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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 #1

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This is an interesting and insightful read and well-written paper that creates two different model re-analyses datasets for ozone, and presents results for annual-mean and annual 1-hour maxima O3 concentrations and trends focussing on three regions in Sweden. Results are presented both seasonally (regional average) and spatially. Trends for different percentiles across the whole distribution as well as vegetation and health metrics are also investigated with interesting differences between North compared to Central and South regions in Sweden. The authors perform sensitivity simulations to attribute these trends and highlight the role of emissions, the O3 hemispheric background and meteorology to both monthly mean O3 and across different parts of the O3 distribution. It does highlight that care is needed when including surface obser-

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vations, as these can be influenced by local processes that cannot be captured by the underlying model such as night-time inversions. Overall, there are good insights, but a few clarifications in places would be beneficial.

Major points: 1. The attribution results for the “bound” and “meteo” simulations. The text in this section needs to be clarified, and it would be highly beneficial to establish the sensitivity of the results to the methodology/underlying assumptions. In section 2.5 the method for attributing trends to meteorology and other factors is given as: “The respective contributions to the trend are formed by subtracting the MFG with the corresponding sensitivity simulation” . How exactly how the contributions are calculated i.e is the difference in O3 between the MFG and the sensitivity simulations first calculated to produce a ΔO3 and then trend for ΔO3 or the residual O3 then calculated? Is this trend in Δ O3 assumed to be the contribution to the O3 trend as plotted in Figs 9 and 10? Please be explicit about this calculation in the text.

A key question is how does this methodology and assumptions made influence the results? in particular:

1) In particular how sensitive are the results in all sensitivity experiments to the choice of the year 2011?

2) For the “bound” simulations, is the year for constant boundary conditions not important, given the assumption of a constant background of O3 from 2000 onwards? The reader also needs to understand further about the O3 boundary conditions: the source of these boundary conditions and why they are assumed constant after 2000. Would there be a much larger contribution to the O3 trend if the boundary conditions varied after 2000?

3) The authors state that the impact of meteorology is difficult to interpret and note meteorology variations cause a positive trend on O3. This does seems rather unintuitive- why is there a positive trend due to meteorology, is this because of the methodology, rather than a robust finding? Varying meteorology is usually noted as the cause of diffi-

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culties in O3 trend detection (e.g Colette et al. 2016; Lefohn et al. 2017). Hence there may possibly be a larger O3 trend in a simulation with constant compared to varying meteorology, leaving a residual positive trend? Alternatively, there could be a trend in a given meteorological variable over this period that would cause a trend in O3. It would be highly beneficial to investigate the above points to establish why this trend is positive, and as noted in 1) the sensitivity of this result to the constant meteorological year selected should be assessed for any robust statement to be made.

Colette A, Aas W, Banin L, Braban CF, Ferm, M, et al. 2016. Air pollution trends in the EMEP region between 1990 and 2012. Joint Report of the EMEP Task Force on Measurements and Modelling (TFMM), Chemical Co-ordinating Centre (CCC), Meteorological Synthesizing Centre-East (MSC-E), Meteorological Synthesizing Centre-West (MSC-W). EMEP: TFMM/CCC/MSC-E/MSC-W Trend Report (01/2016)

Lofehn et al (2017) – reference is in the text already.

2. The regional-average trends. How sensitive are these trend results to when the averaging is performed in the calculation, especially in the case of calculating trends in O3 percentile ranges? In this study, regional averaging is done after calculating percentiles at each grid box and then a regional trend is calculated, but trends could be calculated for each grid box first and then averages calculated subsequently or the data could be pooled.

3. Spring and summer peaks. A change in when peak O3 occurs throughout the 24-year period is commented on in several places in the text. However, as noted in Fig S6 there is large inter- interannual variability, such that it is hard to make any robust conclusions about shifts in maxima and dominance of spring vs. summer-time peaks over the course of the trend period. Is there any further evidence to illustrate this point?

4. The data assimilation process. In Figure 1 it seem that the match model simulation feeds into data assimilation, whilst I thought it would be a combination that produces a new simulation. Data assimilation is a process that combines a “background field”

or “first guess” with observations to produces new physically consistent model fields. This is usually an iterative process that occurs as the model runs forward. However, the figure makes this appears as a post –processing correction, although section 2.4 discusses a “first guess”. If the process is iterative, the figure does not capture this flow and should be revised for clarity. If this is a post-processing effort the text should be revised to state this.

