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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol immission maps and trends over Germany with hourly data at four rural background stations from 2009 to 2018" *by* Jost Heintzenberg et al.

Anonymous Referee #2

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This manuscript investigates aerosol and trace gas concentrations and emission changes in northern Europe based on a combination of atmospheric measurements, emission inventory and air mass back trajectory calculations. The investigation has a potential to be scientifically interesting but, in its present form, lacks information and details that should be incorporated. My main comments in this regard are given below.

The background information for this study presented in section 1 is written very well. However, the text is totally European centered. For completeness for a reader not that familiar with this topic, it would nice to have at least one paragraph shortly summarizing whether similar work has been done outside Europe, for example in Norther America

or Asia.

There are a few issues related the applied data. First, the paper does not report anything about data coverage or its quality. Were there any major data gaps, not apparent in Table 1, that might influence the analysis performed in the paper? Is all the data quality checked and how? Do the detection limits of different instruments, especially what it comes to trace gas data, play any role here? Second, there are no continuous PM10 data for Melpitz, neither SO2 data for Collmberg. How does this influence the analysis of this paper? Third, what is purpose of presenting PM0.8 derived from SMPS if that is not used anywhere in the paper? One final issue: Figure 1 caption refers to particle volume concentration when showing PM10. I suppose this is a mistake.

What is the benefit of displaying particle number concentrations plots (Figs. 1, top and bottom left) for the rest of this paper? Particle number concentrations are not related to any of the discussions about emissions later in the paper. The authors briefly mention atmosphere new particle formation as one particle source, and emphasizes SO2 plumes or lofted layers as important locations for this source. If these two sources dominated new particle formation over Europe, how would they explain the common observation of regional new particle formation events, taking simultaneously place over tens to hundreds of km scales, in a vast number of surface measurement sites in Europe.

As mentioned by the authors, PM10 in Figs. 1 and 2 have practically no resemblance with each other. Noting that both these data sources are used together in later analyses, the authors should discuss this issue and its potential consequences in their analysis. What are the potential reasons for the differences between these two figures? Why the map in Figure 1 does not capture the emission hotspots of Figure 2? There is strong PM10 gradient from North-West to South-East in Figure 1. Why does Figure 2 not give any indication about such gradient? Are the main PM10 in the South-East direction located outside the map area?

The authors state that NOx an SO2 maps look very similar (related to figures 1 and 2). I wonder why these results are shown in the paper, especially when considering that both these trace gases are important parts of trend analyses presented later in the paper.

Based on Figure 3, the authors discuss the influence of wind speed and wet scavenging on measured concentrations. Unfortunately, this discussion remains very qualitative. If included in this paper, more concrete results on the influence of these two variables should be presented.

In the last part of the paper, the authors investigate the time evolution of emissions during the past 10 years. I have a couple of concerns related to this. First, the authors only stated that they optimized the annual and monthly emissions of 2009 for the rest of the time series using their measurements. This is too scarce information. The authors should provide more details on how this exercise what done in practice. Second, as well known, emission changes have been very different in different parts of Europe during the past years. Is the approach applied in this paper able to catch this feature in any way, and if it is not, this should be explicitly mentioned in the paper.

The final conclusion of this paper remain somewhat vague. The authors should more concrete summarize what new information this study brings on top of what is already known about the past emission changes in Europe. The authors mention some of the limitations of the current study yet, based on the comments give above, I feel that this list could be expanded a bit.

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