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Interactive comment on “Decadal record of satellite carbon monoxide observations” by H. M. Worden et al.

Anonymous Referee #2

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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.

p25706, line 13: Jones et al., 2007 or 2009 as in the reference?

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

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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.

p25710, line 23: add “at nadir” after 12 km diameter.

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.

p25713, line 1: As previously, why $DFS > 0.9$ and not 1 for example?

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?

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.

p25718, line 6: change 2007 by 2008 in the sentence “. . .in global fire emissions from 2007 to 2009. . .”

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.

p25718, line 4: Hooghiemstra et al., 2012 and not 2011.

p25729 Tab2: It would be better to also add the values for the bias and RMS used for

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IASI and not only write “see text”.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 25703, 2012.

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