Atmos. Chem. Phys. Discuss., 13, C1638–C1640, 2013
www.atmos-chem-phys-discuss.net/13/C1638/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Air quality over Europe: modeling gaseous and particulate pollutants and the effect of precursor emissions" *by* E. Tagaris et al.

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Received and published: 24 April 2013

There are significant caveats associated with trying to assess the uncertainties in emission inventories from the differences between observed and predicted concentrations. Many other sources of uncertainty contribute to the differences. Some of them are mentioned in the paper; for example, overestimated wind speeds may lead to under predictions of pollutant concentrations. Others such as the incommensurability of grid-cell averaged predictions with point measurements are not mentioned. The horizontal grid resolution used here (35 km) may be too coarse to characterize the variability in NO2 and SO2 fields. Wind direction is another very important source of uncertainty

but at these modeling scales the predictions are probably not very sensitive to wind direction.

The limitations mentioned above are common to all inverse modeling techniques targeting emissions. There are also caveats associated with the particular emission scaling technique used here. Again, some of them are mentioned in the paper such as the misinterpretation of transported pollutants as part of the emission uncertainties in small countries. There is another caveat not mentioned in the paper: that is the scaling of PM2.5 emissions with to ratio of observed to predicted PM2.5 concentrations. The predicted concentrations of PM2.5 components in Figure 4 show that most of the PM2.5 mass is secondary, in the form of sulfate, nitrate and ammonium, and probably a significant fraction of the organic carbon is secondary too. The uncertainty introduced by scaling primary PM2.5 emissions using a ratio governed by secondary PM2.5 concentrations is probably larger than the uncertainty introduced by scaling NOx emissions by the ratio of NO2 concentrations, which is mentioned in the paper. This uncertainty would be exacerbated by the systematic underestimation of secondary organic aerosol formation, a modeling issue acknowledged in the paper. Without speciated PM2.5 data, it would be difficult, if not impossible, to properly scale PM2.5 emissions. However, something that could be added to this paper is an analysis of the change in PM2.5 performance after the scaling of SO2 and NOx emissions alone. This may yield further evidence for the assessment of the uncertainties in SO2 and NOx emissions.

What I would recommend is a grouping of the limitations, which are currently scattered throughout the paper, as an organized section under the description of the method. This should be followed by a systematic discussion of the limitations under the discussion of the results. The caveats that I tried to identify above may be added to the discussion. I would also recommend adding clear warnings in conspicuous locations, such as the abstract and the conclusions, that the scaling factors in Table 3 should not be construed as measures of the level of uncertainties in the emissions inventories of those countries and that bottom-up or more accurate top-down (inverse modeling)

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methods are necessary for a true assessment of emission uncertainties.

As a minor comment, “the modified emissions improve model’s performance for all examined pollutants” is a broad generalization of the results. While this may be the case for Europe (Total) RMSE and MAE for Max8hrO3 increased in North and South Europe. Also, it would be good to list the number of monitoring stations, especially the number of PM2.5 monitors, for each country.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 6681, 2013.