

Interactive comment on “Comparison of nitrous oxide (N₂O) analyzers for high-precision measurements of atmospheric mole fractions” by B. Lebegue et al.

Anonymous Referee #2

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The manuscript ‘Comparison of nitrous oxide (N₂O) analyzers for high-precision measurements of atmospheric mole fractions’ by B. Lebegue et al. presents a comprehensive comparison between literally all currently available measurement techniques for nitrous oxide. It is a very valuable contribution to the atmospheric measurement community, and well suited for publication in AMT after considering the following mainly minor comments.

General remarks: As already mentioned by the other reviewer, two manufactures are co-authors on this paper, whereas the other three manufactures are not. This probably originates from the fact that two instruments were directly provided by these manu-

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factures. The manufactures of the other instruments should also have the chance to at least look at the results, and provide input if necessary. If this has not happened already, I recommend that the other manufactures are contacted before the final AMT paper goes online.

Specific comments: Page 10948, line 20: concerning the use of calibration gases with synthetic air matrix, the N₂, O₂, and Ar content should be specified. Spectroscopic techniques can be very dependent on the matrix, and it would be important to mention this somewhere in the paper.

Page 10953, Linearity assessment: Interestingly, the slope was changing quite significantly for some of the tested analysers. Do you have an explanation for this? This also stresses the need of two or more calibration gases, as you suggested.

Figure 3, temperature dependence, and discussion in the text: There is some interesting structure in the QC-TILDAS instrument data, which has nothing to do with temperature dependence. Do you know the reason for this behavior? The conclusions that there is no temperature dependence for this instrument is probably not supported by the data you present here. It could well be that you see a dependence if you consider only the data where you have an actual temperature change. For future temperature dependence tests I would recommend that a temperature cycle is repeated at least three times.

Figure 4, discussion of water vapour correction: It should be stressed here that the determination of the correction is always necessary when humid measurements are made. P 10959, line 2 states that a careful evaluation is not necessary for the QC-TILDAS and FTIR instruments. For the FTIR, I agree, since it has a built in dryer, but the QC-TILDAS must be checked, since there is the potential for instrument to instrument differences, and the correction can also be changed in the data acquisition software.

Air comparisons: If I understood correctly, no calibration of the instruments were made

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over the 100 h air comparison periods. The result therefore might depend on the time since the last calibration of the instrument. It would be good to have comparable conditions for all comparisons, e.g. an initial calibration at the beginning of the experiment. Furthermore, the manuscript is relatively vague concerning recommendations of the frequency of calibrations. It clearly seems that the results for many instruments would significantly improve if more frequent calibrations are performed. Furthermore, it also would be valuable to have a time series plot (including the difference) in addition to the deviation histograms. This would give additional information, and drift issues etc. can currently not be seen in the data presented for the air comparison.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 10937, 2015.