

Interactive comment on “Changes of daily surface ozone maxima in Switzerland in all seasons from 1992 to 2002 and discussion of summer 2003” by C. Ordóñez et al.

Anonymous Referee #1

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General comments

The paper gives a well-documented and relatively clear presentation of a statistical trend analysis of surface ozone measured in Switzerland during a decade. The issue of trends in tropospheric ozone is presently a topic of large debate and this study gives interesting contributions to the discussion and therefore deserves to be published.

The presentation of the methods used, the results and the conclusions are mainly done in a fairly clear way. I wonder, however, if the manuscript could be somewhat shortened and the discussion made in a slightly more condensed way. The present

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version is somewhat voluminous and the paper could improve by a general clarification of the discussion.

The conclusion, that large precursor emission reduction has not led to significant changes in the ozone daily maximum concentrations is striking. The question is, however, how good indicator the median of daily (seasonal) ozone maximum really is for detecting trends induced by emission changes. The published surface ozone trends in Europe have to my knowledge either identified an increase in the mean background concentrations (clean air masses) or a decrease in the high percentiles of ozone. The median daily maximum ozone used in the present study may reflect these effects combined and thus mask the results. It would be very interesting to know if the conclusions would be the same for e.g. the median of the 90 percentiles or a similar indicator. My guess would be that the median of daily max would be much less indicative of emission induced trends than the peak values (or high percentiles). If the authors could elaborate on this it would be of great interest.

My second concern is the monitoring sites. Table 1 shows that the majority of the sites are fairly close to the urban emission areas. Even the least NO_x-polluted sites have annual median NO_x concentrations of 5-10 ppb, which tells that the distance to the sources are rather short. The authors do indeed mention the possible influence of titration by NO. I wonder, however, what the expected change in ozone at these sites would be. Could it be that the stations are located just in the transition regime where the NO_x/ozone relationship changes from a destruction regime to a production regime, and that this location is the reason why no changes in ozone are seen?

Technical corrections

- p. 7050, line 21: Could replace the word “works” by another phrase.
- p. 7050, line 22: Typo. “ad” should be “and”
- p. 7051, lines 1-4: Use either the abbreviation “Sect.” or “section” not both.

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p. 7060, lines1-5: Clarify if it's warm fronts or cold fronts that are discussed.

p. 7065, lines1-5: The mentioning of a possible future paper is not relevant unless this is an accompanying paper submitted together with the present one.

p. 7065, line 28: "This made it possible to.."?

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7047, 2004.

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