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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The size-composition distribution of atmospheric nanoparticles over Europe" *by* David Patoulias et al.

Anonymous Referee #1

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Evaluation of Patoulias et al., "The size distribution of atmospheric nanoparticles over Europe"

In this paper, the authors apply the PMCAMx-UF air quality model to a European domain to assess its ability to simulate nanoparticles over the domain. They suggest the model performs favourably, with the results generally within a factor of 2. They also assessed the simulated impact of organics, finding it leads to a large increase in N100 particles in parts of Europe. They also found using VBS improved model performance.

In general, this work appears to be well done, though is rather limited in its scope of model evaluation, the core of the study.

In a model application and evaluation such as this, it is important to provide some idea

of what is driving the model. Is it the emissions of nanoparticles (which is a rather uncertain quantity, particularly the appropriate size distribution to be used given the model resolution: how is the very near field dynamics of traffic emissions treated?)? Is it boundary conditions? Is it nucleation, and if so, from emissions within the domain or from boundary conditions? (Note: there is rather little discussion of boundary conditions or emissions: they should discuss both in more detail and provide a spatial distribution of emissions by size.. they can use NTotal, N10, N50 and N100, though as noted below, I would use N10-50, N50-100, N>100). Even if just for a one week simulation, they should provide the results of four additional simulations: Halving the BCs on all PM, halving the BCs on species that might react to form condensable species, Halving the emissions. (They need not use changes of one half, but something where the response would be seen if it is important). In the end, the article should address, very precisely, why the simulated levels are what they are, by size. For now, there is a bit of that for organics.

It is interesting that their simulated results are spatially more uniform than might be expected. Looking at Fig. 5, the simulated results are typically about 2500-10000 The observations go rather lower. This requires more discussion. It also appears as though the results at Hyytalia are dominated by boundary conditions that are fixed in time... Is this true? If not, an interesting pseudo-steady state appears to be at work that should be explained. On the other hand, the simulation shows more variability than the observations in Fig. 8. Again, rather more discussions is warranted as this is the focus of the paper.

The title should be changed. This article is not focused on nanoparticles over Europe, but the simulation of nanoparticles over Europe. It also has almost nothing on the actual composition (they have a small piece on sensitivity to SOA formation). Much more information and analysis is necessary to have the more general title. I would propose "Spatial distribution of simulated nanoparticles over Europe"

The comparison of results to the flight data is remarkable. They really do need to show what is contributing to the results, particularly how the bump at 800 m exists given the vertical diffusion found in most air quality models. The bump at 800 m is very interesting and really understanding it is key. Given the model's ability to capture this, I was surprised there was no discussion of it. They could take the analyses done to assess the processes leading to their simulated levels discussed above and use that.

The manuscript also does not provide any information on how well the model captures aerosol composition. If the model is within a factor of 2 on total number, but off by a factor of 2 on OC or sulphate mass, this has important implications, particularly if the differences are in different directions.

I think it would be better to show their results by size groups, e.g., N<10, N10-50, N50-100 and N>100. This would better demonstrate the variability in the different sizes, and if, for example, one size range is much more uniform than the others.

It appears that WRF is applied without nudging, but is simply reinitialized every three days/ Why? To what degree does the reinitialization impact the system? How does the performance degrade after multiple days?

In summary, the submitted manuscript is a good start on what can be a nice contribution to the literature, though it is currently too limited in what they have done and what they explain. The authors should provide a more extensive analysis on what processes drive their simulations. At a minimum, they should: 1. Provide a set of calculations showing how the model responds to changes in emissions and boundary conditions, and the role of sulfate nucleation, and the origin of the SO_2 (BC or emissions inside). 2. Provide some information on how well the composition is captured is also needed. Without such further information it is difficult to say whether the model results are reasonable or not. 3. Provide more detailed information on the model application, including vertical cell spacing, overall performance for more species. How well does WRF capture the meteorology? 4. Change the title to include "simulation" as the article does not really focus on the distribution of nanoparticles (as observed)

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