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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric measurements of gas-phase HNO3 and SO2 using chemical ionization mass spectrometry during the MINATROC field campaign 2000 on Monte Cimone" *by* M. Hanke et al.

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The paper presents a field study, using chemical ionization mass spectrometry (CIMS) for on-line detection of gas phase HNO3 and SO2. The field study was aiming at mineral dust - photo oxidant interactions, in particular at possible (heterogeneous) reactions of SO2 and HNO3 on dust particles. Unfortunately, only a brief dust episode was encountered, which however showed evidence for enhanced uptake of HNO3, while SO2 was much less affected.

The contribution is relevant, original, informative and of high scientific value. Publica-

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tion in ACP is recommended after considering following comments.

Section 2.3: What is the effect of the rather slow response of the HNO3 signal on the data accuracy? Could this induce a systematic error? Is an averaging over 30 minutes sufficient to smoothen out the delay effect?

In section 3.2 (comparison of HNO3 with NOy) I feel the authors are not really touching the real problem; they present a linear regression through a cloud of points on the HNO3 versus NOy scatter plot and are concerned about the non-zero offset. First of all, it would be useful to indicate to which extend HNO3 is expected to contribute to NOy, which are possible other components, and which factors can explain the variability. Then, I wonder if the offset of 0.5 ppbV is significant, seen the large spread on the datapoints, and the bias induced by the 3 points with highest NOy. In stead, the data cloud can be interpreted as lying between to limiting lines, each of them having an intercept which is probably not significantly different from zero, and with slopes from ˜0.3 to ˜1.5 (naked eye estimate). The question is then how to explain this range in ratios and in particular when the slope > 1; can these data be attributed to a particular event or air mass or wind direction? The estimate of the upper limit of nitrate contribution from aerosol is useful, but re-fitting the regression through the data cloud after subtraction is meaningless (as mentioned by the authors). So I suggest to drop this. For clarity, I suggest to leave out the data points of 28 June as they are clear outliers, but do not affect the general picture. In Fig. 8, the parameters b[0] and b[1] should be defined and values should be limited to 2 or 3 significant figures.

Section 3.3 (Mineral dust event): it would be very informative for the reader to include also a time series of the dust concentration (mass or number of coarse particles) to illustrate the exact timing of the event.

Language use: Avoid the phrase "It is distinguished between..."; use instead "the figure distinguishes between..." , "we distinguish between..." Many phrases are long and awkward, more use of comma's should be made. If possible, have the manuscript edited

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Interactive Comment

by a native English speaker. The authors have dedicated much effort to describing the experimental arrangement, calibration procedures and possible artifacts. To my taste (but this is a personal opinion) this section might be a bit too heavy for the general readership, and I think a more concise description would make this part more accessible. For instance, I don't think it is necessary to mention the method used by Adams et al. (2002) to make the point on SO2 uptake (page 2212 line 5) (for other lab studies cited, the used method is not given).

Minor remarks: The CITE 3 experiment seems to be rather a long time ago. Is the critique on SO2 measuring methods still valid nowadays? Indicate the flow direction in the SO2 filter branch on Fig. 3 Page 2220, line 7 - 8 is a bit obscure. What is meant with ambient conditions? RH and temperature? What is meant by potential effects? Page 2232 lines 2 and 4: housekeeping: should be logbook?

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 2209, 2002.