

Interactive comment on “Intercomparison of NO₃ radical detection instruments in the atmosphere simulation chamber SAPHIR” by H.-P. Dorn et al.

Anonymous Referee #3

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The manuscript by Dorn et al presents an intercomparison of seven optical absorption NO₃ instruments in the environmental chamber SAPHIR. Instruments were compared under a variety of conditions to investigate possible interferences by NO₂, H₂O, and aerosol. The manuscript provides a detailed statistical analysis of the various experiments and the main conclusion is that the instruments generally agree well with each other, that all participating groups provided good estimates of their errors, and that only aerosol lead to measurable interferences in some of the instruments. In addition, the manuscript makes a recommendation that aerosol filters commonly used in CRDS instruments be frequently changed.

The manuscript shows some very interesting data and the interpretation of the data is well thought through and conclusive. Description of the participating instru-

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ments, the experimental setup, and the statistical analysis is very detailed. My main comment on the information presentation is that the authors should provide a clearer description of the lessons learned from the experiments. For example, what would be an optimal exchange frequency for the aerosol filters? What further improvements do these instruments need? What error sources should be further reduced? How do the lessons learned apply to atmospheric deployments and what do the intercomparison results mean for past and future atmospheric observations of NO₃. Without clearer and more general conclusions this manuscript will only serve the participants as a proof of the quality of their individual instruments, but have no lasting value for the scientific community.

Despite this concern, I support the publication of this manuscript in AMT, as intercomparisons of instruments are crucial to the community and the manuscripts data and interpretation is of high quality. I have a number of comments that the authors should consider before the manuscript is published in AMT.

At times the manuscript is unnecessarily lengthy and the many details obscure the main points of the text. For example, the CRDS instruments and the CEAS instruments are so similar in their principle that Section 2.1 could be considerably shortened by providing a general description of each instrument principle and tables with the details of the different instruments. The current detailed description could be moved into the supplemental material. This would also make it easier to understand where the differences between the various instruments really lie. Also the description of the individual experiments could be shortened since much of the information is shown in Table 1 and Figures 3 and 4.

Page 309, lines 9-13: I agree with the authors that intercomparisons in environmental chambers are a great way to learn about instruments and their behavior.

However, this is only one side of the story. One could also argue that the highly simplified conditions in a chamber do not allow for a full investigation of possible, and often unknown, interferences with other atmospheric components. I suggest adding a sentence that more clearly explains this issue.

Page 310 line 21. This is a contradiction with the introduction that states that DOAS has been used for the past 3-4 decades to measure NO₃.

Page 315, line 18ff: To be consistent the various error sources in the MPI-CRDS should also be quantified. Again, it may help listing all the error sources of the instruments in a table to give the reader a better overview.

Page 324, line 3: "deconvolved" should probably be "convoluted"?

Page 325, line 2: Should the error of the cross section be 5% (1σ)?

Page 325 bottom and page 326 top: Please comment on how this systematic variability was identified. A better explanation on how arc-lamp instabilities can cause such effects would be helpful here.

Section 3.1: The precision is not defined as the error at zero concentrations, but rather, speaking in simplified terms, as the "reproducibility" of a measurement at any concentration. Please explain why the precision at concentrations near zero are representative for the precision at higher concentrations.

Page 331 top. This paragraph is very similar to the caption of Figure 3 and can be shortened accordingly.

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