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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 #2

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This manuscript presents an extensive evaluation of trends in O3 at a large number of European sites and a comparison to a chemical model. There is a lot in this paper. Too much actually. I recommend that the statistical analysis of the observations and the modeling be separated into two papers.

The analysis is important so that we can understand the impacts of policy choices (eg emission reductions). However I have some specific concerns with the statistical methodology. Although the methods are not very well described, it appears that the authors have done extensive smoothing of the data, which will reduce the noise and

give statistically significant trends where none were present in the original data. I would argue that taking out the seasonal cycle is an appropriate tool, but smoothing out noise and other variations is not appropriate. In many respects, calculating seasonal O3 trends is a more useful and relevant metric, rather than an annual trend, even if the data have been de-seasonalized. In addition, the authors have chosen to use a P-value of 0.1, which will also exacerbate the number of significant trends observed. Keep in mind that at least 10% of the "significant trends" are in fact due to random deviations.

The finding of "No significant trend" is still an interesting policy result. If in fact the emission reductions have not led to reductions in O3 then this still needs a better reconciliation with model forecasts that suggest O3 "should have" declined.

In addition, I am not convinced that calculating a single trend for all Europe is meaningful when there are so many regional variations. The model comparison with this single European value is not useful.

In summary, the authors must do a better job of explaining exactly how these trends were calculated, eliminate artificial trends due to smoothing, and use a more accepted P vale of 0.05. All results should be updated to reflect the true statistically significant trends.

Detailed comments:

Pg 18436, lines 8-10: Rewording needed.

18437, line 18: There are a number of important references that should be mentioned including the HTAP reports and the US NAS/NRC report (2009) on background air quality.

18438, Line 15: where should be were. 18439, lines 3=5: Don't understand this. 18441, lines 1-11: The authors mentioned three different methods (Lowess, Mann-Kendall, Sen-Theill) but they do not describe in detail how they use the three methods.

This is a critical omission. It is not clear how they have done the spatial averaging presented in Table 1. Are the trends calculated individually for each site within each country then averaged, or are the O3 values averaged first?

18445, line 7: The huge Siberian fires were probably also important in 2003.

The modeling section really needs to be in a separate paper. There are a large amount of details about the model that are not included and the comparison with observations are handled in just a few summary figures.

Table 1: Need to explain how multiple sites used to calculate a single trend in each country.

Figure 6: Seems to duplicate information in Table 4.

Figure 12: Using this single metric (all Euro trend) to compare with the model is a massive simplification. The model discussion and evaluation need to be expanded greatly in a separate paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 18433, 2011.