

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 careful evaluation of our manuscript. Below, we will address the main points and outline how we will adapt the manuscript.

I think properly accounting for model transport errors (e.g. vertical mixing and turbulence) is not highlighted in the abstract as one of the main limitations (or reasons of the discrepancies with other estimates) of this approach

We agree with the reviewer on this point and will adjust the abstract accordingly.

It is important to note that discrepancies in the estimates can be attributed to various components of the system. While, to the first order, the large-scale bias in the prior modeled CO columns can be mostly attributed to bias in prior emissions, discrepancies

C12763

across estimates cannot be attributed solely to differences in methodology, as they can be features of components unaccounted for in the system

Again, we totally agree here. In the discussion on page 28714 we therefore stated: "Second, the translation of emissions to modeled CO columns occurs on limited spatial resolution and is thus influenced by model errors. These latter may concern the emission process (emissions heights, temporal distribution), or the subsequent transport processes (convective redistribution, advection). On the larger scales (e.g. R2) small-scale mismatches are smoothed out and a favorable comparison is found. On smaller scales the deviations between model and IASI can remain considerable after optimization, as illustrated in Fig. 1". We think that this section exactly addresses the issue raised by the reviewer.

Secondly, I think the amount of IASI data (and coverage) is mostly providing the observed constraints in the emission estimates. I believe this should also be highlighted more in the manuscript

This remark comes a bit as a surprise, because we clearly state in the abstract (and elsewhere in the document): "It is shown that the IASI observations provide a strong constraint on the total emissions needed in the model". In reply to reviewer 1 we performed an extra sensitivity analysis with larger errors on the IASI data. This hardly impacts the results, confirming that the IASI data indeed put strong constraints on the emissions.

Thirdly, I am not clear as to whether the title is appropriate for this work. The title implies estimates of the magnitude of CO emissions together with their associated uncertainties. I think discussion on the uncertainty estimates (i.e quantification) is warranted especially for this work to be made useful to the community in terms of potential improvements in our understanding of Russian peat fires.

To some extend, we provide an uncertainty estimate in the abstract (22-27 Tg CO). However, we acknowledge that we used only one model, one source of satellite data

C12764

(although we compare to MOPITT), and a limited set of sensitivity experiments. Nevertheless, we think that this is the most advanced study to date that uses sophisticated 4DVAR satellite data assimilation. Also, we compared our simulations with optimized emissions to CO measurements made in Finland (results will be included in the revised manuscript) and we find a good correspondence. This gives confidence in the magnitude of the derived emissions. Probably, however, the true uncertainty is larger given the fact that some of the structural error sources mentioned by the reviewer cannot be investigated. We will mention this more clearly in the revised manuscript.

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

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