

Interactive
Comment

Interactive comment on “A model study on changes of European and Swiss particulate matter, ozone and nitrogen deposition between 1990 and 2020 due to the revised Gothenburg protocol” by S. Aksoyoglu et al.

Anonymous Referee #1

Received and published: 30 June 2014

General Comments

This manuscript presents an application of a nested photochemical modeling system to assess the changes in pollutant concentrations and nitrogen deposition due to emission changes. The manuscript is well written and structured very clearly. The design of the model simulations is sound, though it appears that many of the results are quite sensitive to the choice of 1990 boundary conditions. Therefore, it would be good to see additional simulations and analyses that further quantify the relative impacts of

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



emissions, boundary conditions, and interannual meteorological variability on changes and trends in pollutant concentrations. Moreover, while the authors present an evaluation of the 2006 simulations, they do not put model performance in the context of other recent model evaluation studies such as AQMEII or FAIRMOD. In addition, the model evaluation is focused on operational metrics rather than on assessing how well the modeling system performs in capturing observed changes, i.e. dynamic evaluation. Given that PM observations are mostly lacking prior to 2000, a dynamic model evaluation may entail performing model simulations for some or all of the 2000 – 2010 time period when more observations were available to establish that the modeling system can capture emissions-driven changes. Performing such additional simulations, while certainly challenging to carry out, would provide a better context in which to judge and interpret the model predicted changes under the various 2020 scenarios.

Specific Comments:

Page 14,202, line 4: insert “with emissions” between “were performed” and “for 1990”

Page 14,202, lines 9-11: these statements appear to be based on the results shown in Section 3.2. However, as stated in that section, no PM_{2.5} observations were available that span the entire 1990 – 2005 time period. While the rates of change inferred from more recent measurements may lend some support to the modeled trends, these observations simply are too sparse or for PM₁₀ instead of PM_{2.5} to be able to draw quantitative conclusions about how well the model captured emissions-driven changes. I recommend rewording this section to more accurately reflect the findings discussed in Section 3.2 and to avoid overstating the accuracy of the model results. As discussed in my general comments, it would be best to perform model simulations for the 2000 – 2010 time period when more observations were available to establish that the modeling system can capture emissions-driven changes.

Page 14,202, line 19: insert “average” before “ozone levels in polluted”

Page 14203, line 12: please specify which aspects of the ozone distribution were dis-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

cussed in Wilson et al. (2012), e.g. annual mean hourly values, annual mean daily maximum values, summertime values, etc.

Page 14,205, lines 12-13: please discuss the impact of this choice on the simulated ozone changes and their comparison to observations. In particular, this model configuration does not allow the simulation of stratospheric ozone influences on surface concentrations while such influences may affect the magnitude, interannual variability and potentially trends of the observed concentrations used for model evaluation.

Page 14,205, line 18: please insert “annual mean” before “ozone”

Page 14,205, lines 25-26. Please provide more details on how the 1990 boundary conditions were prepared. Were 5 ppb subtracted from the ozone concentrations in all layers for all boundaries and all hours of the year? How were boundary conditions for other species that may affect ozone (e.g. PAN, CO) prepared? And how was seasonal variation taken into account, and for which species?

Page 14,205 line 27 – page 14,206 line 1: Which year were the ozone column densities extracted for – 2006 only? If so, how sensitive would the 1990 results be towards using ozone column densities that reflected conditions in that year?

Page 14,206, line 14: Was the replacement of TNO/MACC emissions with Swiss emissions performed for both the coarse and fine grid?

Page 14,207, line 12: Please define SNAP.

Page 14,208, lines 17 – 19: What were the results of this evaluation of meteorological variables? They do not appear to be presented in this manuscript. Also, why was the evaluation of meteorological variables performed only for a small portion of the modeling domain?

Page 14,208 – line 20 – Page 14,210, line 14: It would be good to put the 2006 model performance summarized here in the context of other recent 2006 simulations over Europe, e.g. the AQMEII Phase 1 simulations analyzed in Solazzo et al. (Atmospheric

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Environment, 2012 a and b)

Page 14,209, Lines 1-2. Are these 19 sites equally distributed throughout the modeling domain or concentrated in a particular area? Adding this information would help with the interpretation of the results.

Page 14,209, lines 2-3: Was analysis performed to confirm that the the model did not capture the strength these inversions? And if so, how did this affect simulated wintertime ozone concentrations?

Page 14,209, lines 5-6: Where is this shown? I suggest adding a time series for ozone similar to the PM2.5 time series shown in Figure 2.

Page 14,209, lines 7-8: Are the results similar for other stations in the modeling domain? Focusing this analysis on two stations only does not allow general conclusions about model performance at rural vs. urban sites.

Page 14,209, lines 10-12: The ozone distributions at Cheaumont are not “very similar”, the median of the model distribution is shifted towards the left of the observed distribution and the modeled distribution does not have any of the observed high values above 70 ppb.

Page 14,210, lines 16 – 28: Given that more observations became available starting around 2000, it would have been good to perform simulations for 2000 and 2010 emissions to see how well the model captured particulate matter changes over that time period. This would have been a more direct way of trying to establish the model’s credibility in capturing emissions-induced PM changes than the current discussion in this section that is more qualitative because of the lack of observations for the time periods that were modeled. While it may be beyond the scope of the study to perform such additional simulations, the authors may want to add appropriate caveats that a more quantitative analysis to establish the modeling system’s ability to capture PM trends is still necessary.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

[Interactive
Comment](#)

Page 14,211, Lines 1-4: It might be good to include a time series showing the 1991 – 2008 observations along with the model values simulated using 1990, 2005, and 2006 emissions (all using 2006 meteorology) to get an idea of interannual variability in the observed trends and how the model predicted emissions-induced change compares to this interannual variability.

Page 14,211, Lines 20 – 23. Please also add a discussion on the impact of the choice of boundary conditions for the 1990s on the results shown in Figure 7. For many areas, the increase between the simulations with 1990 and 2005 emissions appears to be close to the assumed boundary condition increase of 5 ppb.

Page 14,211, Line 26 – Page 14,212, Line 7: Why are the results in Tables 3 and 4 only calculated for the Swiss stations? Aren't EMEP/Airbase observations for 1990 and 2005 available to also analyze the observed and modeled ozone changes in other countries?

Page 14,212, line 27: same comment as above – why are results only shown for the Swiss stations?

Page 14,213, lines 16-17: Please add more discussion on the role of the choice of boundary conditions for the 1990s on the results shown here. It may even be worth performing additional sensitivity simulations with higher and/or lower boundary conditions for the 2005 and/or 1990 scenarios to more fully assess the impact of these choices on changes in AOT40 and SOMO35.

Page 14,215, lines 1-5. Are there any observations these modeled changes could be compared to? If so, this information should be added.

Page 14,215, lines 20-21. See my previous comment on Page 14,202, lines 9-11

Page 14,216, lines 13-15: Weren't the 2006 boundary conditions obtained from MOZART, not observations? Please clarify this statement.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

