

Interactive comment on “A compact, high-purity source of HONO validated by Fourier Transform Infrared and Thermal Dissociation Cavity Ring-down Spectroscopy” by Nicholas J. Gingerysty and Hans D. Osthoff

Anonymous Referee #1

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This paper discusses the optimization of a HONO calibration source suitable for field work. For many instruments, the source needs to be relatively free of compounds that could interfere with the measurements of HONO. Current methods of HONO production often produce concentrations that vary with operating conditions (humidity, temperature, flow rates), as well as requiring long periods before stable outputs are achieved. Because of issues of stability and reproducibility, the output of HONO calibration methods need to be confirmed in the field with a secondary HONO instrument, often an absolute measurement technique such as absorbance spectroscopy or a chemilumi-

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nescent NOx analyzer. Ideally, a field calibration source would provide stable and reproducible concentrations of HONO that could be quantified in the laboratory and reliably reproduced in the field, such that a secondary instrument would not be needed for source quantification.

The source described in this paper is based on the design of Febo et al. (1995), which has been used extensively by other groups as outlined in the manuscript. The authors describe changes in the design of the source that appear to minimize impurities such as CINO and NOx, and characterize the output of the source using FTIR and TD-CRDS instruments. They find that their “optimized source” can produce concentrations of HONO with approximately 97% purity and is stable after 1.5 hours. The authors report HONO concentrations from the source ranging between 1.5 and 3 ppm from FTIR experiments and 11 and 50 ppb from TD-CRDS experiments (with some dilution occurring).

The paper does provide some new information regarding the production of a stable, high purity HONO source for instrument calibrations and is likely of interest to the atmospheric chemistry community. While worthy of eventual publication, the paper does not provide sufficient information to give the reader confidence in reproducing the measurements. More experimental details regarding the individual flow rates, humidity, and temperatures should be provided. It is also not clear whether the source routinely produces concentrations of HONO with the stated purity over a range of operating conditions. The authors should include measurements of the HONO concentrations and impurities produced by the source over a range of temperatures and humidities that may be encountered when the source is used in the field. The authors should also provide information on the reproducibility of the source using the same operating conditions after it has been turn off. Illustrating that the output of the source is stable and reproducible from day-to-day might suggest that the source could be robust enough to be used for instrument calibrations without requiring a secondary instrument for quantification in the field.

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Specific comments:

Methods: As mentioned in the paper, previous studies have demonstrated that the production of HONO by reaction R4 requires the gas stream to be humidified. However, the authors do not state the humidity of the gas stream used in their experiments.

Page 4, line 88: Is there any reason for the 20 ccm dilution with O₂? If so, why O₂ and not N₂, zero air, or scrubbed room air?

Page 4, last paragraph: How much dilution was typical? What are final flow rates? More experimental details should be provided.

Fig. 1: The position of the needle valve and dilution do not match the written description. The figure only shows the input of the TD-CRDS being diluted and not the FTIR, while the text describes dilution before both instruments. The schematic would benefit with typical flow rates.

Page 7, line 161: In this discussion, the authors are probably referring to reaction R5, and not R6.

Page 7, line 169: It appears that the improvement in source 2 is only a reduction of CINO contamination, as both the NO and NO₂ concentrations increased. This should be clarified.

Page 8, equation 2: Check the equation – there appears to be an error translating the fonts.

Fig. 5: The authors should define the lighter shaded points in both plots. Are the lighter shades indicating points while the source is stabilizing? If so, why are there lighter shade points at the end of the time series? The authors should clarify how they determined that the output had stabilized.

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