**Geoscientific
Model Development
Discussions**

Interactive comment on "Towards an online-coupled chemistry-climate model: evaluation of COSMO-ART" *by* C. Knote et al.

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This paper describes the evaluation of online-coupled model COSMO-ART. Since we have some experience for some of the modeling periods discussed in this manuscript, I'd like to give my comments and suggestions below.

-Although a lot of measurements were used for model evaluation, I find the model performance is overstated in the manuscript since important processes such as aqueous chemistry and wet deposition are not yet implemented in COSMO-ART. In order to make it clear for the reader at the beginning, I suggest that authors mention the missing processes in the abstract.

-There is one more process -not mentioned in the manuscript - which could cause an

overestimation of nitrate. Lack of wet deposition might lead to overestimation of nitric acid in the gas phase and consequently overestimation of particulate nitrate.

-There is not enough information about the pathways for SOA, what precursors are treated, how the yields are, whether SOA module is still based on the original SORGAM by Schell et al., (2001).

-Page 1821, first paragraph: the statement "very good agreement " (also in the conclusions) is based on mean values over all stations (Figure 2, Table 2). In line 4, it is written : "Sometimes the diurnal variability is underestimated by simulations (not shown)". It would be more useful to show time series, diurnal variations especially for wind speed to justify that agreement is "very good".

-Figure 21, top panel: Is there any reason for not using AMS data after 23rd June?

-Figure 21, lowest panel: It seems that there is no OA for Harwell measurement data (only inorganic aerosols are seen in the lower panel). But somehow the total seems to be similar to PM2.5 data at Harwell. I think this needs to be checked.

-Page 1834, line 11-14: "in the model it is almost exclusively POA". The description of SOA formation pathways in COSMO-ART would help to understand why it is like that.

-Page 1838, Sulfate: We modelled the same period June 2006 with CAMx model which includes aqueous chemistry and wet deposition (Aksoyoglu et al., ACP, 2011). Comparison with our findings for the same period would give an idea about the contribution of cloud chemistry to sulfate.

-Page 1839, line 15: Authors state that their SOA parameterization has too low yields. It would be very useful to see the yields used in this study.

-In the section of "Organics" biogenic SOA is not mentioned at all although SOA from biogenic emissions can be very important especially in summer.

-Page 1841, lines 15-20: "We think those three processes together explain a large part

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of our overestimation of nitrate aerosols. Other models experience similar problems in simulating aerosol nitrate, for example CAMx (PMCAMx) (e.g. Aksoyoglu et al., 2011, Andreani-Aksoyoglu et al., 2007) or WRF/Chem (e.g. Li et al. 2010) once the diurnal cycle of aerosol nitrate contributions is looked, and not only the mean". These sentences give the impression that nitrate overestimation is a general problem in models. However, CAMx version used in our studies includes aqueous chemistry and wet deposition, therefore sulfate and nitrate cannot be compared to those described in this manuscript. Our experience suggest that over-and under estimation can happen due to input parameters (e.g. boundary conditions, emissions) but not due to the CAMx model itself.

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