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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Low modeled ozone production suggests underestimation of precursor emissions (especially NO_x) in Europe" *by* Emmanouil Oikonomakis et al.

Anonymous Referee #1

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General comments and overall quality

This paper is mainly focussed on the agreement between modelled and observed surface ozone in Europe in the year 2010 using various sensitivity model runs to identify reasons for mismatches between the modelled and observed levels of ozone. Other species like CO, SO2 and PM2.5 are also included but only to a very small extent. The work is presented in a clear and sound scientific way with no major errors, and overall this is as a robust and well-performed study with interesting findings that certainly deserves to be published. Some questions and comments are given in the following.

A few general comments: Neither the title, abstract or conclusions mention anything

about the additional species (SO2, CO and PM2.5) being included in the work. Furthermore, these species constitues a very small part of the paper and apparently with a fairly small implication for surface ozone which is the main focus of the paper. Thus, one could consider to take out these species completely. This is left to the authors (or the editor) to decide.

A main issue when doing comparisons between observed and modelled ozone is the question of how to treat vertical concentation gradients near the surface. During the summer season the effective dry deposition and uptake in vegetation will lead to signicifant gradients in ozone near the ground. Since the air intake of the ozone monitors mostly are at around 2 m the gas concentrations at that altitude in some way need to be related to the mean concentations in the model's lowest layer, in this case around 20 m. Has this issue been considered and if not – how important could this effect be?

Underestimation of the high peak values is commonly seen in almost every model study. The authors should include some discussion on this general feature with references to a number of relevant modelling papers. Could it be that this artefact is reflecting the unavoidable smoothing (in emissions, meteorology etc) that all CTM relies on?

Specific comments

P3 L8 (and Fig 1). The definition of sub-regions and in particular sub-region 3 seems a bit odd. If the point is to divide Europe into areas with homogenous characteristics with respect to climate and air pollution statistics, then region 3 doesnt seem a very natural choice since it merges clean background sites (West coast of Ireland) with central European sites (e.g. Czech Republic). Apart from perhaps the most northern part, a latitudinally based definition of sub-regions is not very meaningful for Europe. Thus, it would make more sense to split region 3 into two regions or to create another set of sub-regions better reflecting climatological patterns. (See e.g. the PRUDENCE regions: http://ensemblesrt3.dmi.dk/quicklook/regions.html)

P3 L9. With the model top at 460 hPa, the domain seems shallow compared to the model setup that is normally used for regular modelling in Europe. The authors should inlude some sentences justifying this choice of vertical range.

P7 L4. For studying ozone peak values the time period 11-16 UT is selected "when the ozone production and mixing ratios often reach their maximum", the author states. This seems as a bit narrow and early to capture the highest ozone peak values. On average, for the entire 6-months summer season, 11-16 UT may be the peak period in some regions (see Fig 3). However, during high ozone episodes the peak values will often occur later in the day, and a period 12-18 UT would seem a more natural choice or even 14-20 UT.

P7 L14. Some details (geographical location and alitude) of the 8 stations with data on both T and O3 should be given, e.g. in a map.

P10 L17 (and Fig 4). How representative are the mean diurnal cycle of NO2 for this very large region? The header states that only 8 sites are included, and presumably (with some knowledge of the Airbase data) most of these sites are from the Northern UK?

Technical corrections

P2 L8-9. Consider rewriting this sentence: "Apart from the ozone precursor emissions, the other key driver of the surface ozone concentrations, as well as its chemistry, is the meteorology; from local to global scale". To state that meteorology is a "key driver" of surface ozone concentrations is somewhat meaningless without a few words explaing how met could affect ozone.

P2 L13-14. This sentence is imprecise. Although T is peaking in the afternoon, incoming solar radiation is not. Rewrite.

P2 L18. are -> is

P2 L19. This phrase should be reformulated and clearified: "The evaluation of modeled

ozone production from the ozone concentrations may not be a safe option."

P5 L4. This phrase should be reformulated: ". . . large discrepancies have been obvious . . ."

P7 L14: Rephrase this: ". . . surface stations which contain both temperature and ozone . . .". (The station doesn't "contain" temperature and ozone.

P8 L23. The text and the caption of Table 5 should explain for which time period (summer season?) these statistics were based on and for what type of data (hourly, afternoon means or something else?).

P10 L13. Typo: "overestimation" should be changed to "underestimation"

P10 L17. Rewrite. The word "now" doesn't seem appropriate. Change e.g. to "in this region" or something else.

Fig 5 and Fig 6. The time period (11-16 UT?) which the afternoon average is based on should be given in the Figure captions.

P12 L15. Rephrase this: ". . . seems to be the most effective scenario . . ." (The point is presumingly that the model scenario with increased emissions of both NOx and VOC is the scenario that gives the best fit with the observations).

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