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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An improved infrared carbon monoxide analyser for routine measurements aboard commercial airbus aircraft: Technical validation and first scientific results of the MOZAIC III programme" *by* P. Nedelec et al.

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General Comments: The paper presents an excellent description of the CO analyzer developed for the MOAZIC program, and gives strong arguments that it makes accurate and precise measurements. My only suggestions are for relatively minor revisions specified below.

Specific Comments: There are three, relatively minor scientific issues that should be addressed.

1) Paragraph beginning on Page 3719 and ending on 3720 - More information regard-

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ing the O_3 trap should be given. Specifically what tests have been done to ensure that the trap does not affect the ambient CO concentration, either positively or negatively? A lack of correlation between CO and O_3 in the stratosphere does not indicate that the trap is benign. As the authors show in later sections, CO and O_3 are negatively correlated in the stratosphere. Indeed, if a negative correlation is not observed up to 1 ppmv O_3, this may indicate a positive O_3 interference in the CO measurement. The authors should quantitatively discuss the O_3 - CO correlation that they observe in the stratosphere and compare it to what has been observed in various research programs.

2) The authors discuss several comparisons of their instrument's results with other measurements. Figure 4 presents the correlations of the simultaneous measurements. Slopes near unity show that the instruments, on average, measure the same concentration and thus provide a check on the MOZAIC instrument's accuracy. However, the precisions of the compared instruments are reflected in the scatter of the points about the regression line. The authors should quantitatively compare the deviation of the points from each regression line with that expected from the instruments' precisions.

3) Figure 6 shows CO and O_3 measurements during an encounter with a tropopause fold. In the discussion beginning on the second line of Page 3724, the authors seem to imply that the relative variations of CO and O_3 are not what they expected, and they attribute this to the slower response of the CO analyzer. However, I think that the relative variations are very close to what is expected. O_3 rose from background values near 50 ppbv to 95 ppbv while CO dropped from a background of 143 to 117 ppbv. According to Figure 10 of the paper, stratospheric air has about 37 ppbv CO when O_3 is 320 ppbv. This is similar to the values observed by others, (e.g. Danielsen et al., Three-dimensional analysis of potential vorticity associated with tropopause folds and observed variations of ozone and carbon monoxide, Journal of Geophysical Research, 92 (D2), 2103-2111, 1987.) If enough of that stratospheric air were added to the background tropospheric air to raise the O_3 from 50 to 95 ppbv, then CO would fall from 143 ppbv to 125 ppbv. This is close to that observed, and in fact CO falls a little more

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than expected rather than less. The authors should discuss these considerations more clearly.

Technical Corrections:

1) The second and third to last paragraphs of the Introduction are largely duplicative. They should be combined into one.

2) In the last paragraph of Section 2.2, the higher pressure of 2.5 bar is said to increase the signal-to-noise ratio by a factor of about 2. I assume that this is compared to a pressure of 1 bar, but this should be explicitly stated.

3) Page 3719, lines 5-7 - The authors comment on the importance of not having compressed gas cylinders. This repeats previous statements. The repetition should be removed.

4) The authors have produced an excellent English manuscript. However, there remain a few minor English misusages that should be corrected.

5) Some more details of the instrument precision should be given. Page 3720 describes "noise" of 5 ppbv, and the caption of Fig. 2 suggests a precision on the zero of 5 ppbv. Since the ambient concentration is calculated from the difference of the signal in the measure mode and the zero mode, should the overall precision be calculated as the propagation of the zero and the signal precision, i.e. about 7 ppbv?

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