Atmos. Chem. Phys. Discuss., 12, C823–C826, 2012
www.atmos-chem-phys-discuss.net/12/C823/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interaction of anthropogenic and natural emission sources during a wild-land fire event – influence on ozone formation" *by* E. Bossioli et al.

Anonymous Referee #1

Received and published: 22 March 2012

The manuscript presents a modelling study of the photochemical impact of a major wildfire event in Russia during summer of 2006. The subject of the paper is certainly within the scope of ACP and specifically addresses the question on how the emissions from wildfires and from biogenic sources interact and affect ozone production rates. Before considering publication on ACP, I suggest the authors to consider the following points:

1. I believe the title is not accurate. The manuscript reports on sensitivity tests perturbing fire and biogenic volatile organic compounds (VOC) sources, thus the word "anthropogenic" sounds a little bit out of context at the end of the reading. I suggest

modification of the title with "Ozone production from the interaction of wildfire and biogenic emissions: a case study in Russia during summer of 2006"

2. Introduction. I suggest adding this recent reference, which is a review of the subject of the paper: "Jaffe and Wigder (2012), Ozone production from wildfires: A critical review, Atmospheric Environment 51, pp. 1-10." This other paper addressed the issue of the interaction of wildfire emissions with BVOC and urban areas with respect to the production of ozone: "Junquera et al., Wildfires in eastern Texas in August and September 2000: Emissions, aircraft measurements, and impact on photochemistry, Atmospheric Environment 39 (2005) 4983–4996." It could/should be used as a term of comparison with results presented here. E.g. the authors report a major contribution of isoprene BVOC oxidation in the fire plume of 63% and 33% over a forest and near urban area, respectively. Moreover, I believe there are also other studies following the Russia major wildfire event of summer of 2010. E.g., from a quick search on ACP: "Atmospheric impacts of the 2010 Russian wildfires: integrating modelling and measurements of an extreme air pollution episode in the Moscow region, I. B. Konovalov, M. Beekmann, I. N. Kuznetsova, A. Yurova, and A. M. Zvyagintsev, Atmos. Chem. Phys., 11, 10031-10056, 2011."

3. Methods. In section 3.2 and 3.3 the impact of wildfires and BVOC emissions is studied. The method consist in using differences among these simulations: A. Reference: all emissions included B. NoFIRES: wildfires emissions off C. NoBIOG: BVOC emissions off D. NoFIRES+NoBIOG: wildfires and BVOC emissions off The impact of wildfires is assessed using the difference of runs A-D, that of BVOC using the difference A-C. Simulation D is discussed by the end of section 3.3, with no clear target. In my opinion, this method is not correct. According to the Factor Separation analysis framework presented by Stein and Alpert ("Factor Separation in Numerical Simulation", J. Atmos. Sciences, Vol. 50, No. 14, 1993), when the effect of two interacting factors are examined (as in this case), the following differences of runs listed above should be used: B – D for effect of wildfires alone C – D for effect of BVOC alone A – (B+C) + D

for the combined effect of wildfires and BVOC The difference between A and C or D is useful when only ONE factor is under investigation. However, as the authors state from the title, the objective of this paper is to study the interaction of wildfires and BVOC emissions, thus the Stein and Alpert (1993) framework should be applied. I believe all results presented in sections 3.2 and 3.3 should be reformatted in this context.

4. The authors should mention somewhere in the paper that they neglected the effect of concomitant aerosol emissions on photolysis rates. As also reported in the review of Jaffe and Wigder (2012), the "obscouring" effect of aerosol may decrease the ozone production at the surface by up to 20%.

5. Mixing vs. photochemistry. A good discussion point might be an analysis of the relative attribution of ozone production rates along the plume path of mixing with background airmasses and photochemical processing inside the plume of original emissions. This is an interesting point raised in the Jaffe and Wigder (2012) review, which is still unclear in the existing literature and thus might be an innovative contribution of this paper.

6. Role of PAN. Maybe a more detailed analysis of the evolution of PAN concentration is needed. Indeed, PAN is believed to be the main reservoir species regenerating NOx in the plume even weeks after the injection and is thus a key driver of the evolution of the VOC/NOx ratios in the plume as it travels. PAN is currently only mentioned in the sensitivity test on NOx/CO ratios in emissions. In the case of low NOx/CO emission ratio in fires the authors calculate that PAN makes a fraction of NOy of 25%, while in the high NOx/CO ratio the share is 40%. What is the magnitude in ppb of PAN in the two cases? Indeed, from literature (see Jaffe and Wigder, 2012, and references therein) it seems like PAN should be much higher in the low NOx/CO ratio case, because of the enhanced abundance of oxygenated compounds. Please clarify this point.

7. On the NOx/CO ratio. In the reference case a high NOx/CO ratio of 0.06 is chosen. Actually, the region of interest may be considered more similar to a boreal environment,

where usually lowest values of combustion efficiencies are reported (see again references in Jaffe and Wigder, 2012), which are associated with lowest NOx/CO ratios, with respect to other environments (e.g. tropical, savannah, etc. Shouldn't be the low NOx/CO ratio emission of 0.025 be used as the reference in this case?

8. Sensitivity tests. This tests may be quite useful at least for two reasons: (1) estimate uncertainty on the ozone production assessment present in the result section, and (2) identify the single parameter, if any, which dominates the simulation uncertainty. The authors describe the results of tests without showing any figure/table and do not draw any clear conclusion related to the two points just mentioned. I suggest showing some results (maybe in the supplementary online material, to avoid an excessive number of figures in the main paper) and better clarifying what the reader may learn from these tests. The latter point should be concisely repeated in the conclusions and in the abstract, because it could be a good contribution of the paper to the scientific community.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 3467, 2012.