**Atmospheric
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Discussions**

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Interactive comment on "**A three-dimensional variational data assimilation system for multiple aerosol species with WRF/Chem and an application to PM**$_{2.5}$ *prediction" by Z. Li et al.*

Anonymous Referee #1

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General remarks:

The paper by Li et al. presents a 3DVar algorithm to assimilate aerosol properties in a WRF/Chem model. It is coupled to the MOSAIC model which can explicitly treat aerosol processes. The

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manuscript contains valuable information and it should be published in ACP. Although it would have been advantageous to include more independent observations (no assimilation of them) for evaluating the assimilation scheme, the paper makes a valuable contribution to the scientific community busy with the assimilation and forecasting of atmospheric aerosol properties.

I have three more general remarks:

In section 4 the authors explain the 3DVar scheme and address the assimilation scheme for the aerosol species. In the abstract the authors mention that the analysis increments of the total mass concentrations of 8 aerosol species are treated. However, in the assimilation scheme finally applied only five control/analysis variables are used, as four of the eight species are lumped into one variable. In sections 2,3 and 4 the authors

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give sufficient motivation why finally five and not eight control/analysis variables are used in the data assimilation. However, this lumping of four of the eight species in one analysis variable should be mentioned also in the abstract. It should also be mentioned in the discussion on p13537, lines 7ff.

In my view, sections 2 and 3 could be combined into one section. Then also the (dis)advantages of single- or two step assimilation schemes don't have to be explained two or three times.

The important information, which exact parameters are finally assimilated is described late in section 6.1. Large parts of lines 11-23 of p13533 could go into section 4.4 where the observational operators of the assimilation scheme are described. This would improve the assessment of the assimilation scheme.

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Specific comments:

P13519, lines 4-12: in the list of observations – also aerosol profile measurements could be mentioned, they provide very valuable information.

P13520, line 24: 'actively developed' – what does 'actively' mean/express?

P13520, lines 25-27: in the list of aerosol species treated – is desert dust also accounted for, or not relevant for the region of interest?

P13521, line 3: The size bins are defined by dry particle diameters – is there interaction between relative humidity in the model and the aerosols?

P13527, section 4.4: As the measurements of PM within networks represent surface concentrations – as stated also by the authors – how is this treated exactly within the assimilation scheme? Is the total mass concentration of the five analysis

variables only taken from the boundary layer levels to compute the analysis increments? Could the authors give some information on this?

P13528, line 25: The authors describe the WRF/Chem configuration wrt the vertical extension. Could they also give the upper altitude level (height or pressure)? Figure 2 gives a hint, but not exactly.

P13531, lines 15-16: '... suggest a need to use multi-species concentrations as the analysis variables'. It is not clear if lumping more species together in one variable or treatment of more individual species it is meant.

P13533, line 21:why is the limit value of PM2.5 O-F chosen to be 120 g/m3? And should it not be μg/m3?

P13533, line 2: 'analysis PM2.5' – is this

surface/boundary layer analysis PM2.5?

P13536, lines 19ff, description of figure 9: This is a bit confusing. The authors write that in case of nitrate the error reduction (also rmse) persists up to 24h. However, in Figure 9, the forecast with DA shows for 6h a more or less same rmse and afterwards a higher rmse relative to the forecast without DA (reduction of rmse only relative to the 6h step). Also, the last sentence does not really fit with what is shown in Figure 9 (in the figure, larger rmse for EC and nitrate, smaller for sulfate). The sentence on p13537, line 26/27 wrt the improvement concerning EC, OC and nitrate should also be re-visited.

Technical comments:

Abstract, line 9: 'specie' – change here and everywhere in the manusript to 'species' please

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(specie is not the singular of species – different meaning).

P13517, lines 13 and 22: ... to develop ... / ... to assimilate ...

P13517, line 17: WRF/Chem – explain the acronym please

P13520, line 3: ... total mass concentration' ... for clarity

P13520, line 5: reference name: Tombette

P13520, line 14: it's Sekiyama et al., 2010

P13522, line 10: 'other inorganic aerosol' concentrations

P13529, line 25: ECMWF – please explain the acronym (although well-known, not everybody is familiar with ECMWF)

P13531, line 27: 'Another feature worth

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mentioning is . . .'

P13534, lines 26ff, discussion of Figure 6: It seems that in Figure 6 the columns are twisted, otherwise the description of the results makes no sense. In addition, still p13534, lines 2ff it should write '. . . increases by 0.1 for EC and by 0.2 for . . .RMSEs are reduced by 10% for OC and by 50% for nitrate (however, it's 25%, not 50%)

P13535, line 8: delete 'EC'

P13537, line 11: size bins were used.

P13538, line 23: non-negative

Figures:

Figures 6/8/9: numbers, axis numbers and titles difficult to read, please enlarge.

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