Dear Editor,
Please find our responses to referee #2 embedded below in *blue italics.*

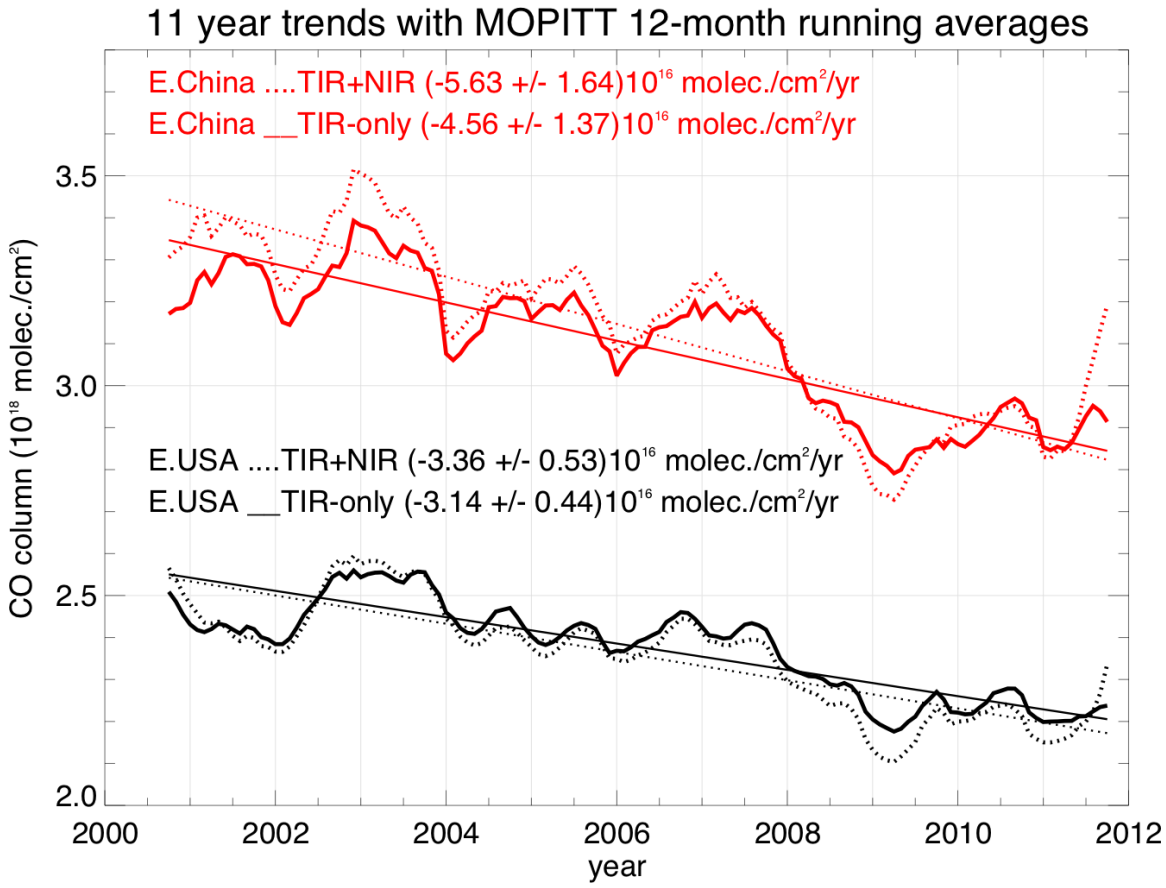
Anonymous Referee #2
Received and published: 3 October 2012

This paper by Worden et al. presents an examination of CO trends in the Northern and Southern Hemispheres along with regional trends for Eastern China, Eastern USA, Europe and India. It is interesting to see the features from different instruments and it is the first time to my knowledge that a decrease in CO is observed in Eastern China. Nevertheless, this work, while of his importance, could be viewed as an update of trend studies observed by satellite instrument as performed by Yurganov et al. (2006; 2010) for CO. I recommend that this paper as long as some points, outlined below, are addressed.
Thank you for reviewing our manuscript. We agree that this work builds on previous studies by Yurganov et al., but we decided it was worthwhile to update the MOPITT results with the most recent data version (V5) that accounts for time-dependent instrument parameters and to present a comprehensive view of all available TIR satellite measurements of CO total column.

p25706, line 13: Jones et al., 2007 or 2009 as in the reference?
Thank you for catching that – should be 2009 (fixed).

p25708, line 25: You start your study saying that you did not use the product combining TIR and NIR channels because the results you got are very similar to the TIR-only results. I am curious to see these results because I know that in your previous study (2010) over China and the paper over US by M.N Deeter (2012) presented interesting results with a larger sensitivity close to the surface. You probably do not need to add these results in the manuscript but I will be interested to see them.

