion in Finland convincingly shows that the posterior emissions clearly lead to an improved simulation.

7. Negative emission updates are inferred in region R2 (Fig. 3). The color bar does not allow to quantify the actual increment. Could you comment on these negative values?

The lower panel in figure 3 shows the increment in %. In most of the regions, these increments are positive (implying more emissions). In the blue areas the increments are negative, implying that less emissions are needed. We note here that the prior emissions in that region are rather small. Also, the posterior emissions always remain positive, since we implemented an algorithm that avoids negative emissions (Bergamaschi et al., 2009). Finally, in all source inversions the calculated emision increments may compensate for model errors. For instance, too fast vertical transport would lead to a too early detection of the CO by IASI and the need to lower emissions over that loacation. In general, however, there is no reason to be surprised by neagative emission increments.

8. No details about the assimilation method are given here. Even for those familiar with the method, a section describing the settings and the assumptions of the assimilation is required.

We will provide more details in the Appendix of the revised manuscript. We will also address the minor comments given by the reviewer.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 28705, 2012.

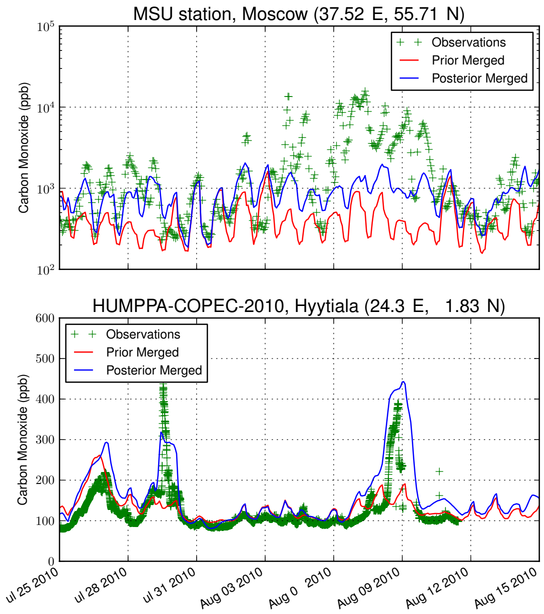

Fig. 1. Validation plot. Note that the upper panel has a logarithmic scale.