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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Influence of the aerosol solar extinction on photochemistry during the 2010 Russian wildfires episode" *by* J. C. Péré et al.

Anonymous Referee #1

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The authors have included an interesting update to the chemical transport model, CHIMERE, involving an online coupling between aerosols and radiative effects on photolysis. This interaction has been shown before to be important in extremely polluted locations like Beijing but its importance in cleaner parts of the world is not clear. They focus on a wildfire event in Russia in order to address this importance for the model's performance against evaluation data in Moscow when the plume passes over. The difference between the model predictions with and without the new coupling are modest for ozone predictions. The major weakness of the paper is that the discussion sidesteps the comprehensive impacts of the wildfire, and instead only mentions the model's performance against itself (specifics described below). I would recommend the authors add some of this to the manuscript or perhaps submit to a more appropriate journal

like Geoscientific Model Development.

Major Issues:

1) The introduction is brief, and focuses mostly on the problem from the perspective of model development. Yet the larger environmental impact perspective is somewhat lost. In other words, the authors present the fire episode as an example where the model might screw up if it doesn't have correct optical feedbacks. But what is the nature of this model problem in the context of the effects of the fire on the total environment, or model domain? Is it emitting so many BC particles that the effects of photolysis on secondary particles are kind of negligible? Are the uncertainties from photochemical reaction rates swamped by uncertainties in precursor emission rates? A reader could leave this paper thinking that fire plumes act to decrease O3 concentrations, increase NO2 and slightly decrease PM10, and that all of these effects are mild. Of course the whole story involves massive emissions of a host of pollutants. I think this piece of the story needs to be emphasized more.

2) Figure 5 simplifies quite a lot of information and is a useful graphic. It also puzzles one at first glance. The JNO2 values have changed by about half of the change in JO3 as AOT increases. Yet the total change in O3 barely shows up (at about -3% at most severe). Clearly, the concentrations of NO2 and O3 are playing important roles here. I think it would be interesting to probe the model a little further and look at the actual rates of formation and destruction of O3 to get a more intuitive feeling of how the balance is affected by the aerosol direct effect. Also, why were the values here plotted for midday? Was it because it makes it easier to calculate AOT through a single column. As the authors say in the text, it's the time of day with the least atmospheric path.

3) Page 7070-7071, lines 24-4: I'm uncomfortable with this discussion and the conclusion that taking the photolysis impacts into account could have a non-negligible effect on the air quality prediction. It is clear from figure 7 that the model is quite wrong on

August 8. Yes, the model with aerosol feedbacks does very slightly better, but I think the authors are overstating the importance of the model improvement when they imply that it can make the difference between exceedance and attainment in air quality models. It seems like a relative coincidence after all, that the model without aerosol feedbacks falls above the threshold and the one with feedbacks falls below.

4) The authors focus almost exclusively on O3 and include one plot at the end of the manuscript addressing PM mass. However, just by looking at the substantial reductions in sulfate and OA, I would expect there to be a significant effect on aerosol growth and the size distribution in general. Can the authors discuss this? How are there 8 aerosol bins affected?

5) Since most of the discussion involves model development, I was surprised to not see a description of the computation time increase associated with incorporating the online coupling with the aerosols (including the core-shell calculations). This would help model developers decide whether or not they would like to implement the method in their own models (i.e. are the changes in O3 worth the time spent computing?).

Minor Issues:

1) The boundary conditions are not exactly consistent in time with the episode. Do the authors have an estimate for the influence of boundary conditions on their model domain? When predicting a very specific episode like this one, are they convinced that the lack of time dependence at the boundaries is acceptable?

2) What version of WRF is being run offline? A little more detail on some of the weather selections for WRF would not be inappropriate. After all, the indirect effect calculations and the convective transport modules are relevant to this study. This is quite relevant for figure 6, which might suggest that emissions from the fire are being injected too high.

3) What is TUV doing with sub-grid clouds? The gird cells (30 km) are somewhat

coarse even for a regional simulation when the study is focusing on a plume.

4) Page 7066, lines 10-23: While there is no denying that photolysis is critical for ozone formation in the atmosphere, the authors should also mention briefly the importance of VOCs in catalyzing ozone formation. The explanation given seems incomplete. This issue further relates back to the importance of emission inventory uncertainties compared to photolysis rate estimation uncertainties.

5) Figures 2 and 3: Please consider putting percent signs near the color axes for easy reference. Also, are these data for the lowest layer of the model or some column average? May I suggest the authors replace "diurnal-average" with "daytime average" here and throughout the text?

6) Page 7069, lines 6-9: This statement seems true at first read, but certainly depends on the aerosol concentration, which will vary day to day and isn't necessarily a minimum at midday.

7) Figure 8: Are the SO4 and SOA species for PM2.5 aerosols?

Presentation Issues and Typos:

1) Page 7065, line 28: Please consider putting the description of the simulations in their own section so that it is clearer to the reader that the computational experiment is being described. As it is now, the information is somewhat buried.

2) Page 7058, line 15: In "terms" of air quality. . .

3) Page 7058, line 26: "the exceedance of . . ." and please change exceed to exceedance as appropriate throughout the text.

4) Page 7060, line 6: Please replace 'emphasize' with 'focus'. There are many other English language typos throughout the paper. These don't detract from the scientific quality of the paper, but they do distract. Please have someone look over the manuscript with the intention of correcting these mistakes.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 7057, 2015.