Atmos. Chem. Phys. Discuss., 11, C15184–C15188, 2012
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hindcast experiments of tropospheric composition during the summer 2010 fires over Western Russia" *by* V. Huijnen et al.

Anonymous Referee #2

Received and published: 8 February 2012

The manuscript by V. Huijnen et al. is a model investigation of the atmospheric composition during the period of severe wildfires in the Western Russia in July – August 2010. The goal is to improve accuracy of forecasting air pollution from wildfires. Satellite data from MODIS, MOPITT, IASI, OMI, and SCIAMACHI were assimilated by a CTM forecasting model and the authors believe that accuracy of the model improved. The paper operates with the FRP technique to estimate emissions, as well as with current satellite retrievals. An important drawback is ignoring results of published ground-based measurements. Also a critical analysis of FRP technique has not been presented. However, the paper may be published after revision.

General remarks.

First of all, the title made me to open a dictionary: the word "hindcast" is a modelers' slang. Moreover, there is no explanation for this term in the introduction. Please, add a couple of sentences about hindcast, forecast, reanalysis, etc. Also the word "experiments" in the title is misleading. This is a minor problem, however, if both are explained in the very beginning of the paper.

The paper uses the FRP approach for the bottom-up emission estimate. However, a better accuracy of the FRP technique versus, say, "active fires" technique is not proven yet. E. g., the "active fires" technique used by Fokeeva et al. (2011) demonstrated much closer agreement with a "top-down" estimate for the Russian fires. Comparisons of FRP emission estimates with other estimates is not presented in the paper. I understand, that the "active fires" technique might be too time-consuming and not usable for operational forecasting, but this should be explained clearly.

A weak point of the paper is insignificant citing of literature and practically lacking information about experimental techniques and retrieval algorithms. Also, to characterize any algorithm or experimental technique one sentence is enough, but it should be done. MACC is not the only forecast system: GMI is being developed at the NASA/Goddard, others may also exist. Numerous ground-based measurements of the atmospheric composition (not only AERONET data) for this event have been published and must be compared with the model results (see references below).

The authors noted: "the highest sensitivity of the [satellite] instruments ... [that] provide data used in the assimilation is to the stratosphere". It is correct. According to Fig. 1, the sensitivity of IASI column to CO in the bottom layer $\sim$ 400 m thick is just $\sim$0.15. Real change of partial column (or a change in the computed value due to another emission rate) in this layer should be multiplied by 0.15 before adding to the entire retrieved (or modeled and convolved) column. This is what is usually meant by the low sensitivity of TIR IR sensors to surface emissions. This fact diminishes accuracy of any schemes for top-down estimates based on TIR satellite data only (without ground-based validation). That is why using ground-based data would improve the paper a lot, or, at least, indicate the accuracy of computations. As for CO, it would be useful to compare model surface

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concentrations (Fig. 7) with observed surface concentrations on the same graph. The same can be done for available surface data for other components.

I agree with the second reviewer that the authors care too much about a better "image" of the paper. The value of the paper would just increase if they demonstrate real accuracy and real problems of the model used. Also they should look in a wider context, e.g., an interesting question is NO2 perturbation due to fires: the temperatures might be very high, formation of NOx might occur. Does it happen or not? Comparison with in situ measurements before and during fires (see references) in Moscow would be highly interesting. A paper in ACP should not be devoted to testing a model only.

Specific remarks.

1. CNT is not spelled out. 2. IASI CO retrievals during Russian fires are found to be more accurate than those by MOPITT and AIRS (Yurganov et al., 2011). There are traces of (previously used?) IASI data (CO averaging kernel in Fig. 1, also p.31864, line 4, p.31864, line 3. I recommend to mention why IASI CO data were not used, if there is no possibility to compare the data for the same event, but different instruments and algorithms. 3. For the inversion module COPRAFIT a reference (Eremenko et al., 2008) is missing in the reference list (page 31865).

Summary. The study is devoted to a problem that is very important both practically and scientifically: forecasting air composition perturbations due to fires. The authors have done a good job, but they did not devote enough attention to real accuracy of the specific forecasting scheme. Accuracies of both FRP and satellite data are questionable; this point should be elucidated in more detail. A significant revision of the paper is necessary.

References

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Interactive Discussion

Discussion Paper

Izvestiya, Atmospheric and Oceanic Physics, 2011, Vol. 47, No. 6, pp. 682–698. ISSN 00014338

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 31851, 2011.

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11, C15184–C15188, 2012

Interactive Comment