Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-338-RC2, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Reanalysis of and attribution to near-surface ozone concentrations in Sweden during 1990–2013" *by* Camilla Andersson et al.

Anonymous Referee #2

Received and published: 7 August 2017

This study is a nice example of combining models with observations, and making use of the model to better understand the causes of observed trends. In general the paper is nicely written, and will deserve publication in ACP after attention to the following points.

I have two significant reservations:

1. As noted on page 11, the length scale used for the 2dvar is 1000 km. The authors note that this is large, but claim that this is justified by the sparse network and the weak gradients in Sweden. However, Fig. 7 makes clear that the gradients can be rather large, especially in Southern Sweden. I would like to see more discussion of

Printer-friendly version

Discussion paper

this problem, ideally with results from some sensitivity runs to help demonstrate if this really is a serious issue or not.

2. Much of the discussion around annual mean O3 values results from the problem of nocturnal ozone depletion, which is said to be more important in southern Sweden. As nocturnal O3 itself is quite irrelevant for most health and vegetation metrics, why wasn't the analysis focused on some ozone-indicator that actually reflects these problems? This could be daytime ozone, M7, M12, or the daily 8-h values mentioned in Table 3.

Page-by-page comments:

Page 1, Abstract

l16 - ... 'performance over' rather than 'performance than'

Page 2

l9-11 - use more recent refs.

l14 - define NOx as NO + NO2.

l21-22 - the paper of Fiore et al (2011) provides a much more recent example of this PAN effect.

l31 - here the HTAP results presented in Fiore et al (2009) would also be relevant.

Page 3

l16 - use more recent refs

Page 4

l13 - move technique before ()

l30 - explain or provide references for 'databases EMEP and Airbase'

Page 7

l22-23 - states that MATCH only calculates chemistry for the lowest 5 km of the model domain. Is this a sufficient depth, when looking at the impact of tropospheric boundary conditions?

I was puzzled as to why the boundary conditions are given as mass units. Usually the volumetric mixing ratio is used as this is the more conserved quantity, and independent of pressure and temperature. Why this choice?

Page 8

l18 - states that the model uses 22km grid-spacing, but on page 9 the emissions are interpolated to a 44km grid. Which is correct?

l18 - states that the model uses 22km grid-spacing, but on page 9 the emissions are interpolated to a 44km grid. Which is correct?

l26 - maybe add 'see also Andersson et al, 2007' here as a ref also, since it isn't obvious from the text where the time-development comes from. Further confusion arises on page 23, when it is stated that the trends are taken from Engardt et al., 2017. Please clarify which statement is correct?

Page 9

l25-26 - states that no trend is assumed in the intra-annual variation in emissions, but such a trend is likely to exist. There have been quite large changes in the sources and fuel-mix over this period. Will this matter? I think you should also mention that the spatial distribution of emissions is also held constant, which is possibly a bigger source of uncertainty. (Which year was used for the spatial distribution?)

Page 10. Are these sites all part of EMEP? If not, are the data-quality criteria equivalent to those of EMEP sites?

Page 12

What is 'full-domain'? Is all of Europe covered? Does 'Eur' include Russia?

Page 12 and onwards The 2-letter code 'Se' is a little confusing, neither English nor an accepted abbreviation. The UN code is 'SE', so why not use that directly?

Page 13

l1 - mention that numerics can also cause non-linearity in CTMs.

Page 15

l26 - this says Fig. 5 gives 'annual' O3, but Fig.5 mentions just 'percentiles'. Which percentiles? I couldn't figure out what this Figure was showing.

Page 17

l14 - why say 'possibly caused by'? You have the data, so you can say exactly what caused this.

Page 18

l21-22. This explanation of Fig.8 would have been better presented before it is first referenced

Page 19

l14-15. This Figure reminds strongly of that presented by Jenkin (2008), so it would be good to reference that paper.

Page 20

l22-26. The big change in sign for 'meteo' between the 98th and 100th percentile deserves some comment.

Page 22

l23-26. This sounds like a political statement of the authors views. I agree that NOx control is essential for many reasons, but cite scientific papers to support your state-

ment.

Page 25

l21-22. This statement is unclear. Which earlier studies?

Figures

———-

Generally, the figure quality is quite poor and should be improved. (Some of the figures look like screen-dumps of excel plots, and the various Sweden maps (e.g. S10-S11) have awful color schemes.)

Fig. 2. The C5H8 emissions are so close to zero here that the plot doesn't show anything except that the emissions are very small. These could either be presented on a separate plot, or just described in the text.

Are C5H8 emissions really so small by the way? I have seen larger estimates for Europe.

Fig. 3. Given the frequent discussion of the topographic location of these sites, I think a Table with altitude would also help.

Fig. 4. I found the color choice unusual. Usually one uses red to indicate a warning, e.g. that data-quality is poor. Here red is used to indicate good data-quality,

Fig. 5. As noted above, I don't know what 'ozone percentiles' means if one doesn't specify which percentile. The blue and green colors here can also be hard to distinguish.

Fig. 7. Poor quality.

Fig. 8. Increase the font-size for the percentile labels - they are really hard to see.

Fig. 9. Improve quality. I really liked the content of this Figure, and also Fig. 10, but they both look like screen dumps.

References

Jenkin, ME, Trends in ozone concentration distributions in the UK since 1990: Local, regional and global influences, Atmos. Environ., 42, 5434-5445, 2008

Fiore, AM., Levy II, H. & Jaffe, D., A. North American isoprene influence on intercontinental ozone pollution, Atmos. Chem. Physics, 11, 1697-1710, 2011

Fiore, A., Dentener, F., Wild, O., et al., A., Multi-model estimates of intercontinental source-receptor relationships for ozone pollution, J. Geophys. Res., 114, 2009

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-338, 2017.

Printer-friendly version

Discussion paper