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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "The aerosol distribution in Europe derived with the Community Multiscale Air Quality (CMAQ) model: comparison to near surface in situ and sunphotometer measurements" *by* V. Matthias

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The paper discusses the use of the EPA's Models-3 CMAQ (Community Multiscale Air Quality) model applied to Europe and, at a higher resolution, to the North Sea with particular reference to comparing "ground level" PM10 and aerosol optical depth (AOD) between the model and various measurement facilities.

Overall, it is good to see work published relating to the application of the US code to the EU. The general approach appears sound and there is a large amount of statistical



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comparisons between ground-based measurements and model predictions.

The main message is that, in these simulations (which use suitable inputs for Models-3 CMAQ), the model always underpredicted total aerosol mass and thus AOD, the author suggests due to under representing organic aerosol (perhaps, partially, at the emissions inventory stage).

It is a little worrying that these Models-3 CMAQ simulations are giving consistent underprediction of aerosol mass (of up to 60%) near the ground. Is this seen with the numerous other CMAQ simulations? And do other models, for the same domains, give better or similar results? I'd like to see comparisons made with both these.

I note that the author refers to previous work to justify using only the dry aerosol mass in this paper. It would be interesting to include in this paper, reference to a statistical comparison also of the wet aerosol mass.

Whilst the paper is generally well written and contains much useful information, it would greatly benefit specifically from including maps showing the two domains and the position of the monitoring stations (Tables 1 & 2), eg overlaying measurements. It is not clear how the 2 domains are used and why most comparisons are on the coarse domain. In places more references are required and generally the paper would benefit from closer proof reading - the interchange between using measurement station names and a shorthand key is confusing; there are many mismatchs of references to figures and tables and the actual figures and tables; some figures are missing and others are inadequately labelled; improved English spelling and grammar (less use of "also"!). There is no 'future/further work' section.

Some specific comments:

2.1 - Chemistry transport model The version of CMAQ being used should be made explicitly clear as early as possible both in Section 2 but also in the Abstract. The paper is based on version 4.5 but there are more recent versions available - it should



ACPD
8, S954–S960, 2008

be explained why v4.5 is most appropriate for this study.

The description of Models-3 CMAQ would benefit from explaining that it uses 3 modes for aerosol, that these presume logNormal mass-size distributions (for each mode) and that not all species are modelled in each of the 3 modes. Indeed, throughout the paper there seems to be no mention of CMAQ's Aitken mode - surely this has not been ignored?

Model set-up: it's implied but not stated explicitly whether the new PAH method is used. I presume it is used but there is no discussion of how this addition scheme affects the model results - a significant omission.

Model set-up: is the 18 km resolution grid "nested" in the sense that data from the 54 km grid is used to provide initial & boundary conditions (if so this should be clearly stated) or in the sense that it's a completely separate simulation but the domain happens to cover a geographical subset of the 54 km simulation?

3.2 - PM10 results

It is not explicitly stated whether the use of PM10 and 'total aerosol mass' are interchangeable in these simulations - the version of Models-3 CMAQ used uses a modal representation of aerosol mass so summing the masses of aerosol outputs would not neccessarily be equivalent to PM10.

I note that the model output for "ground layer" is, as expected, the lowest model layer. It would be interesting to explicitly state the value of the top of this (terrain-following) layer at each measurement site and further work could involve higher resolved layers near the ground to investigate whether this improves the simulations. The author should clarify why it is more appropriate to use these data than the model's dry deposition values.

It is also unclear the difference between the 54 and 18 km resolution domains (see earlier comments), particularly why the focus of the comparison in on the 54 km resolution



ACPD

8, S954–S960, 2008

domain.

The statistical analysis would benefit from a brief introduction explaining the purpose, physical meaning and limitations of each statistical measures. Specifically, the number of points in the sample should be stated (eg Table 3); and eg (relative) standard deviation is the spread of daily means with respect to the annual mean and the comparison of this measure between the model and the measurements says little concerning the model's ability to capture the time evolution of aerosol concentrations, although it does indicate that the range of values is broadly similiar. Given the low correlation coeffs (Table 3) I would ask the author to give the time-series figures earlier in his discussion and he should consider providing (a summary of) such time-series for all monitoring stations and also on 18 km resolution.

