

***Interactive comment on* “Summertime impacts of Eastern Mediterranean megacity emissions on air quality” by U. Im and M. Kanakidou**

Anonymous Referee #4

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One of the questions raised by current research on the impact of Megacities on chemical composition is, whether the fact that emissions are strongly concentrated on a small domain changes the fate of primary pollutants and the build-up of secondary pollutants. Thus the question of non-linearity of photo-chemical processes in the atmosphere is posed. The paper by Im and Kanakidou asks this question by performing model calculations of a decentralization scenario, in which megacity emissions for Istanbul and Athens are distributed over larger areas. Other scenarios, in which these emissions, or emissions in the whole Eastern Mediterranean modeling domain are switched off, or in which biogenic emissions are also altered, are also simulated with a mesoscale modeling system. This paper could give interesting answers to the questions outlined above, but it needs substantial improvement in several respects. 1) The local air quality impact can a priori not be determined with a 30 km resolution model. If authors do

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so, then they need to carefully argue, that they are correct, i.e. that their central grid cell for a Megacity represents a kind of average air quality. A comparison with higher resolution runs would be helpful, even if these runs are not used for further analysis of long range transport. The expression “Urban core” for a 30 km grid cell is misleading. Not only primary pollutants are concerned, but also the titration effect of NO on ozone.

2) Model build-up performance should be briefly summarized and not only cited. For example are primary OA values correctly modeled taking into account volatility of POA emissions, is secondary OA build-up correctly modeled. The authors are expert in this field!

3) Major differences between the impact of Athens and Istanbul should be much better worked out. Apparently, this has to do with the chemical regime, the absolute magnitude of emissions. Also the transport regime seems to be different, plumes often seem to be more pronounced for the case of Athens, is this due also to a more stable transport regime ?

4) The time period for the analysis is very short (2 weeks), no information about meteorological conditions is given. Is this period meant to represent climatological or at least typical conditions in terms of air transport regimes and chemical composition? The computational costs for the model runs with low resolution should be not high even for 8 scenarios, so longer periods could easily be simulated.

5) A specific question is on the VOC/NO_x ratio for Athens, which decreases, when Athens emissions are switched off. This is astonishing and should be better explained. One would expect that the rural background with large BVOC emissions and longer lifetimes of VOC's as compared to NO_x favor a larger background NMVOC/NO_x ratio than the urban one.

6) How much do emissions from Istanbul and Athens contribute to average pollutant levels as compared to their contribution to total emissions in the domain, i.e. does their concentration on a small spatial scale change this contribution ?

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7) The authors should better present the spatial changes in pollutants induced by the decentralization scenario, for example as a function of distance from the megacity (and keeping in mind the transport regime). There are some obvious effects, like impact on primary pollutants and ozone titration. But the interesting effects are those dealing with the non-linearity in atmospheric chemistry. For example, how much more (or less) secondary species are formed from more diluted emissions, and how can this be explained with current theory (or the one included in the model). This is the sense in my view of running this highly idealized decentralization scenario.

These questions would need to be answered before acceptance of the paper. All in all the study could lead to an interesting paper, but a more “careful” analysis and presentation would be needed.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26657, 2011.

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