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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.

Anonymous Referee #2

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General Comments:

The manuscript presents top-down estimates of CO sources from the 2010 fires around Moscow using TM5-4DVAR inverse modeling system, in conjunction with IASI total columns of CO. A suite of sensitivity tests was carried out to explore the range of estimates as a result of using different assumptions in the prior emissions, injection heights, and timing. The range serves as a proxy in characterizing the posterior source uncertainties. These estimates were then compared with estimates from previous studies of the massive fire event. They find significant differences between their results relative to the other estimates, pointing to an important need to reconcile estimates from different approaches.

I find the manuscript to be timely, well-written, easy-to-read and suitable for publication

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Interactive Discussion

Discussion Paper

in this journal after some revisions. This manuscript provides important contribution to biomass burning studies especially over peat-burning regions, which are yet to be well understood. However, I have several concerns with regards to the methodology, and presentation/interpretation of their results.

Specific Comments:

Firstly, I think the manuscript needs to highlight how the errors are represented/characterized in the inverse modeling system, including model and representativeness errors. This is certainly a critical component of any inverse modeling work. More importantly, 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. While this issue has been discussed in the latter section of their discussion and conclusion, I think this has to be stated upfront, in conjunction with what is currently being described as features that distinguishes their modeling approach from other studies in the past. The conclusion of the weak sensitivity of the estimates to the choice of injection height and prior emissions is dependent upon the bias in vertical transport (as diagnosed by the higher PBL heights of the model). In addition, while the use of semi-exponential PDF of the prior emissions and the use of nested modeling approach is commendable, I think the resolution of the model (1 deg) also play a major role in their estimates, not just through broadly accounting for aggregation and correlated measurement errors but also on subgrid scales unresolved by the model (e.g. convective scales and radiative feedback). The reported sensitivity of the estimates will also be dependent on this. 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 across estimates cannot be attributed solely to differences in methodology, as they can be features of components unaccounted for in the system. I am not clear whether the last statement in the abstract is sufficiently robust given the reasons discussed above.

ACPD

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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.

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.

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