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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. Yurganov for the interest in our paper.

As far as I understood, the main point of his comment is a claim that our emission estimates are wrong, while the estimates presented in ACPD paper by Yurganov et al., 2011 are correct. Unfortunately, it appears that before publishing this comment, Dr. Yurganov did not actually read our paper beyond the abstract. Otherwise, (1) he would undoubtedly note that CO emission values obtained in our study are also a kind of a

top-down estimate constrained by data of ground based monitoring in Moscow (and I'd like to note that in-situ ground measurement are in general much more accurate than satellite measurements of atmospheric composition). (2) I believe Dr. Yurganov would also take into account that in contrast to their study where "inversion" is performed with very crude and obviously outdated method based on a box model and using a rough correction of satellite measurements, our study is based on simulations performed in a consistent way with a state-of-the art 3D chemistry transport model. In this situation, it is especially unexpected that Dr. Yurganov mentions our assumption about SSA value, as one of the reasons for the emission "underestimation" in our study, while the model used in their study does not take into account optical effects of smoke aerosol at all (!). (3) I think that Dr. Yurganov could note that CO emission estimates reported in our paper concern only a European part of Russia, while their box model "covers" almost the whole Russia and even considerable parts of Kazakhstan and China. In contrast to the statement made by Yurganov et al. that Siberia is an area which is "obviously without fires", satellite measurements (see e.g., http://firefly.geog.umd.edu/firemap/) detected numerous fires there (as well as in Kazahstan and China) in summer 2010.

If Dr. Yurganov really believes that "traditional inventories" are already sufficiently accurate, then what is a scientific point of the study by Yurganov et al., 2011 and several other similar studies performed in the same way by the same leading author? Why care to elaborate and to publish alternative "top-down" emission estimates? (Actually, as far as I know, considerable uncertainties in available emission data are commonly recognized; our reasons for using FRP based emission estimates are in detail explained in Introduction).

Therefore I believe that the reasons for the discrepancies of our emission estimates are mainly associated with crude assumptions made in the study by Yurganov et al. 2011. I admit, however, that our study is also not perfect. Several possible sources of uncertainties in our estimates are mentioned in the paper. I strongly recommend Dr. Yurganov to read it carefully before publishing a next comment in the interactive discussion.

Reference: Yurganov, L., Rakitin, V., Dzhola, A., August, T., Fokeeva, E., Gorchakov, G., Grechko, E., Hannon, S., Karpov, A., Ott, L., Semutnikova, E., Shumsky, R., and Strow, L.: Satellite- and ground-based CO total column observations over 2010 Russian fires: accuracy of top-down estimates based on thermal IR satellite data., Atmos. Chem. Phys. Discuss., 11, 12207-12250, doi:10.5194/acpd-11-12207-2011, 2011.

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