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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric impacts of the 2010 Russian wildfires: integrating modelling and measurements of the extreme air pollution episode in the Moscow megacity region" *by* I. B. Konovalov et al.

Anonymous Referee #2

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Referee Comments to the manuscript acp-2011-211.

Atmospheric impacts of the 2010 Russian wildfires: Integrating modelling and measurements of the extreme air pollution episode in the Moscow megacity region by Konovalov et al.

The paper presents the results of modeling of extreme air pollution over the Moscow region caused by intense wildfires in summer 2010. Emissions from forest and peat fires are estimated using Fire Radiative Power (FRP) measured by MODIS, corrected

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Interactive Discussion
Discussion Paper

for possible underestimation due to fire smoke, and emission factors from literature (Wiedinmyer et al., 2006). Emission factors for CO and PM10 are optimized by fitting model predicted air concentrations with surface measurements at a selection of sites and the average values of derived scaling factors are applied to adjust the emission factors of all other species. The air concentrations of CO, PM10 and ozone, calculated with the CHIMERE CTM, are compared with observations in the Moscow region, for the sites excluded from the emission optimization. The authors showed the applicability of FRP measurements, available from satellites in near-real time, to derive wildfire emission factors and made an estimate of total emissions due to the 2010 Russian fires. It is also shown that accounting for fire emissions significantly improves calculations of CO, PM10 and ozone with the CHIMERE model for the Moscow region. The importance is demonstrated of accounting for the reduction of photolysis rates due to solar radiation absorption by smoke aerosols.

The subject of the paper is quite relevant, given considerable (though episodic and geographically limited) effects of wildfires on air quality. The work is in particular interesting because the availability of emission data from wildfires at spatial and temporal resolutions required for regional modeling is still lacking (or rather limited). In general, the paper is fairly well written, though it could benefit of being somewhat less long, better structured (see comments) and checked for typos. The paper is well suited to be published in ACP, after the authors have considered/addressed comments and recommendations below.

General: 1. The paper contains a good deal of discussions in almost all sections concerning quite a number of assumptions involved in the work, so that it gets rather demanding to keep overview of the effect of all of them on final results. It could be recommended to have a separate section summarizing the uncertainties in emission estimates and model calculations due to the assumptions made and discussing the sensitivity of results to uncertainties in input parameters and modeled processes.

2. In the Introduction, the authors make an overview of model calculations of wildfires,

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but do not specify which emission data those models used. Also, it'd be useful to see some quantitative comparison of fires emission data from different methods/databases reviewed in the Introduction, giving a feeling about how close to or far off each other those estimates are. Also when selecting emission factors for this work (Table 2), it'd be relevant to show the range of variability of emission factors available from different estimates.

3. It's advisable to include a map showing the location of all sites considered in the work and those selected for the optimization, including an explanation concerning the selection of sites, their number and location. Can the authors say how the choice of "optimization" sites affects the accuracy of model calculations compared to measurements at the "validation" sites?

4. Very large traffic emissions of NOx cause ozone titration in normal conditions in Moscow. Very large emissions of NMVOCs (compared to NOx) reduce the titration effect and contribute to even larger production of ozone. Could the authors give some comment on that?

Other minor comments: P. 1241 L. 2: correct 'pollulated' to polluted;

L. 3-4: I'm not sure that the paper really "analyzes the chemical evolution of the atmosphere..", but rather attempts to reproduce with a model the observed pollution episode

P. 12144 L. 19-25: Not very successful formulation. It is obvious that the anthropogenic air pollution in Moscow is very large and should not be neglected. On the other hand, it was clearly that the wildfires caused those severe pollution episodes in Moscow - "likely dominant"?

L. 12: The most common . . ..

P. 12145 l. 23-24: repetition of previous page

P.12146 l. 15-16: " compared"... "underpredicted"

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L. 29-30: Explain why the “significance of peat fires” is an “important feature”? It is very unusual? Or changing the situation dramatically?

P. 12148 L. 14-15: Please explain why the temporal profile of fire emissions from Hodzic et al. (2007) has been used, even tough it inclusion of fire emissions in CHIMERE did not improve calculated temporal variability of AOD in that work

P. 12152 L. 15: Probably the authors meant to say “windblown dust generation” instead of “saltation”, which is only one on the processes which may cause dust production

L. 20-21: Suggested “European part of Russia” instead of several European regions of Russia”

L. 24-26: How can the authors explain that CHIMERE manages to perform for Eastern Europe comparably to Western Europe despite “potentially large uncertainties in the anthropogenic emission inventory data for Eastern Europe”? The reference is given to Vestreng (2004). do the authors think that the quality of East European emission data has improved during last 5-6 years?

P. 12158 L. 13: Explain “I” and “p”

L. 22: nine vegetative (?) land types

P. 12159 L.1 : Strange use of the word “complication”

P. 12162 L.13-14: Suggestion: making the assumption about linearity of PM10, explain already here 9noted in p. 12164) that PM10 is dominated by primary particles in the fire episodes.

P. 12165 L. 10-15: I think the text is a bit confusing. I’m not sure I understand correctly/ or agree with the authors. For ex. I cannot see that “F1 and F2 are significantly larger” for CO than for PM. The bottom line is that F < 1 means that the emissions were originally overestimated, whereas F>1 indicates emission underestimation. That means from Table 3. that both CO and PM emissions were overestimate for forest

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(especially PM) and underestimated for peat (especially CO). If this is what the authors say, it should be stated more clearly.

Combining Table 2 and 3, I arrive to emission factors for PM10 about 5x10ˆ-4 kg/MJ, which is about two orders of magnitude lower than those used in Sofiev et al. (2009). This is despite maximum FRP values were used. Could the authors comment on that?

P. 12174 L. 2 . . . other types of vegetative land cover

L. 6: The MODIS AOD measurements were used to correct / to eliminate a negative bias (instead of compensate).....

L. 17 : . . . this study showed/demonstrated the feasibility

L. 19: “The comparison of results.... confirmed” - one did not need model calculations to see that wildfires did cause those pollution episodes

L. 25: .. measured ozone concentrations....

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 12141, 2011.