

Interactive comment on “Pan-European rural atmospheric monitoring network shows dominance of NH₃ gas and NH₄NO₃ aerosol in inorganic pollution load” by Y. Sim Tang et al.

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Received and published: 8 July 2020

Understanding the budgets of sulfur and nitrogen compounds and how they interact by e.g. inorganic aerosol formation is of key importance. The NEU network provides an outstanding contribution as it provides a comprehensive and quality controlled dataset across many countries. This paper clearly shows the large efforts required to set-up and run such a large monitoring network. Hence, although the dataset is from some time ago, it should be published and I recommend to publish the paper with a number of revisions.

My main concern is that the paper is quite long. I have the feeling that some features

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which are now presented at different locations could be merged to guide the reader. One of these is the message that ammonium nitrate dominates above ammonium sulfate which is concluded from the correlation between components, ion balance, seasonality, etc. I would appreciate if the authors could try to focus the results section into a more integrative storyline than the stepwise approach chosen now.

Two parts I feel are less important for the paper are the following:

1. Concentration to Country emission correlation: The short life time of ammonia and NOx cause substantial gradients within larger countries. For that reason I would argue that the correlation between country emissions and averaged concentration levels is not saying a lot. Figure 9 presents these data and is hardly discussed in the paper. The emission density in the surroundings cells to me sounds more appropriate and tells something about the representativeness of the stations for the different pollutants.
2. Section 4: This section is hardly connected to the monitoring network results. I would rather see a discussion on the future of this network. Should it be continued? Adapted? Or?

The two main findings presented in the conclusions section are not new, and a few references to earlier works could be provided.

Content wise, I have the feeling that the role of chloride depletion reactions of sea salt are interpreted as outliers in the interpretation of data, see below.

As a modeler I would be very eager to compare our model results to the dataset and hope that the data will be openly available.

Individual remarks:

Title: I would recommend to move the word “atmospheric” to in “inorganic atmospheric pollution”

Line 7: Vieno reference is a bit strange here – not a monitoring work

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Line 9: the negative impacts ... should not be a new paragraph. The first two paragraphs contain two sentences now.

Section 2.2.1 page 7 line 25: Could you indicate the breakthrough estimation is in comparison to ammonium aerosol levels, especially for the agricultural sites.

Page 8, line 13: please refer forward to the results section on the impact of the NaCL denuders.

Section 2.6: Some countries may have large shipping contributions to NOx and SO2, how did you treat these in the indicator used here? Why did you choose 4 grids around a station and not the nine around and including the grid cell with the station?

Section 3.3.1 page 14 Line 1-3 details on the dry deposition schemes seem out of place here.

Page 14 line 36-43: The comparison between N and S is based on mass here. Given the scope on ecosystem deposition provided elsewhere I could imagine that a comparison based on acid equivalents makes more sense than the mass. I do not see the consistency between the currently higher N levels and emission reductions since the nineties as the emissions did not start from a ratio of 1:1.

Page 15, line 16. The 10-50% contributions in Putaud et al refer to the ammonium salts, not only ammonium. Please correct. Moreover, this paragraph seems more appropriate in the discussion or implication section than in the results chapter.

Page 16 line 3. Here the correlation between precursor and aerosol is discussed in the paragraph on the correlation with emission densities. Right place?

Page 17 line 3. HNO3 maybe highest in eastern Europe, but NOx emissions aren't. Could it be that the lower ammonia and hotter summer climate plays a pronounced role in the explanation as indicated in the seasonal cycle with summer maxima in the region (in contrast to western Europe). Similarly, in the presentation of the oxidized nitrogen on page 20 (L 24) the limitation on ammonia availability could be mentioned.

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The higher correlation between nitrate and ammonia emissions is indicative for this issue as well. Ammonium nitrate formation could be checked with the ammonium salt ion balance. Often inverse relationships between nitric acid and ammonia are modelled due to the limiting impact of the equilibrium with ammonium nitrate. Do you see this feature in the data?

Page 21, the current levels are interpreted in relation to emission reductions which are not indicated from this network. The SO2 to SO4 ratio variability across the network may be the most interesting feature concerning sulfur for model developers. Did you see the anticipated systematic behavior for this ratio?

Page 21 the Bugac discussion interrupts the main information flow.

Page 22. The ion balance for southern Scandinavia may be affected by sodium nitrate formation and not so much by an overestimation of SO4. Na:CL depletion ratio may give a hint here. Further down on the same page the remark is made but no connection is made.

On page 24 another check is made on ion balances with hard statements on lab quality— are these issues not connected and is one actually looking at sea salt depletion reactions?

Page 22: does the HCl distribution provide a hint at the importance of the marine source for it?

Page 27 line 1-5: the impact of ammonia and temperature on seasonality of nitric acid is not discussed ad should be mentioned. OK, it is done in the next paragraph. Why not combine these?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-275>, 2020.

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