

Interactive comment on “Importance of mineral cations and organics in gas-aerosol partitioning of reactive nitrogen compounds: case study based on MINOS results” by S. Metzger et al.

Anonymous Referee #2

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This is an interesting paper describing the use of 3 thermodynamic equilibrium models of different complexity to understand the partitioning of nitrate and ammonia between aerosol and gasphase. The box model experiments are constrained by measurements from the MINOS campaign, during three periods characterized by rather different aerosol composition. The authors show the importance of including mineral dust components, and perhaps for the first time the importance of organic acids on the equilibrium of ammonium.

My more fundamental criticism is that the update of the thermodynamic model EQSAM

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is described as being "to be submitted", and that there is no way to verify what is actually being assumed in this work. I would like to see a brief summary of these major updates are being put in an appendix to this paper, or that these draft papers are being made available to the reviewers.

The other caveat is that it is relatively difficult to judge whether a less constrained approach of the 3 equilibrium models would still yield such comparable results, especially under different chemical conditions. This is not an issue that the authors should resolve in this paper- but the reader should be aware of it- and I hope that the authors can qualitatively discuss this.

Detailed comments: - In the abstract (and throughout the whole paper) it should be clearer that model experiments were constrained by measured total NH_x (NH₃ and NH₄) and total NO₃ (HNO₃ and NO₃) concentrations.

- p. 12858 l. 15 add ...nitric acid IN THE EASTERN MEDITERRANEAN.
- p. 12859 l. 5 ammonia is biogenic (certainly not only from soils)
- p. 12860 l. 10 give references to this (e.g. Schaap et al.)
- p. 12860 l. 20 give reference for this process
- p. 12862 l. 4 in Sciare.
- p. 12862 l. 18 describe how the acids are lumped according to protonation constants (equation)
- p. 12862 l. 19 in the presence.
- p. 12863 l. 9 why is equilibrium assumed (especially in the light of the statements earlier that nitrate can evaporate from smaller particles and condense on larger).
- p. 12863 l. 9 probably you should mention already here the names of the eq. models.
- p. 12863 l. 9 in this section it should be clearly mentioned that you use the total

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NH_x and total NO₃ (I think separately for fine and coarse fraction, but not described) constrain the models, and re-iterate that the resulting equilibrium composition is the modelling part of the story. Also other components constrain.

p. 12870 l. 2 According to=>similar to

p. 12870 and throughout the text; would be helpful to repeat in words every now and then what the different cases stand for.

p.12871 l. 25. I don't understand; you said you apply equilibrium modelling, and yet you turn back to dynamical approach. What sensitivity are you discussing? The fact that mineral aerosol volume would be larger when the non-soluble part is included? You don't tell how you look into this sensitivity.

p. 12872 l. 15. 'not consistent' is not the right wording. They are consistent but suggest a different level of agreement. Indeed a correct timing of some peak value may give a high correlation whereas the bias may be large.

p. 12862 l. 20 and further. The discussion is not clear. Apparently there is an issue that measured organic acids described in section 2 are only a fraction of the sum of the organic acids. I think that the authors propose to use a fraction of the measured OM as acids.

but then in:

p. 12873 l. 11 there is a statement that measured values of Sciare 2005 are used. I can not follow what has been done. I suggest a clarification in various parts of the text

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 12857, 2005.

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