The focus of this study was to present the total column CO data records from all the TIR instruments, so we did not include the MOPITT TIR-NIR V5 product. Since this is of interest, we show below the comparison of MOPITT V5 TIR-only (solid lines) and TIR-NIR (dashed lines) CO column for the E. China and E. USA regional averages. Note that the MOPITT TIR-NIR total columns show a larger trend (especially in China) but are still consistent with the TIR-only results.



p25710, line 23: add "at nadir" after 12 km diameter. *(done).*

p25712, line 7: I do not understand why in one case you used a criteria of DFS>0.75 and for another one with DFS>1. In my opinion I will only use data with a minimum of one piece of information.
The criteria for minimum DFS, although always somewhat arbitrary, was chosen differently for the hemispherical averages (mostly ocean) than for the data over land in order to retain the most uniform sampling of all latitudes from 0-60°, while still rejecting those retrievals with a significant influence from the prior. This effect was studied by comparing distributions for a range of DFS criteria in the hemispherical averages. DFS > 0.75 provided the best compromise. We have added this explanation to the text.

p25713, line 1: As previously, why DFS>0.9 and not 1 for example?
For the limited sampling of TES data, criteria are always chosen to retain as many observations as possible while still filtering out unsuitable retrievals.

p25714, line 1: You explained the biases with AIRS but I also see a bias between MOPITT and TES on 2005, 2007 in the NH, or on 2010 between MOPITT and IASI in the SH, and you did not mention them. Why are there these differences?
These biases due to spatial sampling differences were discussed generally in Section 4.1, but this did not explain the specific differences noted by the referee. We will revise this discussion to be more explicit and move it to section 3. In particular, the difference for

Oct. 2010 in the SH for IASI vs. MOPITT is due to more frequent sampling by IASI of intense Africa biomass burning. Looking at individual days for this month, MOPITT typically samples only ~ ½ the area in southern Africa where high values of column CO are observed by IASI.

p25716, line 19: You just mentioned Fig 9 but please give a comment and explain it. Moreover, it will be easier to read the figure if you use the same color scale on both plots and add ticks on x axis. For example on the upper plot I do not know if you showed the MOPITT data for 2012.
This figure will be revised as suggested. We have added the following description to the text:
"As further verification of the consistency in the MOPITT and AIRS datasets for detecting patterns in interannual variability, Figure 9 shows a zonal average time record of normalized CO total column. 12-month running averages of MOPITT and AIRS CO total columns in 10° latitude bands are normalized to the time average for each band. The variations in CO for both instruments are very similar in time and latitude, and both observe a deep minimum in 2009-2010."

p25718, line 6: change 2007 by 2008 in the sentence ": : :in global fire emissions from 2007 to 2009: : :"
We are not certain what the referee intended here. We have checked the van der Werf et al., 2010 reference again, and figure 8 clearly shows a decline in global fire emissions that starts in 2007.

p25718, line 1: "This will require data assimilation and inversion with a model that controls for emissions and OH." I think you should mention the study by Klonecki et al. ACP (2012) where they assimilated IASI CO total columns in a global model. I think they showed the influence of this assimilation on OH distribution. In the MACC project they probably have already studied this impact.
Thank you for this reference – it is good example of data assimilation with IASI CO, but it doesn't look like they performed an inversion for CO emissions, and we could not find how the OH distribution was affected. However, this paper was relevant to IASI validation and we include it as a reference for that.

p25718, line 4: Hooghiemstra et al., 2012 and not 2011. *(fixed).*

p25729 Tab2: It would be better to also add the values for the bias and RMS used for IASI and not only write "see text". *These are now included in the Table 2 footnotes, as well as the text.*