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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A refinement of the emission data for Kola Peninsula based on inverse dispersion modelling" *by* M. Prank et al.

Anonymous Referee #2

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The authors have investigated the disagreements between the existing emission inventories for the Kola Peninsula and identified possibleerrors in the emission database of EMEP concerning some major industrial sources located in the Kola region. They have also compiled a new inventory of anthropogenic emissions of SOx and PM for the Kola peninsula, which has been used in pollution transport simulations with the SILAM air quality model. Model results based on both the original EMEP and the revised emissions have been verified against observations.

The topic of this paper is certainly interesting and the performed analysis is valuable since it is important to improve the quality of emission inventories (if possible) in order to get more reliable results from air quality modelling.

For this reason, although the scientific novelty of this paper is somewhat limited, I

support publication of this manuscript after addressing the following comments.

Specific comments:

Section 2:

On page 15964 it stands that the emission database of EMEP includes both anthropogenic and natural emissions. This is somewhat misleading, as only SOx emissions from Italian volcanos and DMS are included in the EMEP database as natural emissions. There are many other sources of natural emissions that are not included. It is advisable to rephrase the sentence.

On pages 15968-15970 the SO2 emission from the EMEP database are evaluated. Among other issues, differences between available datasets before and after 2006 are discussed. In Table 1 you show the currently available data from 1980 until 2020, and identify a "dramatic decrease" until 2007 and a "return to the level of 1980s" in the 2010 and 2020 emission projections.

Some of the problems that you point out in section 2.1 and illustrate in Table 1 could have been clarified in advance, as the EMEP emissions are well documented.

While retrieving data from the EMEP database (http://www.ceip.at/emission-data-webdab/emissions-used-in-emep-models/), one gets information on the date when the given gridded data were created. According to the website, the emission projections for 2010 and 2020 are dated from 2004-07-15 (Vestreng, V. et al., 2004, Inventory Review 2004. Emission data reported to CLRTAP and the NEC Directive, EMEP/EEA Joint Review Report, EMEP/MSC-W Note 1, July 2004). Thus, these data were compiled before 2006. The same applies the data for 1980 and 1985, which are dated 2004-07-15 and 2002-10-21, respectively.

All gridded emission data for Russia between 1990 and 2003 (2004) were compiled in 2006. It is documented in the EMEP Status Report 1/2006 (http://emep.int/publ/reports/2006/status_report_1_2006_ch.pdf) that in 2006 historical

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emissions from 1990 to 2004 were recalculated based on the latest reported data and latest available spatial distribution in order to perform EMEP simulations for the above years in support for the Gothenburg Protocol. This explains why 2010 and 2020 emissions are more similar to the 1980s. These emission projections should be removed from Table 1, as the data is not up-to-date and it is not used in this study. Moreover, their presence in the table might certainly lead to confusion.

The misplacement of the Nikel plants had been identified already in 2007 and emissions from grid-cell (48,91) were moved to the correct Nikel location, i.e. into grid-cell (46,90). This can be seen in Table 1 for years 2005 and 2007.

Regarding the magnitudes of SOx emissions from the sources on Kola Peninsula in the current EMEP database, your study rather convincingly shows that there is some problem with the EMEP data. This is not an unexpected conclusion however. EMEP simulations indicates the same, as documented in various EMEP Status Reports in the past few years. Your study further confirm that a correction is needed in the EMEP database.

Section 3:

On page 15976 you say: "For the long term simulations only one configuration based on the Eulerian kernel of SILAM was used to limit the computational costs." Which meteorological input was used?

On the same page simulations using TNO-GEMS and PAREST-MP emission data are mentioned. Time series for these runs are shown in Figure 5, sub-figure (e). What was the meteo driver in these simulations? This information should be included in the figure caption or in the text on page 15976.

Technical comments:

Please correct line 9 on page 15978: "increase of the for mean values..."

Please insert a value in line 3 on page 15983: "(up to a factor of times)..."

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Numbers in some of the tables (e.g. Table 1 and 5) are difficult to read in the printed version of the manuscript. However, I understand that this might be difficult to correct.

In some figures (e.g. Figure 4, 5, 6, 7, 9 and 11) the axis labels are difficult to read.

In Figure 2. I would have preferred to see the gridded emission maps with more distinct colors than those provided on the Google maps. The Google maps are certainly very fancy, but a somewhat more colorful figure with well visible borders between grid cell would better illustrate the problem with having emissions misplaced in grid-cells of 50km size. Now the topography shading under the emission grids on the Google maps "smooths out" the differences between grid-cells and between the different years. In addition, the pictures are not from http://www.emep.int, but from http://www.ceip.at/.

There seems to be a mistake in the captions of Figures 5, 6 and 7. In the text both sub-figures (b) and (d) refer to "Eulerian SILAM, HIRLAM meteo". I assume that subfigure (d) should be "Eulerian SILAM, ECMWF meteo".

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 15963, 2010.