

## ***Interactive comment on “European summer surface ozone 1990–2100” by J. Langner et al.***

### **Anonymous Referee #1**

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#### General comments:

The manuscript presents model calculations of the influence of future climate change vs the influence due to changes in emission and background concentrations on surface ozone in Europe. The paper is well written and presents results and findings in a clear and sound way. The main message (as I see it) is that the projected RCP4.5 emissions scenario more than balances the increase in ozone expected due to climate change in South Europe. The manuscript is at a level almost ready for publication.

Regarding the model evaluation: A bit more text on the observed ozone data used for model evaluation could be nice. How did the authors take into account that the amount of measurement data changes substantially over the years (1990–2009)? Which year(s) do the “number of stations” in Table 1 refer to? Presumably the number of sites south of 50N is of a much shorter monitoring history than north of 50N?

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The paper gives strong indications that future reductions in precursor emissions (provided the RCP4.5 is relevant) will have a much stronger impact on ozone than climate change. A common problem is, however, that apparently all models today fail to reproduce the European ozone trends (or lack of so) in the past 20 years, and the reason for this discrepancy is unclear. How do the model in this paper agree with the observed development in Europe in the period 1990-2009? I understand that this could be a topic for a paper by itself, but even so a short discussion on this topic could be interesting in a model evaluation perspective (i.e. “to what extent can we actually have confidence in modelled ozone trends?”)

Regarding the modelled trends in ozone:

The authors find that the emission reductions are most effective in reducing the ozone peak values. This is interesting in a political perspective as most of the air quality target values (in EEA etc) are linked to peak values. The authors could consider to show the change in one of these target values, e.g. the number of days exceeding the 8-h value of 120  $\mu\text{g}/\text{m}^3$ .

Specific comments:

Were changes in soil moisture/drought taken into account in the modelling? What about wild fires? Any change in the VOC speciation of the emissions with time (changes in emission profiles)?

The definition of the “variability” in Table 2 is somewhat unclear/imprecise.

Typo at p. 7707 l. 3: The name “Katragku” should be “Katragkou”

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7705, 2012.

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