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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.

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I thank Dr. Chubarova for her interest in our paper and for her useful comments.

Our simulations of the evolution of CO and PM10 in the Moscow region are not sensitive to the choice of a value of single scattering albedo, because these pollutants are predominantly of primary origin. And in fact, one of the most important results of our paper is demonstration of a reasonable quantitative agreement of the observed temporal variability of CO and PM10 in Moscow with our simulation in an extreme situation when

the atmospheric composition was strongly affected by fires. We have demonstrated also that even though many factors affecting real ozone behavior are rather uncertain in our simulations, our model is capable of reproducing ozone measurements rather satisfactorily. Among the most uncertain factors are pyrogenic emissions of VOCs, which are not constrained in our study by any direct measurements. Probably, with a larger value of SSA adopted in the simulations, it would be necessary to apply smaller values of the correction factors (F1 and F2) to VOC emissions derived from FRP measurements. However, it is very unlikely, that any qualitative conclusion concerning the impact of fire emissions on ozone evolution could change.

As it is mentioned in our paper, we have adopted SSA=0.8 taking into account results reported by Meloni et al., 2006. They presented an extensive analysis of special measurements performed at the island of Lampedusa in the period from July 2001 to September 2003. In particular, using backtrajectory analysis, they identified aerosol from forest fires in Europe and found a rather broad distribution of SSA values ranging from about 0.7 to 0.95 both at 415.6 nm and 868.7 nm. The SSA values obtained from their analysis were compared to the values published in the literature (including publications cited in the comment by Dr. Chubarova), and the conclusion of this comparison was as follows: “the previous studies reveal a wide range of variability for the SSA of urban/industrial and biomass burning aerosol; our results are within these ranges at both wavelengths”.

We would be happy to use SSA values measured in Moscow during the considered period. Unfortunately, SSA measurements were not publicly available among several other physical characteristics measured at AERONET stations (http://aeronet.gsfc.nasa.gov/cgi-bin/type_one_station_opera_v2_new). The paper by Chubarova et al. 2009, in which SSA measurements in Moscow in 2002 were reported, is not a publication in a peer-reviewed journal, and it did not appear for us to provide a sufficiently solid ground for configuring our study. Specifically, we did not find there sufficient information to judge about representativeness of the reported SSA

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value (0.95±0.01) for the whole Moscow region and for different days associated with strong air pollution caused by fires. Moreover, we could not disregard an important inconsistency in the published results: as it is shown in Fig.11.10, a smaller than measured SSA value (0.90 instead of ∼0.95) should be assumed in order to insure an agreement of radiative transfer simulations performed with the TUV model with measurements of erythemally weighted irradiance. SSA measurements in Moscow in summer 2010 are not yet published and were not available for us. We certainly could not be aware of a paper by Chubarova et al. 2011, which is accepted for publication in the journal "Geography, Environment, Sustainability" (published by the Faculty of Geography of Lomonosov Moscow State University). I will be glad to find the important and interesting results of Dr.Chubarova published in one of more commonly known peer reviewed journals, and then it will be much easier for us (and, I believe, for many other scientists) to use them in our (their) future studies.

The second major comment of Dr. Chubarova seems to be a result of some misunderstanding. According to Eq. (6), emissions from both peatfires and other fires are assumed to take place simultaneously. Also, it is noted further that "although peat fires cannot be directly detected from satellites, we expect that if a crown or surface fire is observed over the dry peatland, there is some probability that a subsurface peat fire takes place at the same time."

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