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Atmospheric Measurement Techniques
Discussions
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Interactive comment on "Comparison of nitrous oxide (N_2O) analyzers for high-precision measurements of atmospheric mole fractions" *by* B. Lebegue et al.

Anonymous Referee #3

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This paper describes testing and comparisons of several methods for the measurement of nitrous oxide in the atmosphere. Resolving small gradients in the remote troposphere is a particularly challenging. As optical instruments have recently been developed, there is hope for improvement over traditional methods. Overall, this is a well-written and timely paper.

General comments: The sections focusing on testing (repeatability, drift assessment, long-term stability, effect of humidity) are interesting and useful. In section 3.4, it might be worth repeating that these tests were performed under controlled conditions (little variation in temperature?). For the short-term repeatability tests, were 15-20 minute

periods chosen because this corresponds with the Allan variance minima for most instruments? Or, perhaps it was necessary to wait 15-20 minutes between wet (ambient) and dry (tank) samples. If either are true, it should be stated explicitly.

The air comparison could use more explanation. I think the authors should comment on how reliable these results are considering that some comparisons are based on measurements of dry air, while some are based on wet air with factory water vapor corrections, which, except for QC-TILDAS, you have shown are not adequate. You mentioned that the water vapor during the third period was < 1% (winter), but I suspect that water vapor was at least as high during the second period (May), yes? Thus, I'm surprised that the dispersion of ICOS-EP/FTIR results shown in Fig. 6 are so tight, considering that the comparison involved ambient (wet) air. From figure 4, we could expect positive bias of up to ∼0.5 ppb as water vapor approaches 2%. Thus, I would expect to see a tail on the negative side of the distributions in Fig. 6 for these instruments. Further, the slopes shown in Table 8 suggest real calibration differences. But these could also be related to water vapor, so how relevant are the slopes shown in Table 8? If you choose to report them, I suggest including an uncertainty and the N2O range measured. Alternatively, it might be better to replace Table 8 with a figure showing mole fraction differences as a function of time, or instrument X vs FTIR as a scatter plot with r-squared shown in each plot.

Specific comments: p. 10945, line 18: check text: "mid infrared vs mid-infrared"; "sweeps in the frequency" p. 10948, line 28: I'm confused by the term "scale" here. Clearly you have different sets of calibration standards. Are they all referenced to the same scale, i.e. WMO X2006A, but by various pathways? Or is the MPI scale independent (p. 10949, line 4?). p. 10953, line 14: should "decreased to" be replaced with "decreased by"? p. 10955, line 1: replace "constructors" with "manufacturers" p. 10975, fig. 1: Are the vertical lines on the lower figure necessary? They don't seem to correspond with anything. Table 6: It might be useful to include the short term repeatability (1-min) in this table so that it would be clear from the table why some instruments could not reach the 0.1 ppb target. Table 8: Not sure that the intercept is all the important in this table. However, I would suggest that an uncertainty on the slope could be included. I would also suggest showing expanded time series or differences between instruments during the air comparison, so the reader could then see the range of N2O measured. Figure 6: Should include in caption that manufacture's water vapor corrections were applied to ambient air measurements

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