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Interactive comment on “Comparison between simulated and observed chemical composition of fine aerosols in Paris (France) during springtime: contribution of regional versus continental emissions” by J. Sciare et al.

Anonymous Referee #1

Received and published: 7 August 2010

The paper presents a 3-week intensive measurement study in the city of Paris, from which the OC/EC profiles were analyzed and compared with regional model simulations. Except OC/EC, total ion concentrations are estimated from previous studies' correlations, as well as SOC/OC, based on two independent approaches. Finally, a source attribution is presented, based on back trajectory analysis, supported by model results.

The results in general are adequately explained and the assumptions made are clearly mentioned. The manuscript is clearly written and the conclusions are most of the time

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convincing. The methods used are explained in great detail, except an important part missing from the model description that will be addressed below (comments #5 and #13). I suggest the work to be considered for publication in ACP, after addressing the following minor issues.

1) A plot showing the mean diurnal profile of the aerosols studied during the three periods can be very useful, since it might support the idea that ions originate from far away, thus do not present the expected diurnal variation of an urban site, while the carbonaceous aerosols do. This was shown in general in figure 9a, but the inclusion of ions and the separation in the three periods might provide useful additional information.

2) Page 16865, lines 3 and 20: artifact-free measurements: this is a very strong statement and I heavily doubt that it is correct. Maybe the new measurements are much better, but artifact-free?

3) Page 16869, lines 2-5: Sea-salt is very low even when marine air masses arrive in Paris? How low (in order to be considered negligible) are sea-salt concentrations?

4) Page 16870, lines 16-17: The domain covering the north part of France does not appear in figure 2.

5) Section 3: a description of how the emissions are distributed between hydrophobic and hydrophilic should be included, as well as a description of the aging of carbonaceous aerosols. These are necessary in order to interpret the model's results.

6) Page 16875, line 8: How about the local and regional sources of NH₃, NO_x and NO₂?

7) Page 16878, lines 11-14: How does the model perform the resto of the studied period?

8) Page 16878, lines 17-21: Why these issues, and especially the numerical diffusion, are not present during the rest of the studied period?

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9) Page 16879, line 2: The simulation labeled “impact of regional emissions” is in reality an “impact of local emissions” study, and should be named as such.

10) Page 16879, lines 8-19: The discussion, in conjunction with figure 8, is confusing. Adding the red and grey lines is not expected to give the blue line, mostly because the sources are not exclusively local and EU (based on the authors’ EU definition), but also from elsewhere, sources that are accounted for in both the two sensitivity simulations. In addition, when a simulation is being called e.g. “impact of EU emissions”, one would expect to see how much the emissions of EU contribute. Instead, the grey line shows the impact of all sources, excluding EU.

11) Page 16879, lines 20-25: Inorganic thermodynamic equilibrium includes also NH_4 . The changes of NO_3 are also being affected by changes of NH_4 (thus NH_3 emissions) and have to be taken into account in the discussion.

12) Section 5.4: The definition of SOA is missing. Do the authors mean by SOA the organic aerosol mass produced by gaseous precursors, or also the aged primary organics as well? This definition is crucial for the understanding of the whole discussion.

13) Page 16880, line 26, to page 16881, line 5: This part cannot be evaluated without knowing the amount of WSOC vs. WIOC emitted, and the conversion of one to the other via aging. See also comment #5.

14) Page 16882, line 20: This does not make sense. The model does not have dedicated BC emissions, but has a scaling factor based on OC emissions?

Technical corrections

15) Page 16878, lines 10 and 16: 8-10 of June should be 7-9, based on figure 7.

16) Page 16883, line 23: “more stable” should read “less variable”.

17) Figure 8: The colors in the figure and the text do not correspond with the colors in the legend. My guess is that the legend is wrong, otherwise the whole discussion is

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off.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 16861, 2010.

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