

Interactive comment on “Inter-comparison of four different carbon monoxide measurements techniques and evaluation of the long-term carbon monoxide time series of Jungfraujoch” by C. Zellweger et al.

C. Zellweger et al.

Received and published: 15 May 2009

We thank the referee for the helpful comments and suggestions on our manuscript. For clarity, the suggestions are italicized, followed by our replies. Technical errors were corrected and are not included in the following reply.

Introduction, page2384, line20: "To date, no comprehensive CO instrument intercomparisons have been published..." The authors cite several old papers for intercomparison results, but miss a recent paper by Japanese groups. Tanimoto et al. (2007) compared NDIR to GC/RGD, and three different NDIR instruments, including Horiba's CO monitor.

The above mentioned paper is now cited in the revised version of our manuscript. This is indeed a very substantial and important addition because it links Horiba instruments (cross flow modulation technique) with gas filter correlation NDIR instruments. We were not aware of this work because the journal is not accessible through the Web of Science.

e.g., page2388, line 5: The authors state "natural air" in several places What does this exactly mean? - zero air purified from ambient air (not synthetic air)? Please clarify.

Standards in "natural air" are standards made from ambient air, which has further been purified or spiked with other gases to reach the desired mole fractions. It is now clarified in the manuscript that the standards were in ambient air.

Figure 6, section 3.2: The authors phrase "depletion". However, CO + OH is slow, unlike fast chemical titration of O3 with NO. Does it mean soil uptake? Or do the authors just technically phrase it? Please clarify.

It was a technical phrase. Lowest CO values are reached at JFJ when clean air with low CO mole fractions is advected to the site. This is the case for unpolluted air masses especially with an origin in latitudes further south. Because it is not depletion in a chemical sense we agree with referee 1 that the term "depletion" can lead to misleading conclusions. We show only pollution events in Figure 6 in the revised manuscript. The text was also adjusted accordingly.

page2395, line14: "the fit was close to linear..." In Figure 6 caption, the orange line is defined as the "linear" trend of the baseline data. Which is correct? Please clarify.

Function (1) was first used to fit the JFJ data. This resulted in a value of a_3 close to zero, which means that the trend part of the fitted function was close to linear. We then modified function (1) by setting $a_3=0$, which resulted in a linear trend. This is shown in Figure 6. We clarified in the revised manuscript with the following sentence: "Due to the small contribution of the non-linear term a_3 equation (1) was simplified by setting

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

$a_3=0$ to calculate the linear baseline CO trend at JFJ (orange line in Figure 6a)."

Figure 1, section 2.2.5: The gas standards are all balanced with natural air, except nitrogen for Horiba instrument. Why? Also I would like the authors to add if there is matrix gas effect with these four instruments.

There is no particular reason that the standard for the Horiba instrument standard was balanced with nitrogen. It was more historically that all NDIR CO measurements at JFJ were made using CO in nitrogen standards. There is also no matrix effect for this technique. However, the VURF technique requires a standard gas in natural air, because the UV fluorescence reaction is quenched by oxygen. We added a sentence in the experimental section to clarify this issue: "Note that the VURF instrument requires a standard in natural air because the UV fluorescence reaction is quenched by oxygen. No matrix effects for standards balanced with air or nitrogen are known for the other analytical techniques."

In my opinion, another important messages from the technical section of this paper is that in addition to GC method, NDIR method can be used for long-term trend analysis, if we make appropriate zeroing and use hourly means data. This could be more emphasized in the paper.

This is correct; it is an important message of the paper. However, we think that it is already enough emphasized in the manuscript by the following statements:

End of section 3.1: "In conclusion, one-hourly NDIR CO data of the JFJ station can be considered to be fully comparable to data obtained with a VURF instrument and are therefore suitable for trend analysis as presented in the following section."

Conclusions: "The inter-comparison demonstrated that the cross flow modulation NDIR technique provides reliable data on an hourly basis and is well suited for CO measurements even at remote sites; however, data with higher temporal resolution has to be interpreted with caution."

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Figure 7: The authors discuss interannual variations with this figure. However, the growth rate (or anomaly) can be more visible than simple comparison of the seasonal cycles. This can be easily done by looking at output of Figure 6. Please consider.

We added a growth rate curve in Figure 6 as suggest by referee 2 and discuss the inter-annual variability also with respect to this curve in the revised manuscript.

Figure 8: The authors suggest that the increasing Asian CO emissions may offset the CO trend in the FT over Europe, and that the modeled contribution maximizes during January-May. Does the CO trend at JFJ become further slower if only Jan-May data are plotted? I would encourage to see seasonal differences.

No significant seasonality in the JFJ CO trend was observed, as discussed on Page 2396. A slightly lower downward trend was only observed for the months of February and March, which lies within the period of largest contributions of Asian and North American anthropogenic CO as modeled by Pfister et al. (2004). We added the reference to Pfister et al. (2004) in the revised manuscript. However, a comparison of the emission inventories with different seasons would be difficult to interpret because the inventory data covers the whole year, and some of the emissions (e.g. heating) are not constant throughout the year.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2381, 2009.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)