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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.

L. N. Yurganov et al.

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First of all we ought to express our appreciation to both anonymous reviewers for reading our paper and proposing several useful improvements. In what follows we reply to specific comments (the reviewers' remarks are in italic).

Response to the Anonymous Referee 1.

Our replies to the particular remarks.

(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 stud-

ies, 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).

The global and semi-hemispheric inventories in the Introduction section were updated and revised. The IPCC TAR values were included (Ehhalt et al., 2001), and new estimates for the industrial CO input in the HNH were given.

(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.

MOPITT and ATSR (AATSR) data were averaged (integrated) over 3 areas (Europe, Siberia and North America) and presented as figures 5 and 6.

(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.

Surface measurements were extended until the end of 2003. For consistency, only the data covering the entire period until December 2003 were included in the calculations of the semi-hemispheric burden anomaly. The examples of these data are displayed in Figure 3. The emission anomaly estimates were re-assessed after inclusion of the 2003 surface data.

(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.

The map of measurement sites is presented as Figure 1. A representativeness of total column monitoring sites is a weak point of the total column data set; all of the locations are in Europe. To improve the situation we consider two other sources of information. They have advantages and shortcomings: surface sites are scattered more over the Globe, but do not give any information about the free troposphere. MOPITT has ideal coverage, but its sensitivity to the boundary layer is low (Deeter et al., 2004). We expect that a combination of all three data sets may improve the final results.

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

Concerning the problems of accuracy, comparability and calibration. As one can notice, we deal with anomalies. As a rule, systematic errors for different methods, algorithms, measurement techniques, etc., are especially difficult to quantify. Moreover, different sites have their own seasonal patterns, depending on geographical location and other factors. A consideration of anomalies in contrast to the absolute values allow one to resolve (or at least to minimize) both problems. However, we agree that this is important and we added information about precisions, calibrations, comparisons, and references to publications where these issues are described in more details.

(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 Ÿ15 years.

The reasons for choosing the reference period are explained in the submitted ACP paper. The trend issues are not discussed here because of a relatively short period in consideration and large interannual variations.

(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 TAUtrans 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.

We agree that a 3D CTM with real meteorology would be better than the presented 2-box model for the 1998 meteorology. This would make possible to improve the accuracy of the estimates comparing to present 30

(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 Ltrans and Lchem (Tg CO/yr) used in the model.

Correlation plots are presented in Figure 8 and correlation coefficients are calculated. Ltrans and Lchem are presented in the Supplement in a tabular form.

Response to the Anonymous Referee 2.

Specific comments:

Page 5002: A map (additional to Table 1) with the locations of all measuring stations *would be helpful*

A map is added (Figure 1)

Page 5004: In September 2002 AOD and CO observations from MODIS were enhanced due to forest fires. For 2003 the authors state "forest fires started burning in May". Was this determined by MODIS data as well? If so, it would be good to say this in the paper.

The hot spot number in May, June, and July, 2003 was abnormally high, especially in Siberia (Figure 5). This is discussed in the new version of the paper.

As already pointed out by referee No. 1 an effort should be made to include 2003 in situ data.

The 2003 in situ data are included.

Page 5005: The process how CO burden anomalies are derived needs to be described clearer (it is much better described in the JGR paper). It is also not obvious to me why the stations were grouped in TC (total column stations at low altitudes) and BL (in situ boundary layer stations) combined with FT (free troposphere in situ AND column).

The forest fires emissions enhance CO concentrations both in the boundary layer and in the atmospheric total column, but these effects are different in magnitudes. This is now discussed in the paper. This is the reason for a separate calculation of the semi-hemispheric burden anomaly. Low altitude total column sites measure the anomaly in both boundary layer and free troposphere. CMDL stations give information about pollution of the BL. The information about free troposphere is now available only from FTIR measurements at Alpine stations (supplemented by the in-situ measurements at Zugspitze).

Page 5006: Using GEOS-CHEM with 1998 meteorology. Though I understand the necessity to use sometimes meteorology from previous years for modelling, in this case when anomalies and interannual variability are discussed, an assessment of the

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influence of using older meteorology should be made at least.

The issue of possible errors due to interannual variations of both chemical and transport CO sinks was considered in more details by Yurganov et al. (2004).

Page 5007: ATSR fire counts. In order to explain anomalies in 1999, 2000, and 2001 the authors suggest that the varying nature of wildfires could cause differences in CO emission factors. This sounds a bit 'hand waving'. Is there any way to support this argument by for example looking into soil type of the burning regions or difference in CO emissions from different fire types?

This a good point, but a little bit going outside of the frames of this paper. It is one of the focuses of the recent paper by Kasischke et al (2005) that is accepted for publication at GBC (we were aware of preliminary versions of this paper, it has been under preparation during last two years). Really, not only total burned area (or number of fires) is important but also where these fires do happen and what is the type of this fire. Siberian fires seem to supply more pollutants per unit area than the North American fires (Kasischke et al., 2005). Some discussion of this issue is included in the paper. Also see our Figure 8.

"normal" emission seasonal cycle. The authors put 'normal' in quotation; however, in a scientific journal it would be better to describe the used emissions instead of assuming that 'normal' can be understood by everybody. 'Normal' is also used further down on the page and in the figure caption of figure 4.

This is a matter of definition. Is the low fire activity normal and high fire activity abnormal? If the strong fires would repeat every year, then high emission would become "normal" and a low emission would be "abnormally low". That is why we use a quotation for this word. It is just important to define clearly what we mean under "normal". We assume that it is done in the current version of the paper.

The conclusions to this paper are a bit thin. The authors end with 'that it is most likely *that wildfires are responsible for the CO build up'. A closer look and quantifications of the areas burned, type and timing of fires as they suggested in their JGR paper would be desirable here.*

It may be desirable for a reader. Unfortunately, the experimental material and modeling tools do not allow a study of the mentioned points seriously. Any discussion of these matters in frame of this paper would be not enough convincing. However, the top-down estimate of CO burden imbalance (keeping in mind a decisive role of the source variations, esp. fire emissions) is valuable itself. A reader may be addressed to the papers of Kasischke at al, who is the leader in this field.

Table 3: 'Accuracy' means here the standard deviation between the two and respectively 3 numbers. I would prefer either stdev or variability

The accuracy given here is based mainly on sensitivity tests carried out for the model by Yurganov et al. (2004).

Figure 1: A uniform scale of the x and y-axis would make comparison easier. Since only the data between 2000 and 2004 is discussed the authors might want to consider if they want to display only this time period.

We tried to keep uniform scales in the new version of the Figure. A retrospective look at the absolute variations before the period of consideration seems to be useful.

(3) Technical corrections: Page 5004: Instead of "Specifically, there were 1 (12), 1 (6), and 7 (10) such days in July, August, and September, respectively (numbers in brackets indicate the total observational days for each month)." Maybe something like: there were 1 out of 12 days in July, 1 out of 6 days in August and 7 out of 10 days in September that exceeded the summertime 2002 daily values.

Thank you, we accept your version.

the paper Edward et al. (1994) is cited but is not listed in the references

Sorry, it was a misprint.

Page 2006: DeMore et al. 1997

Corrected.

Page 5008: Detter et al., comma in front of journal title

Corrected.

Page 5009: Edwards et al., 2004 cite with doi number and without acceptance date

This paper was accepted on June 11, 2004, now it is on the proof-reading stage.

Page 5010: Zhao et al., remove one comma after Strong

Corrected

Page 5016: Figure 3, it would help to print 'top panel, left axis' and 'middle panel, right axis' and 'bottom panel, left axis' in bold or underlined in order to help the reader

Corrected.

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