**Atmospheric
Chemistry
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Discussions

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Interactive comment on "Technical Note: Temporal change in averaging kernels as a source of uncertainty in trend estimates of carbon monoxide retrieved from MOPITT" *by* J. Yoon et al.

Anonymous Referee #1

This paper faces a very important issue in the trend estimation of atmospheric trace gases from remote sensing measurements: the estimation of the uncertainty due to the temporal change of the averaging kernels. This problem is generally neglected in studies of trend, instead this paper shows that the variability of the averaging kernels can introduce a significant error in the results. Therefore, this paper surely deserves the publication on ACP. However, I think that the procedure used by the authors to estimate the error due to temporal variation of the AKs on CO trends measured by MOPITT is not clearly described. I think that the authors should explain more in detail how the linear trends reported in Fig. 5 have been calculated. See below the specific

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comment on this point.

Specific comments

P.20321, l. 26: I suggest to change "uncertainty" in "error".

P.20321, l. 26: I suggest to delete "using AKs" (it seems that the trend estimation somehow uses the AKs, I think that the authors refer to the error on the trends when the AKs are not taken into account).

p. 20322, l.26: "temporal resolution: 1 month" seems referred to Fig.1, that is an average from 2001 to 2010 (so it cannot have a temporal resolution of one month). Probably the distributions that have been averaged have a temporal resolution of one month. I suggest to specify this aspect.

p. 20325, Eq.(3): $dx_0/dt(I-A)$ has be to replaced by $(I-A)dx_0/dt$.

p. 20326, l. 15-17: the authors should specify more in detail the procedure of how the linear trends reported in Fig. 5 have been calculated. They say that these trends have been calculated making the assumption that the true state is 50% more (or less) than the a priori state. Did they simulate the MOPITT measurements with this assumption and they calculated the trends of the surface CO retrieved by the simulated measurements? Or did they somehow use Eq. (4) to calculate these trends?

p. 20327, l. 18: I suggest to specify that "the anomaly of surface a priori CO" is the difference between the a priori and the seasonal mean.

p. 20328, l. 9-12: The sentence "However, since at higher altitudes .....than for the near-surface layers" is not clear. The uncertainty caused by time varying AKs is generated by the temporal variation of the AKs and by the difference between the true and a priori states. How the two points ((a) and (b)) determine smaller uncertainties at higher altitudes?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 20319, 2013.

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