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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Formation of organic aerosol in the Paris region during the MEGAPOLI summer campaign: evaluation of the Volatility-Basis-Set approach within the CHIMERE model" *by* Q. J. Zhang et al.

Anonymous Referee #2

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This paper is an interesting study of the Paris region making good use of measurements and PMF analysis from the MEGAPOLI campaign. The paper is well written, and apart from some suggestions as given below, is suitable for publication in ACP.

Major comments:

1. On page 29482, the authors point out that they use only the PMF solution from Freutel et al.. Explain why this choice was made, and any possible consequences of other choices.

2. In general, the paper does a good job of considering many of the uncertainties involved in OA modelling. I miss mainly some details of the emissions.

Firstly, a small discussion of the accuracy of the biogenic emissions is needed. The model uses MEGAN. A recent paper by Langner et al. has shown factor 5 differences between BVOC emissions in different European models, with MEGAN giving by far the highest estimates. This is an obvious source of uncertainty in BSOA predictions, and warrants some words. (I also wonder if a lower BVOC emission and higher ageing rate would give similar conclusions.)

In Section 3.2 I wonder about the consistency of the emissions for inorganics versus organic emissions. The emission factors are from different sources, but are the spatial distributions consistent? How does the MEGAPOLI database compare to the standard (LA) one, also even outside Paris? Some maps of the emissions in Paris would be helpful (replacing Fig. 1 say) to clarify such things.

On p29486, last paragraph, the emissions story is also rather confusing. What is the relation between the PROCARB diesel emissions and assumptions, and the factor 1.5 mentioned above. Here the authors find some other fraction (75%).

Finally, I wonder how consistent these Junker and Louisse based emissions are with the EMEP emissions used for all else. A common problem is that bottom-up estimates of POA emissions may be larger than estimates of PM2.5 emissions. Some words on this for the Paris area would be useful to set the results in the context of other European studies.

Minor comments:

Abstract, p29477 line, rephrase sentence with "model mostly after long range"

p29479, line 16. POA "used to be" generally considered...

p29479, line 17. One of the Donahue or Robinson references would probably be appropriate alongside Pankow here.

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p29480, line 3. I don't know why the author say "especially" here, what do the latter papers have to do with the framework. They are rather updates aren't they?

p29480, line 14. "derived from" instead of "applying"

p29480, line 18. "Better agreement", compared to what?

p29482, line 10. Say "incoming" rather than "encountered"

p29486. Does Seinfeld and Pandis give much data on Atlantic OA for boundary conditions? What is meant by climatological analysis - whose, where?

p29486, line 1. The 1.5 fraction includes SVOC as well as IVOC.

p29486, The PROCARB reference has no address, and the title is given in French. Add address and translation, and some words on what this is for English-only readers.

p29489, line 20. "appropriate"

p29489, line 27. To put the different VBS studies in context, it could be mentioned that other studies (e.g. Lane et al, 2008) use a higher ageing rate.

p29490, item 2. Again, I think SVOC as well as IVOC is included in the factor 1.5 POA.

p29492, lines 13-14. I would have said that the peak temperatures were underestimated, not "well represented".

p29493, line 5. Say "is reduced" rather than "gets lower".

p29494, lines 10-11. No need for 3 significant figures in bias numbers.

p29495, lines 12 on. Say just "relative biases", not "biases (relative biases)".

p29495, line 14. Say "larger", not "more important".

p29497. What about coarse nitrate?

p29497. I am not convinced by the arguments concerning sulphate, OH and RO2.

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Firstly, if SO4 is a result of long-range transport, problems in reproducing radicals at an end-point site inside a mega-city are probably irrelevant. Also, after long range transport I would expect all sulphur to be as sulphate, regardless of moderate uncertainty in chemical formation rates.

p29497, last sentence. Why mention just the financial crisis? Surely emission controls, traffic growth, and other factors cause emissions for 2009 to be different to those of 2005.

p29498, line 6. Say "the CSS method", and remind the reader where to find this explained.

p29501, lines 20-24. Some of the problems with the morning peaks are probably due to dispersion, as evidenced from the comparisons for NOx and BC. Some mention of the implications of this is warranted I think.

p29504, line 13. Say model evaluation, not control.

Tables:

Table 1. Give full name of sites.

Table 3. I found this a little confusing. The VBS approach etc should be clearly underlined as being headers. Make it explicit what blanks mean. (e.g. not included, or not available.)

Figures:

Fig. 2 on, the x-axis for the month-plot is presumably day of month, rather than local time? State which month in the caption. What is white "fond"?

Fig. 3 on, make it clear if the data are hourly or daily averages.

Supplementary:

Table S1. Cite source for mass-distributions used here.

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Table S3. Make it clear that these are mass-yields.

Table S4. Explain "R" and "RMSE".

Table S4, on. Give dates of comparisons, and number of samples.

References:

Langner, J. et al., A multi-model study of impacts of climate change on surface ozone in Europe Atmos. Chem. Physics, 2012, 12, 10423-10440

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 29475, 2012.