Atmos. Chem. Phys. Discuss., 4, S1737–S1740, 2004
www.atmos-chem-phys.org/acpd/4/S1737/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Increased Northern Hemispheric carbon monoxide burden in the troposphere in 2002 and 2003 detected from the ground and from space" *by* L. N. Yurganov et al.

Anonymous Referee #1

Received and published: 8 September 2004

The paper presents new atmospheric CO measurements in the HNH for the years 2002 - 2003, based on FTIR total column measurements, in-situ measurements and satellite observations (MOPITT). The paper extends an analysis for the years 1996-2001 presented in a recent paper [Yurganov et al., 2004] and shows that years 2002 and 2003 are characterized by relatively high CO mixing ratios in the HNH (as had been previously observed for the year 1998). The authors attribute these high CO mixing ratios to biomass burning and correlate their findings with measurements of fire pixels from ATSR. This is certainly an important result which is relevant for the monitoring and understanding of the global CO cycle.

However, the analysis presented in the paper appears very preliminary and needs

further elaboration:

(1) Introduction: The description of global inventories refers to one publication only [Holloway et al., 2000]. It would be more appropriate to include a wider range of studies, including IPCC assessments [IPCC, 2001] and recent inverse modelling based estimates. In particular, the statement "This (i.e. CO from biomass burning) is much larger than the global contribution from the combustion of fossil fuel (300 Tg / year)" is neither supported by the IPCC TAR values nor by most other studies.

As the presented manuscript is restricted to the HNH, also the corresponding HNH budget terms should be listed (for all major source categories).

(2) Spatial analysis: Unfortunately, data from MOPITT are presented only "integrated over the HNH". It would be very interesting to include a detailed spatial analysis of these data, in order to further track down the origin of the CO anomalies. Also for the fire pixels from MODIS only total values for the HNH are presented here. Again, a more detailed spatial analysis would be very valuable.

(3) in-situ measurements: It is somewhat disappointing that in situ measurements are presented only until end of year 2002 (although co-authors include NOAA/CMDL).

Furthermore, it would be interesting to show data from individual in-situ monitoring sites for further analysis of spatial patterns.

(4) Representativeness of monitoring sites: As the presented analysis is focusing on the total HNH the question arises how representative the available stations are. E.g. Figure 3 is summarizing under "TC, FTIR" "four low altitude stations". One of these 4 stations is Zvenigorod, for which the authors show that it has been effected by probably more regional fires.

It would be helpful to provide a map with the locations of monitoring sites.

(5) Intercomparability of measurements: Nothing is mentioned about calibration of CO measurements (for none of the three principal methods: in-situ, FTIR, satellite

ACPD

4, S1737–S1740, 2004

retrievals). Are e.g. all the in-situ measurements comparable with each other (as they have been made by different networks or institutes). Furthermore, the comparability of the 3 prinicpal methods needs to be discussed. Combining different measurement types in Figure 3 ("BL+FT": "BL network for lower 1.5 km, in situ data of six mountain stations, and two Alpine FTIR") seems problematic. Precision and accuracy should be listed for all methods.

(6) Temporal domain: Should be explained why just the March 2000 to February 2002 period is chosen as "reference period". Also potential long-term trends need to be discussed. E.g. at least at some sites in the HNH (e.g. Barrow) clear trends are visible over the last ˜15 years.

(7) Box Model: The applied 2-box model is very simple. In particular, it does not account for any inter-annual change of meteorology (TAU_trans) or OH (TAU_OH). However, due to the very strong CO gradient around 30 degrees latitude, any inter-annual variablity of TAU_trans would have a significant impact. This, and potential OH variability should be discussed. In general, however, a 2D or 3D CTM with analyzed meteorology for the target period of interest would be more appropriate than the presented 2-box model.

(8) Better quantification of results: A correlation plot between CO anomalies and fire pixel anomalies should be presented and correlation coefficients calculated. Also relative deviation of fire pixels would be an important information. Furthermore it would be helpful to quantify the major terms L_trans and L_chem (Tg CO/yr) used in the model.

References

Holloway, T., H. Levy II, and P. Kasibhatla, Global distribution of carbon monoxide, J. Geophys. Res., 105 (D10), 12123-12147, 2000.

IPCC, Climate Change 2001: The scientific basis, Cambridge University Press, 2001.

Yurganov, L.N., T. Blumenstock, E.I. Grechko, F. Hase, E.J. Hyer, E.S. Kasischke,

ACPD

4, S1737–S1740, 2004

M. Koike, Y. Kondo, I. Kramer, F.-Y. Leung, E. Mahieu, J. Mellqvist, J. Notholt, P.C. Novelli, C.P. Rinsland, H.E. Scheel, A. Schulz, A. Strandberg, R. Sussmann, H. Tanimoto, V. Velazco, R. Zander, and Y. Zhao, A Quantitative Assessment of the 1998 Carbon Monoxide Emission Anomaly in the Northern Hemisphere Based on Total Column and Surface Concentration Measurements, J. Geophys. Res., 109 (D15), doi:10.1029/2004JD004559, 2004.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 4999, 2004.