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Comment

***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.***

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We thank the reviewer for the generally positive evaluation of our paper and useful comments. All the questions and comments are carefully addressed in the revised manuscript. Below we describe our point-to-point responses.

Comment: 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

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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

A separate section (Section 4.3) summarizing major assumptions and discussing associated uncertainties is added to the paper.

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

We have specified that the mentioned global modeling studies used wildfire emission estimates obtained with the burned area approach. Regarding regional studies, it was already noted in the discussion paper that “while Wang et al. (2006), Hodzic et al. (2007) and Larkin et al. (2009) used wildfire emission inventories based on the burned area approach, Sofiev et al. (2009) derived aerosol fire emissions from FRP measurements”.

We are not aware of any peer-reviewed publication where estimates obtained with the different methods discussed in the Introduction would be systematically compared. However, in response to the reviewer's comment, we mention the results by Roy et al. (2008) of a comparison of the MODIS burned area product with alternative burned area estimates obtained by mapping the MODIS active fire data.

In our opinion, presenting the range of variability of emission factors available from different estimates in Table 2 would be misleading without an extensive review of available literature on this subject and without paying attention to differences in experimental techniques, potential uncertainties, conditions of measurements and types of vegeta-

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tion considered. This is indeed a very broad topic which was quite recently reviewed by Akagi et al., 2011. Values given in this review are not directly applicable in our case because of differences in the definitions of land cover types. However, in response to the reviewer's comment, we added a brief discussion of uncertainties in emission factors to Section 4.1.

Comment: 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?

Maps showing the location of the considered monitoring sites in Moscow are added to the revised manuscript. A brief explanation concerning the selection procedure is provided in the figure caption. This procedure is discussed more in detail in Section 2.2.

To test sensitivity of the model calculations to the choice of optimization and validation sites, we repeated the optimization and validation procedures after "swapping" the optimization and validation subsets of CO and PM10 monitors. The results were found to be very similar to those presented in the paper. The corresponding remark is added to Section 5.2.

Comment: Very large traffic emissions of NO<sub>x</sub> cause ozone titration in normal conditions in Moscow. Very large emissions of NMVOCs (compared to NO<sub>x</sub>) reduce the titration effect and contribute to even larger production of ozone. Could the authors give some comment on that?

The reviewer described a possible interesting effect associated with nonlinearities in ozone photochemistry. We expect that this effect could indeed be manifested in the considered situation, but we do not think it would be feasible to investigate it in this paper (which, as the reviewer noted, is already large). The possibility of this effect is

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mentioned in Section. 5.3 of the revised manuscript, and we hope that we will have an opportunity to study it in the near future.

Comment: P. 1241 L. 2: correct 'pollulated' to polluted

The word is corrected (it should be read as populated).

Comment: 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

We agree that this paper “attempts to reproduce with a model the observed pollution episode”, but it also analyses the evolution of the surface concentrations of CO, PM10 and ozone. In particular, we evaluated the contributions of fire versus anthropogenic emissions and tried to elucidate the roles of several factors by means of special numerical experiments. We hope that the fact that we attempted to reproduce the observed episode becomes obvious in the next sentences of the abstract (for example, where we speak about the model performance). Unfortunately, we could not find a better formulation of this point.

Comment: 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”?

The criticized statement is removed.

Comment: L. 12: The most common : ..

The correction is made.

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

Repetition is avoided by abridging the corresponding paragraph in page 12144.

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Comment: P.12146 l. 15-16: “ compared”... “underpredicted”

The correction is made.

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

We explain in the revised manuscript that it is an important feature particularly because peat fires could be neglected in the situations addressed by Sofiev et al. (2009). In other words, in our study we had to take additional measures in order to properly calculate emissions from peat fires.

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

We added the citation from the paper by Hodzic et al. (2007) who found that “hourly resolution of wildfire emissions gives better results” (compared to the case of the daily resolution) “when simulating the impact of large wildfire events and comparing the modeling data with satellite observations dominated by biomass burning aerosols”.

