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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Have primary emission reduction measures reduced ozone across Europe? An analysis of European rural background ozone trends 1996–2005" *by* R. C. Wilson et al.

Anonymous Referee #1

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This paper describes an analysis of surface ozone observations from a large number of sites across Europe over a decade. It identifies a significant positive trend in ozone at many locations that appears in both the mean and the tails of the ozone distribution. It demonstrates that individual years have a significant impact on the calculated trends when calculated over this short timescale. Comparison with results of a CTM shows similar trends, although there are differences in some aspects of the distribution. The paper concludes that emission reductions do not have a substantial effect on the observed trends.

The standardized approach and fixed time periods for the analysis are a major strength of the study, along with the analysis of sensitivity to individual years. However, there are also major weaknesses, both in assessment of the trends and their uncertainties, and in the discussion. The paper describes results but is short on interpretation, and therefore it delivers significantly less insight than it should. The study is not able to clearly answer the question posed in the title (on whether emission reductions reduce ozone), although further exploitation of the model results described might allow this. I therefore believe that substantial revision is required before the paper is ready for publication in ACP, and I outline a number of issues and recommendations below.

General Comments

The most valuable aspects of this study are self-consistency in the analysis approaches used and the fixed time period, although it is clear that a 10 yr period is insufficient for robust results. The analysis excluding individual years provides valuable new insight into the sensitivity of the trends, and is probably the most novel and interesting part of the study. However, it also demonstrates that meteorological events in a single year (e.g., 2003) can be sufficient to change the sign of the trend, and hence that the uncertainty on the mean decadal trends are greatly underestimated. It is important to bring this out in the discussion, and suggest ways in which this might be addressed. Does extending the analysis to 15 or 20 years at sites where data is available make substantial changes to the trend?

The 2-sigma error term quoted here does not provide a true reflection of the uncertainty in the trend. This is clearly illustrated by the variability in values when a single year is omitted (shown in Table 4). The mean trend from these ten studies is 0.16 +/- 0.11 ppb/yr (1 sigma error), and this appears a much more robust assessment of the expected uncertainty. While I appreciate the statistical methods used here, it is not clear that the assumptions inherent in these approaches are defensible. If the authors intended to retain this approach, stronger justification is required, along with an assessment of errors not considered (site distribution, short 10-yr timescale, interannual variation) and additional discussion to aid the reader in interpreting them.

Comparison with the model is greatly underexploited. The model trends are compared side by side with the measurements, but little attempt is made to explain or interpret them. Did the chemical boundary conditions change from year to year? If so, then what would the trends have been if they had been kept constant? The model study should allow the cause of the trends to be clearly diagnosed. If emissions were held constant, how much larger would the trends have been? Further analysis of this type is required to answer the question expressed in the title of the paper. There is much valuable additional detail available from this part of the study that is not included in the discussion here!

Sections 3.2, 4 and 5 describe the results of the study, but are short on detailed analysis or interpretation. How do we expect the seasonal behavior to change if changes are due to background ozone or to local emissions? Do we see these features in the current analysis? How does this compare with what other studies have found? What further measurements or analysis do we need in order to reduce the uncertainty or to permit better attribution of the cause of the observed changes?

The last sentence of the abstract is not clearly demonstrated in the paper. It is clear that the observed ozone trend does not match the perceived reduction in precursor emissions, but this does not indicate that the emission reduction had no substantial effect, only that it cannot be seen in the period and locations available. This is presumably because the effects are relatively small compared with the masking effects of background ozone changes, shifts in source patterns, and meteorological variability. This sentence needs to be rephrased to state that the effects of emission reductions are not obvious (or observable?) at most locations, but that their effects have not been quantified. Further analysis of the model results might have allowed these effects to have been quantified.

Specific comments

p.18437, l.15: "Within ozone trend work" - please rephrase (or remove).

p.18437, l.24-27: Note that this focus on marine inflow is somewhat simplistic, both because of the effects of recirculation of continental air over the oceans associated with synoptic-scale systems, and the sensitivity to air mass history. Some controversy remains here.

p.18438, l.3-6: some uncertainty values needed on the stated trends.

p.18440, l.16: given the distribution of sites shown in Fig 2, which are heavily weighted towards Central Europe, some comment on their suitability for assessing European ozone is required. Western, southern and eastern Europe are relatively poorly covered. This point is not mentioned until the conclusions - it needs addressing much earlier in the text.

p.18441, l.14: the meaning of this introductory sentence isn't clear - do you mean that different methods are suitable for different purposes? Please rephrase and/or explain this.

p.18442, l.23-25: two explanations for changes in the 5th percentile are outlined here, but no attempt is made to separate them in the analysis. In the remainder of the paper, this low tail in the distribution is assumed to represent the background, but it is possible that NOx reductions play a part at many sites, particularly in the winter. Why is this discounted? Further discussion of this would be highly relevant in Section 3.2.

p.18444, l.11: section 3.1.1 should be relabeled 3.2, and section 3.2 should be 3.3.

p.18444, l.25: climatological changes are usually defined over 30 yr periods to avoid significant influence from interannual variability, so it should come as no surprise that 10 years is too short. The excluded-years approach is good, and should be combined with the full 10-year analysis to provide a more robust assessment of the uncertainty in the trend.

p.18445, l.18-20: Given current constraints on the global economy, this continuation is not inevitable. This would be a good place to emphasize the importance of continuing long-term measurements, as without them there will not be sufficient data to be confident about trends.

p.18445, l.22: At the start of this section it would be helpful to explain why it is important to look at seasonal trends.

p.18446, l.13-16: Not all air at these remote sites comes from the ocean, and at the 95th percentile it is likely that local or regional sources make a significant contribution.

p.18446, l.22-29: The list of sites is not useful here unless there is some underlying thread that links them together. The discussion in this section is principally descriptive, and could be greatly simplified and/or replaced with more interpretation of what the observed seasonal difference in trends might indicate.

p.18449, l.5: Additional interpretation is needed here. It is clear from Fig 9 that sites with a positive NOx emission trend have a higher ozone trend than those with a negative NOx trend. This suggests that local emissions do play a role, and it allows this impact to be removed (by looking at the mean ozone trend where the NOx emission trend is zero).

p.18449, l.9: also transport of ozone, not just its precursors.

p.18449, l.15: Please explain "fixed climatologies". What is the temporal and spatial variation in these boundary conditions? Most importantly, did they vary from year to year? This has important implications for the analysis, given that a positive trend is also seen in the model results.

Table 2 adds little to the paper and should be moved to the Supplement.

Fig 13: Which trends are observed and which are from the model? What do the different panels show (presumably mean, 5th, 95th percentiles, but in what order)? Please add appropriate labels to the figure.

Typos:

Short title: reduction -> reductions

p.18443, l.11 (end of line): trends -> trend

p.18444, l.12: effected -> affected

p.18448, l.6-7: meaning unclear, please rephrase

p.18449, l.17: models -> model

p.18452, l.10: remove one "each"

p.18452, l.22: includes -> included

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