

Review of “Intercomparison of IBBCEAS, NitroMAC and FTIR for HONO, NO₂ and HCHO measurements during the reaction of NO₂ with H₂O vapor in the atmospheric simulation chamber of CESAM” by H. Yi et al.

The article describe simultaneous measurements of NO₂ and HONO generated in a simulation chamber experiments by several in-situ monitors with a focus on UV LED based incoherent broadband cavity-enhanced absorption spectrometer (IBBCEAS) and its comparison against FTIR and chemiluminescent instruments. The experiments are well planned, and meticulous description of the instrumentation, measurements, intercomparisons and error analyses are provided. Well written, in a general sense, although small text corrections are sought. However, several aspects of the study need to be addressed before the paper can be accepted for publication. I suggest that the manuscript be majorly revised as per the comments listed below:

1. The diameter of the chamber is 2.13 m (page 3, line 88). Is it exactly the separation of cavity mirrors? Also, mirror radius of curvature is 2 m. Does radius less than mirror separation have any impact on cavity transmission or its stability?
2. In section 2.3.1, it is mentioned that I_0 is measured with nitrogen or dry (zero) air in the cavity. In the equation (1) for extinction coefficient, terms for Rayleigh scattering and Mie scattering are included. I think your target samples are trace quantities of NO₂, HONO, H₂O (vapor) and HCHO; all in gaseous state. When your I_0 already has zero air how can there be a contribution from Rayleigh scattering of air and Mie scattering by particles just by introduction of calibration gases? If your I_0 was measured in vacuum, then Rayleigh scattering by gases would be significant. I could not find any introduction of aerosol into your cavity either. Therefore, the equation (1) is misleading; also true for the equation (2) for mirror reflectivity retrieval where second term inside the parenthesis cannot be detected with dry air present in the I_0 spectrum.
3. In fact, if one looks at figure 3 it can be seen that the Rayleigh scattering cross section is 6 – 7 orders of magnitude smaller than the absorption cross sections of the calibration and target gases. Therefore, including zero air scattering cross section spectrum in the calculations makes no sense to me. I suggest to correct section 2.3.1 appropriately.
4. On page 11, line 212, a singular value decomposition method seems to have been adopted for analyzing data. No elaboration or specifics about this method could be found in the article. It is suggested that the method is elaborated in detail or appropriate reference(s) be specified.
5. How did you calculate the overall uncertainty from the individual uncertainties that you specified?
6. Line 220 on page 8: It is mentioned that the statistical uncertainty from the fit was included in your error analysis. No mention was found on how this was calculated and what is the magnitude you obtained as uncertainty from the fit. How did you estimate this?
7. From figure 3 (b) it looks like HONO has two absorption peaks, the first one close to 355 is interfering with an absorption peak of HCHO. In fact, after 357 nm, there is no influence (spectral interference) of HCHO presence on NO₂ and HONO. Interestingly, the LED has an order of magnitude less power below 357 nm. If it is just the NO₂ and HONO measurements that the authors are interested in, use the spectral region from 360 nm would do the job. From figure 4 it is amply clear that in the 366 – 372 nm region, HONO and NO₂ absorption can be clearly distinguished, then what is the need to extend it to 351 nm where light intensity itself goes negligible?

8. While calculating the uncertainty, were the choice of spectral (sub) intervals used for analyzing different species considered? For example, like I said in the above comment, full spectral band or a sub interval of 360 – 378 can be used to analyze for NO₂ and HONO. Would the results be the same? Could you get the same uncertainties for both spectral intervals? If different, then the error analysis must include the errors due to the choice of spectral interval. In your case, if HCHO is the target species, then analyzing it in the 351 – 360 nm band may be desirable than the full window (of course, with NO₂ and HONO as co-analytes). If there is an effect due to spectral window choice, then optimization of the spectral interval of analysis for better results is desired.
9. In the section 2.3.2 on page 8, FTIR spectral analysis is described. Spectral fit in support of FTIR measurements, like corresponding to figures 3 and 4 for IBBCEAS instrument, needs to be shown.
10. In this study authors used “synthetic” reference spectrum by Barney et al., 2001, for analyzing HONO from FTIR measurements. On page 10, line 303, the authors mention that the use of a different cross section for HONO introduced a larger error. Could you do an error analysis for FTIR measurements similar to that performed for IBBCEAS as well and add to the paper?
11. On page 9, line 274, comparison between IBBCEAS and FTIR measurements is specified. Although the discrepancy between the measurements is attributed to lower detection sensitivity of FTIR, the measurements of concentrations at well above detection limits show considerable deviations (Figure 7c). So, this is not the reason as mentioned in the paper. Either the FTIR is underestimating the NO₂ concentrations due to some bias or the IBBCEAS is overestimating. Spectral fit from FTIR would help in this situation to assess the spectral validation of the measurements. I would tend to believe IBBCEAS measurements, more so because its calibrations and spectral validations are provided whereas less information regarding FTIR spectra and fit are provided in the paper. Error bars are given for IBBCEAS side, but no uncertainty analysis seems to be available for FTIR. This needs to be addressed better than attributing deviations to lower sensitivity of FTIR. Line 339-340 in the section 4 (Conclusions) need to be corrected accordingly.
12. This paper has both FTIR and IBBCEAS mounted on the wall of an atmospheric simulation chamber with two separate pieces (like a transmitter-receiver arrangement or the like) with atmosphere in-between. This is done to directly use atmosphere as sampling volume and avoid sampling losses in extracting it into a containing volume. This is a step towards applying this technique into the real atmosphere. However, the real atmospheric conditions may have other interfering gases and suspended aerosol (absorbing, non-absorbing, SOA, etc.). Many atmospheric simulation chamber experiments assessing such measurement methodologies specify the instrument performances under varying atmospheric conditions. How would your measurement sensitivity be affected in the presence of aerosols in the chamber?
13. Finally, a few suggestions for text edits:
 - a. On page 2, line 43, “topics” to be replaced by “topic”
 - b. Line 52, “following” to be replaced by “followed”
 - c. On page 6, line 163. The sentence starting with “When NO₂ concentration” is incomplete.
 - d. Line 168. The sentence starting with “As described ...” is incomplete.