Comment: 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

We agree with this remark. The corresponding sentence is corrected.

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

The nested domain covers only a central part of European Russia. The sentence is corrected accordingly.

Comment: L. 24-26: How can the authors explain that CHIMERE manages to perform for Eastern Europe comparably to Western Europe despite “potentially large uncertain-

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ties 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?

We think that this is probably because emission uncertainties are not a major factor limiting the performance of our model (as suggested by results found in our earlier studies). The mentioned reference may be misleading in this context and is removed. In this study, we used more recent data (please see p.12155, l.9-10 of the discussion paper), although we do not know any evidences that the East European emission data has improved during last 5-6 years.

Comment: P. 12158 L. 13: Explain “l” and “p”

An explanation is added. Additionally, we noticed that Eq. 5 was not quite adequate, and it is accordingly corrected.

Comment: L. 22: nine vegetative (?) land types

They are not exactly vegetative (how to characterize urban area, for example?), but we tried to clarify this point.

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

The sentence is corrected.

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

The mentioned sentences are revised.

Comment: 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 underesti-

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mation. That means from Table 3. that both CO and PM emissions were overestimate for forest especially PM) and underestimated for peat (especially CO). If this is what the authors say, it should be stated more clearly.

We discussed possible underestimation or overestimation of emission factors for one species RELATIVELY emission factors for another species. In other words, we are talking about ratios of the emission factors of CO and PM10. The corresponding sentence is corrected.

Comment: Combining Table 2 and 3, I arrive to emission factors for PM10 about  $5 \times 10^{-4}$  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?

In the revised manuscript (Section 5.1) we address this question as follows: “The ratio of the emission rate ( $E_s$ , see Eq. 5) to FRP determines the emission coefficient characterizing the amount (in grams) of the species  $s$  emitted per joule of the radiated energy. In our case, a value of this coefficient may strongly vary in space and time since it involves the correction factor  $C$  depending on the aerosol optical depth. In the case of PM10, the value of the emission coefficient averaged over the smaller (nested) domain covering the Central European Russia (CER) in the period of intensive fires from 20 July to 20 August is found to be about  $3 \times 10^{-6}$  g/J. This value is more than an order of magnitude smaller than a range of values 8–10 ( $\times 10^{-5}$ ) g/J) of the aerosol emission coefficient estimated by Ichoku and Kaufman (2005) for fires in Western Russia. It is also much smaller than values (1.8–3.5 ( $\times 10^{-5}$ ) g/J) adopted by Sofiev et al. (2009). However, our estimate is not very different from the emission coefficient value ( $\sim 5.5 \times 10^{-6}$  g/J) which can be obtained as a product of the fuel combustion coefficient ( $3.68 \times 10^{-4}$  g/J) reported by Wooster et al. (2005) and the PM10 emission coefficient for temperate forests (15 g/kg) recommended by Wiendinmyer et al. (2006). The differences between all these estimates may reflect yet a limited knowledge about potential biases in the MODIS FRP data, as well as the differences in initial FRP data

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processing. In particular, the maximum daily FRP values modified with the assumed diurnal profile were used in our study, while Ichoku and Kaufman (2005) considered the average of all fire pixels falling into each aerosol pixel. A number of tests carried out in a preliminary stage of this study indicated that differences in the MODIS FRP data preprocessing may indeed account for the mentioned differences in the emission coefficient estimates.”

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

The correction is made

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

The sentence is corrected.

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

A sentence is revised in a little different way, because a more careful statement was needed here in view of results by Sofiev et al. (2009).

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

The corresponding paragraph is rephrased. We tried to make it clear that we used the model to evaluate the relative contributions of anthropogenic and fire emissions. The results may look obvious in the cases of CO and PM10 but are not so obvious in the case of ozone.

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

The corresponding sentence is re-worded

References:

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