The discussion concerning log-normal distribution of aerosol mass needs a sharper focus, for examples:

i) why do we expect a single logNormal dist - what about the Aitken (or coarse) modes?

ii) 'a' in the Table 4 (etc) needs defining

iii) it would be useful to state, explicitly, which values of chiˆ2 and 'a' are required to pass the given statistical tests to remove the need for the reader to use look-up tables. Currently it's very hard to interpret these tables.

The discussion concerning the discrepancies between model and measurement values seems to be implying that a higher resolution works better but yet the figures provided do not justify this. Indeed, the same statistical analysis should be undertaken for the finer grid as for the coarse grid so that the reader can make a valued judgement.

3.3 Chemical Composition

Rather than looking at two sites in detail and then summarising briefing other sites' data, I would outline all available data, plot all species (inc total aerosol mass) as timeseries (as well as table of stats) and then discuss en masse rather than separate



ACPD
8, S954–S960, 2008

out two sites for special attention. (Or an explicit case should be made not to take this approach.)

Measurement stations should also be named using their shorthand key for ease of comparison with earlier tables/figs).

Any comments on why nitrate is not captured well at Birkenes, esp. in comparison to Melpitz?

Fig 6, which may have addressed some of the above points, is a repeat of Fig 5.

"Not specified aerosol" is vague - which of CMAQ species are the authors referring to (and what are the initial/boundary conditions for these species and how are they treated within CMAQ)? Given that CMAQ models aerosol water as a separate entity, although the author has decided to model only dry aerosol, how can the "not specified aerosol" be partly water? (Depending upon how the author addresses this point, amendments will also be required to the Conclusions.)

What is the magnitude of the organic aerosol and EC at the sites and does this difference wrt model help account for the model's systematic underpredictions?

3.4 Aerosol Optical Depth

No mention of AOD being function of wavelength. How is equ (1) amended to take account of this?

It would be useful to plot the Aeronet measurement stations on a map to get a bearing of their positions relative to the other measurement stations eg given AOD dependence on aerosol mass and composition, are we looking at the same data in a different manner, or data at very different sites? The former would allow, for example, an evaluation of the Malm approach.

I did not follow the explanation of why Clermont Ferrand's simulated AOD is an overestimate - surely all modelled AOD data include contributions from all model layers?



ACPD

8, S954–S960, 2008



Given the importance of correct RH, it would be interesting to know whether the model is using similiar RH values as to those recorded by the measuring stations (but we appreciate this is a test of the uncoupled met model).

It is generous to say from Fig 7 that for Lille the model's 6 wks' average represents the measurements. I cannot comment for those plots not shown - if they are relevant they should be shown. Perhaps replotting with diff wrt average (for each of model & measurement) will be a better illustration?

Need to expound, or cite other work, re statement that biogenic emissions are underestimated in the model.

The text refers to Kishinev in Table 10 but it's not present - need to add to Table.

Would the author care to comment as to why the model fails to produce a similiar frequency distribution for AOD as to that measured (Fig 8)? Fig 8 should use same axes in both plots for ease of comparison.

4. Conclusions

Whilst mention was made that the model performed less badly (re PM10) in Winter in section 3.2 it was only for the German station(s?) (and not so for Switzerland & Austria) - this should be clarified in the Conclusions.

The discussion of Saharan dust should be not be introduced in the Conclusions - a mention must be made earlier (3.4). It may be useful, as future work, to use a larger (perhaps coarser) domain that includes North Africa to provide temporally variant boundary conditions for the 54 km resolution domain capturing dust events.

The Conclusions' firm assertion that the AOD variablity is "correctly produced" by the model appears to contradict Section 3.4's discussion of Fig 8. The Conclusions should be amended to account for this.

Some specifics:



ACPD

8, S954–S960, 2008

Fig 1: which resolution is this or is it a mixture of resolutions? and why has the plot been cropped (as illustrated by the x-axis and y-axis range not starting at 1)?

Section 3.4 - refers to equation 2 but presumably it means equation 1 and the reference to Table 4 is misleading.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 1457, 2008.