5. Figure 5 discuses trends in O3 percentile levels. Which percentile levels?

Minor or technical points:

Page 1, Line 13: As above. The second sentence is confusing. I assume the observations are assimilated into the CTM before performing the model simulations.

Page 1, line 20: Please clarify why including all observations leads to artificial trends and why using only time consistent measurements avoid this? Or else remove the text on artificial trends from the abstract for simplicity. See also below. This text appears in a number of places in the manuscript without clarification.

Page 1, line 20: add “Distribution of the” before “surface O3”

Page 1, line 26: change “processes” to something like “factors”.

Page 2, line 11: change the IPCC (2013) reference to reference the specific IPCC chapter.

Page2, line 17, is the clause “the right weather conditions” needed? If so why? Also noted below.

Page 4, line 8; Page 5 line 13, page 6 line 12, page 7 line 30, page 10 line 30- in all these places it is highlighted that artificial trends can be introduced, this seems an important and challenging point, can this text be expanded upon so the reader understand why an artificial trend could appear. The text on page 7 is expanded but still not clear. The text on page 10 seems useful earlier. Some of the repetition could

be removed.

Page 18, line 13, as noted above there is large interannual variability that it is hard to say anything about shifts in maxima and dominance of spring vs. summer-time peaks.

Page 8, line 25, As comment 1. above. Please explain the basis of the scaling used and depicted in fig 2a. i.e. what observed changes? Why has this been assumed to be constant from year 2007 onwards? The basis of the seasonal cycle (Fig S1) should also be given.

Page 9, line 10: it would be useful for the reader to provide some insights into the impacts of using a higher resolution emissions data at 1km by 1km over Sweden as compared to EMEP 50 km by 50km when the data are subsequently interpolated to 44km resolution.

Page 9, line 20, The text on the Swedish contribution of emissions to the domain would benefit from a map of the domain, otherwise is this text needed as the methods are already long?

Page 10, line 4: Do these measurement sites have a station classification in EMEP such as “rural”, if so please add, so the reader can see that these represent the regional background.

Page 16, line 20 and Figure 6: Please explain how the regional average trends were calculated. Is the regional average calculated first then the trend?

Page 17, line 3: Fig7a shows only two colours so the highest values in the northerly mountains are hard to discern. Could the scale be improved?

Page 17, line 19: Although there are some similarities in spatial patterns the colour scales suggest comparable magnitudes in the south and westernmost part of the region shown for the annual maximum 1 hr mean, whilst the south is the area with highest values for the annual mean metric.

Page 17, line 30 should “in the North” be added after March monthly mean?

Page 19, line 20, please explain what weather states favourable for O3 formation means and why relevant for NOx-VOC regimes for O3 production?

Page 20, line 9, there is also greater photolysis of NO2 to form O3 which is why many locations in the northern hemisphere have a peak in Boreal summer. So greater production as well as destruction.

Page 22, line 18: SOMO35 is not primarily used an indicator of long-term health effects. It is used as a metric compatible with short-term exposure and a threshold for adverse effects to occur- see HRAPIE report by WHO (2013).

WHO 2013b. Health risks of air pollution in Europe – HRAPIE project: New emerging risks to health from air pollution – results from the survey of experts. http://www.euro.who.int/__data/assets/pdf_file/0006/238956/Health-risks-of-air-pollution-in-Europe-HRAPIE-project,-Recommendations-for-concentrationresponse-functions-for-costbenefit-analysis-of-particulate-matter,-ozone-and-nitrogen-dioxide.pdf

Page 23, line 26: data assimilation only reduces the impacts of boundary conditions at the surface.

Page 24, line 11: although a number of studies do find that emissions changes are larger than climate change the time period of the climate change and the metric being analysed is important. Whilst this may be the case for annual or summertime means for higher frequency metrics such as percentiles used here this has not been well established.

Page 25, line 8, SOMO35 in the north part of Sweden increase, hence this conclusion should be modified.

Figure 1: see comment 4 above.

Figure 5: See comment 5 above. This figure describes trends in percentile levels but please include the percentile levels that are plotted.

Figure 8-It is hard to see any trend in this figure because of the scale that covers all the percentile ranges. Could separate panels for low –median and median to high percentiles help with clarity? Or perhaps a figure like Fig 5 in Simpson et al. 2014 which depicts the table results clearly. An alternative would be to swap Figure 8 to the supplement and Table S3 to the main text as the table shows the trend results more clearly than the Figure.

Figure 9- the legend is hard to read.

Figure S6- this figure displays two y-axes but only one set of points are plotted?

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