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Comment

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

Anonymous Referee #1

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The paper addresses an important topic concerning uncertainties in inventories of emissions from major industrial sources on the Kola Peninsula. There is definitely a need for this type of analysis to improve current inventories and thereby improve environmental impact assessments.

The general outcome of the exercise is rather moderate. Modelled SO₂ levels still underestimate observations by a factor of 2 to 3. This is a major problem in the presented analysis of the signal from the sources on the Kola Peninsula. The paper does not present an analysis of the agreement between boundary SO_x levels observed and modelled, and therefore it is not clear whether such discrepancies are due to the uncertainties in inventories for the Kola Peninsula. The authors state that uncertainties in DMS emissions are minor sources of uncertainty in the analysis - but this is not

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shown in the analysis and the statement may be questioned. Another potential source of uncertainty may be related to ship emissions that is a major source of SO_x.

The applied SO₂ to SO₄²⁻ conversion rate of 4 to 5% sounds very high. Usually values in the order of 2 to 3% are reported, and this is taking place far to the north where one would imagine even lower conversion rates. Such a high conversion rate therefore demands more argumentation.

The applied 20% fraction of SO_x emitted as SO₄²⁻ is very high and the analysis points at even 30 to 70% direct emitted sulphate. This analysis may, however, be questioned as this may be due to problems with the initial concentrations on the boarder of the model domain. The paper does not present any data on how well the initial SO_x value fits with observations.

The description of the applied metadata for the various model runs is rather confusing. Have the experimental campaign period been handled separately and thus calculations made using both sources of metadata? This should be clarified!

It is not clear what kind of data assimilation that has been performed in this study!?

The authors put emphasis on the use of ensemble modelling, although the computations are performed using tools with differences regarding only transport description and meteorology. This is not the common use of the ensemble principle.

Parts of the text are rather imprecise and should be refrased. In general the paper would benefit from a rewrite of some sections.

- Throughout the paper the Nikel plants site is referred in numerous and sometimes even confusion ways. “The Nikel”, “Nikel site”, “Nikel area”, “Nikel NO_x emission”. This should be streamlined, and the authors should refer to emissions from the Nikel plant and not to “Nikel emissions”. - Page 15967 line 26 “by a factor of times”? Please insert the value! - Page 15968 line 6 has it been documented that PAN is a major source of NO_x in this region? Why is it important to talk about NO_x here in a paper

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concerning SO_x? - Page 15968 line 18 how can distributions be interpolated. One may assume similar distributions between years and scale these, but interpolation sounds weird. Sources may open and close! - Page 15970 – 15971 the description of the available inventories is a repetition and should be avoided. - Page 15972 line 4 is a factor 1.7 between two years a moderate drop in emissions? One would argue that a 70% change is substantial.

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

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