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

**J. Langner et al.**

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We are very grateful for the valuable comments and questions posed by Anonymous Referee #1 that we address below.

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 eval-

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uation: 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?

Response - In the evaluation we made an attempt to account for the fact that the emissions of ozone precursors are decreasing over time in the period 1990-2009 by comparing observations from a specific year with the same year in the model simulation. The number of stations given in Table 1 refer to the total number of stations used in each part of the domain. The requirement for including a station apart from the elevation difference criteria was a 75% data capture for each year and at least five years of data. 75% of the stations had more than 9 years of data in both north and south regions.

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?”).

Response - The current model setup is not well suited to analyze our ability to correctly model ozone trends in 1990-2009 since the meteorological data used to drive the model simulation is taken from climate model output. Trends in meteorological variables in the simulation are therefore not necessarily in phase with observed changes. Anyway the simulations indicate decreasing mean ozone concentrations in the period 1990-2009 when accounting for changing emissions (see Fig 1. in the paper). Ob-

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served trends for 1990-2009 in the northern part of the model domain are weak and slightly negative for daily max for the stations used in the evaluation in agreement with the model results. In the southern part of the domain the observed trend is more variable and uncertain, partly due to shorter observational records, while the modeled trend is decreasing.

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 ug/m<sup>3</sup>.

Response - This is a relevant suggestion. We will consider adding plots of days>120ug/m in a revised version of the paper.

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)?

Response - Soil moisture is taken into account in the calculation of dry deposition of gases including ozone and this is stated in section 2.2. Wild fire emissions were not included in the simulation and the VOC speciation was constant with time.

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

Response - We realize that our definition of variability differs from common definitions (e.g. given by the sample’s variance or standard deviation). We could have used the term “spread” which may be more accurate, but we regard our definition unambiguously explained in the table legend.

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

Response - This will be corrected in a revised manuscript.

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