

## ***Interactive comment on “Air quality over Europe: modeling gaseous and particulate pollutants and the effect of precursor emissions” by E. Tagaris et al.***

### **Anonymous Referee #1**

Received and published: 6 May 2013

My major concerns with the current version of this manuscript are listed below.

1. I think that the focus of this manuscript (scaling and impact of precursor emissions) should be emphasized after certain changes are made to the modeling system. In accordance with Dr. Odman's comments, I think that updating the treatment of organic aerosol in the model is necessary before even looking at particulate pollutants and PM<sub>2.5</sub>. The inclusion of the volatility basis set (VBS) scheme, which treats both primary and secondary OA as semi-volatile and chemically reactive is nowadays common practice to all new papers using regional models (that are at least published in journals such as ACP). This cannot be disregarded here. The use of the VBS will change con-

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centration levels of PM<sub>2.5</sub> but also of gaseous pollutants as the availability of oxidants will change (Zhang et al., acpd, 2012; Athanasopoulou et al., 2013; Bergström et al. 2012).

2. In relation to 1, there is no further model development in the specific modeling system used here, compared to previous efforts (application of CMAQ in the US domain) by the same author a few years ago. The WRF model is now preferred for the meteorological description as it is considered an improved version of MM5. Why the authors are still using MM5? I think that updating to WRF will be an important part of further model development which would strengthen the paper.

3. A significant limitation of the manuscript is the lack of related literature review. There are other regional models that have been applied to the European domain in a similar way. The authors should cite these as well as try to compare their results with those of others. Furthermore, there is even the same model (CMAQ v4.7) that has been recently applied in Europe (although in a smaller domain), however it is not referenced at all here. Some well known regional air quality models that have been recently applied in the European domain and are not referenced here are the CHIMERE model (Zhang et al., acpd, 2012), the EMEP (Bergström et al., 2012), the PMCAMx (Fountoukis et al. 2011), etc. Moreover, Im et al., Atmos. Environ., 2012, and Im and Kanakidou, acp, 2012 have used CMAQ in Europe. The authors should reference these papers as well as comment on differences and similarities between their version of CMAQ and the above CMAQ application. Most of the above model applications have used the MEGAN model for the calculation of biogenic emissions. The authors are using a different one. A paragraph is needed explaining in what basis this choice was made and what are the differences (in numbers) in the calculated biogenic emissions in Europe. I am assuming that there are large differences given the associated uncertainties.

Other comments.

P6683: L9: The abstract should be self-explanatory. This sentence doesn't make much

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sense.

L15: Which model ?

L23: Not clear. Re-phrase.

L28: Not clear. Re-phrase.

P6694: L15: "...the effect of precursor emissions." On what?

L16-19: But you don't make a big effort towards comparing these results with others.

P6686: L8-10: What are the default values?

L11: what is the prevailing direction?

L12: 10 days? Why such a large spin up time is needed? Isn't 2-3 days enough?

L13-14: What are the new pathways? Please describe.

L24: By how much was the overestimation.

P6687, section 2.2: What kind of data are the station data? Daily ?

What are the future goals of the authors in terms of using this model in Europe?

P6688, section 2.3: L3: True, but what kind of feedback does this paper give back to emission people?

Is the PM2.5 observed, dry or wet? What about the predicted?

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 6681, 2013.