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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.

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Referee #1 M. O. Andreae m.andreae@mpic.de Received and published: 1 February 2011

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 in-

herently 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.

Re: A few sentences as follows have been added in the text to briefly describe the REBS approach: The REBS is a purely non-parametric technique that is used to follow any long-term trend and seasonal variation (Ruckstuhl et al. 2010). It assumes that the background signal varies very slowly relative to contributions of regional signal; the measurement errors are independent and Gaussian-distributed with mean 0. These assumptions are suitable to WLG where the variation of the baseline signal is on the order of weeks and the regional signal is hourly to daily. The measurement errors are expected to be random. (see Page 1947, line 22)

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.

Re: The reviewer makes a good point. In the Global Atmosphere Watch (GAW) monitoring network, the terms of "global baseline", "regional background" and "pollution episodes" were used and data selection procedures were developed accordingly. We consider that a "global baseline" could be represented by the CO concentrations ob-

served at comparable latitude over the remote ocean, or at a high altitude remote site (for example WLG at 3810m a.s.l in remote western China, most time capture the well-mixed air flow in the free troposphere). In this paper, although some pollution episodes were observed at WLG, the CO emissions are still very low in the west (see the INTEX-B results in the revised manuscript), the selected background data could well represent global background condition.

The paper presents a detailed statistical analysis of air mass 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.

Re: Total anthropogenic CO annual emissions for the region around WLG (including biofuels) based on the INTEX-B (Intercontinental Chemical Transport Experiment-Phase B, Zhang et al., 2009) has been added to the text, by comparison with the CO emission inventory (including biofuels) , it showed a relatively high emission in north-west Gansu but with much lower emissions in the west.

4. 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.

Re: "These background conditions were ...." has been changed to "Whereas, the background conditions were. . .."

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.

Re: This is because of few invalid data which have not been removed from all the data series. In the revised version, it has been removed.

page 1952, line 3: "Hence, because: : :": This sentence contains a syntax error.

Re: the sentence has been changed to "Because grid cells always have the same PSCF value regardless of whether CO mixing ratios slightly higher or much higher than the threshold, it is difficult to distinguish between relatively moderate and stronger CO sources."

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 rule of arithmetic.

Re: this is a writing mistake, the name of y axis in Figure has been changed to "Bias (WMO scale minus WLG scale, ppb)." (See Figure 3)

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

Re: the Figure 5 has been changed to the right one.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C2477/2011/acpd-11-C2477-2011-supplement.pdf

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