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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of in situ measurements of atmospheric carbon monoxide at Mount Waliguan, China" *by* F. Zhang et al.

M. O. Andreae (Referee)

m.andreae@mpic.de

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In this paper, the authors present the results of three years of CO measurements at a remote mountain site in western China, Mt. Waliguan. The authors must be commended for the very careful calibration and quality control of their measurements, as well as the detailed discussion of the calibration and data analysis procedures. The instrument they are using, a gas chromatograph with mercury reduction detector, is inherently non-linear, but the authors have done a very good job addressing this problem. The paper can stand as an example for how such measurements should be conducted and described.

The results are discussed in terms of background and polluted conditions, with an objective statistical method used to distinguish the two. Of course, there is still some

subjectivity involved in choosing the discrimination criterion, and maybe this should be made more explicit in the revised text. Also, the term “background” is somewhat vague. When the airshed of a site contains numerous dispersed pollution sources, they would contribute to regionally elevated levels, which would be included in the “background”. It may be more meaningful to consider (1) a “global background”, represented for example by the CO concentrations observed at a comparable latitude over the remote ocean, (2) a “regional baseline” that corresponds approximately to what the authors call “background” and which contains the input from dispersed regional pollution sources, and (3) “pollution episodes” that show up as peaks above this baseline.

The paper presents a detailed statistical analysis of airmass trajectories using cluster analysis and potential source contribution functions, which identify regions from where the elevated concentrations of CO observed at Mt. Waliguan may have originated. However, they “validate” these results only by qualitative statements about the location of potential pollution sources, and by references to papers describing model results of long-range transport (Liang et al., 2004, cited on p. 1951). It would be desirable to see a comparison of the inferred source regions deduced in this paper with actual emission inventories.

Specific comments and technical corrections:

page 1941, line 23: It is not clear to what “These background conditions. . .”refers to, as the preceding sentences talks about elevated mixing ratios.

page 1947, line 11, and Figure 4: It is not clear to me why the deviations between the measurements at the two calibration scales show scatter and do not simply fall on a smooth curve representing the transfer function between calibrations.

page 1952, line 3ff: “Hence, because. . .”: This sentence contains a syntax error.

Figures 3 and 4: In Figure 3, the values of WLG – WMO bias are positive, while in Figure 4 the values of WMO – WLG bias are positive. This is in contradiction to a basic

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rule of arithmetic.

Figure 5: This figure is wrong. It is identical to Figure 3.

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