Interactive comment on "Decadal trends in global CO emissions as seen by MOPITT" by Y. Yin et al.

Reply to Referee #2

We thank the reviewer's appreciation of our work and the thoughtful comments. We have made corresponding efforts to revise the manuscript. The full review is copied hereafter and our responses are inserted where appropriate.

Anonymous Referee #2

Yin et al. use MOPITT version 6 CO retrievals along with surface measurements of methyl-chloroform and methane to perform an atmospheric inversion over 2002-2011. They then use results from the inversion to analyze trend in CO emissions and burdens globally and regionally.

This paper provides an important contribution to atmospheric composition science and should be published in ACP. The methods used are well thought-out, and the paper is generally well written. I have only minor concerns, presented below.

General Comments

1. There are a LOT of numbers in the text, especially in Sect. 3.2 and 5.1. It's very hard to keep track of these and to compare their importance. I think the paper would be well served by including a table with global and regional trends from MOPITT, surface stations and the model sampled like each of these (Sect. 3.2) and another that gives the modeled changes to budget terms (Sect. 5.1). Also, it would be nice to add prior emission (& chemical source/sink) trends to Table 1, since these are perhaps more relevant that the mean amounts and provide context.

Thanks for the suggestion. The text was indeed a bit heavy, we have revised Sect. 3.2 in two ways: adding one table (Table 3 inserted below) to summarize the trends in different regions, and changing the unit for MOPITT retrievals from molec/cm2 into mole fractions with numbers more friendly. The entire paragraph is revised accordingly.

For the budgets (Set. 5.1), we updated both Fig. 8 for the global budgets and Fig. 10 for regional emissions in a way that the trends in the prior and posterior are more clearly denoted when the trends are statistically significant. These two revised figures are inserted below.

Table 3. Summary of CO model-data comparision and trend analysis for MOPITT satellite retrievals, surface station observations and corresponding prior/posterior modelling. Trends for each region (in the unit of ppb yr^{-1}) are the mean values for all the grids whose trends are significant at 95% confidence level. The percentages of significant trends are also given per model grid for positive (+) and negative (-) respectively.

REGIONS	MOPITT		Surface		MOPITT column trends										Surface station trends									
	Prior Bias	Post Bias	Prior Bias	Post Bias		Observation			Prior mod			Posterior mod				Observation			Prior mod			Posterior mod		
	(ppb)				*N*	Trend	+ %	- %	Trend	+ %	- %	Trend	+ %	- %	*N*	Trend	+ %	- %	Trend	+ %	- %	Trend	+ %	- %
BONA	-14.1	0.7	-20.8	20.5	*210*	-0.84		*99*	-0.46		*95*	-0.88		*99*	*2*	-2.51		*100*	-1.46		*100*	-2.96		*100*
USA	-16.7	-3.1	-20.4	20.1	*108*	-0.82		*97*	-0.41		*96*	-1.08		*100*	*3*	-1.67		*67*	-1.13		*100*	-3.30		*100*
NHSA	-10.0	-3.0			*74*	-0.61		*84*	-0.19	***8***	*53*	-0.68		*100*	*0*									
SHSA	-14.2	0.2			*160*	-0.59		*58*	0.38	***41***		-0.56		*67*	*0*									
NHAF	-15.0	-2.0	-20.0	6.8	*211*	-0.45	***7***	*55*	-0.38		*85*	-0.73		*95*	*1*	-0.90		*100*	-0.73		*100*	-1.04		*100*
SHAF	-16.5	0.5	-1.5	13.6	*96*	-0.57		*75*	0.07	***27***	*11*	-0.64		*96*	*1*	-0.78		*100*	0.42	***100***		-0.48		*100*
WSEU	-16.1	0.3	-36.7	18.7	*106*	-1.00		*100*	-0.49		*100*	-1.12		*100*	*6*	-2.73		*100*	-2.05		*100*	-3.51		*100*
ESEU	-16.8	0.4			*108*	-0.77		*100*	-0.40		*100*	-0.93		*100*	*0*									
BOAS	-17.3	1.0			*227*	-0.92		*99*	-0.51		*92*	-1.02		*99*	*0*									
MIDE	-16.5	-2.9			*64*	-0.57		*100*	-0.30		*100*	-0.88		*100*	*0*									
SCAS	-12.0	-0.3			*80*	-0.65		*63*	-0.30	***4***	*38*	-0.92		*100*	*0*									
SEAS	-20.1	-3.6	-30.6	22.6	*129*	-1.23		*97*	0.13	***19***	*24*	-1.35		*99*	*3*	-1.76		*100*	-1.83		*100*	-3.78		*100*
AUST	-15.5	-1.7	-6.4	15.4	*105*	-0.62		*100*	0.17	***28***		-0.78		*100*	*3*	-0.34		*67*	--			-1.18		*67*
INDO	-3.8	0.2			*64*	-1.20		*98*	-0.84		*28*	-1.03		*98*	*0*									
OCEAN	-11.9	-0.2	-15.1	9.6	*3092*	-0.72		*96*	-0.07	***22***	*25*	-0.67		*97*	*27*	-1.23	***4***	*89*	-1.00	***7***	*59*	-1.46		*89*

Fig. 8. Time series of global mean annual CO budget changes from 2002 to 2011. Each component is shown in different colour. Solid lines indicate the prior (mean values of the two OH fields are shown for the prior chemical CO production and sink). Dash-dot line represents posterior with TransCom-OH and dotted line represents posterior with INCA-OH. If the trend is statistically significant, linear slope is denoted beside each line. With the order from left to right, the numbers represent slopes for the prior, the posterior with TransCom-OH and the posterior with INCA-OH successively. * denotes significant at 95% confidence level, and ** denotes significant at 99% confidence level.

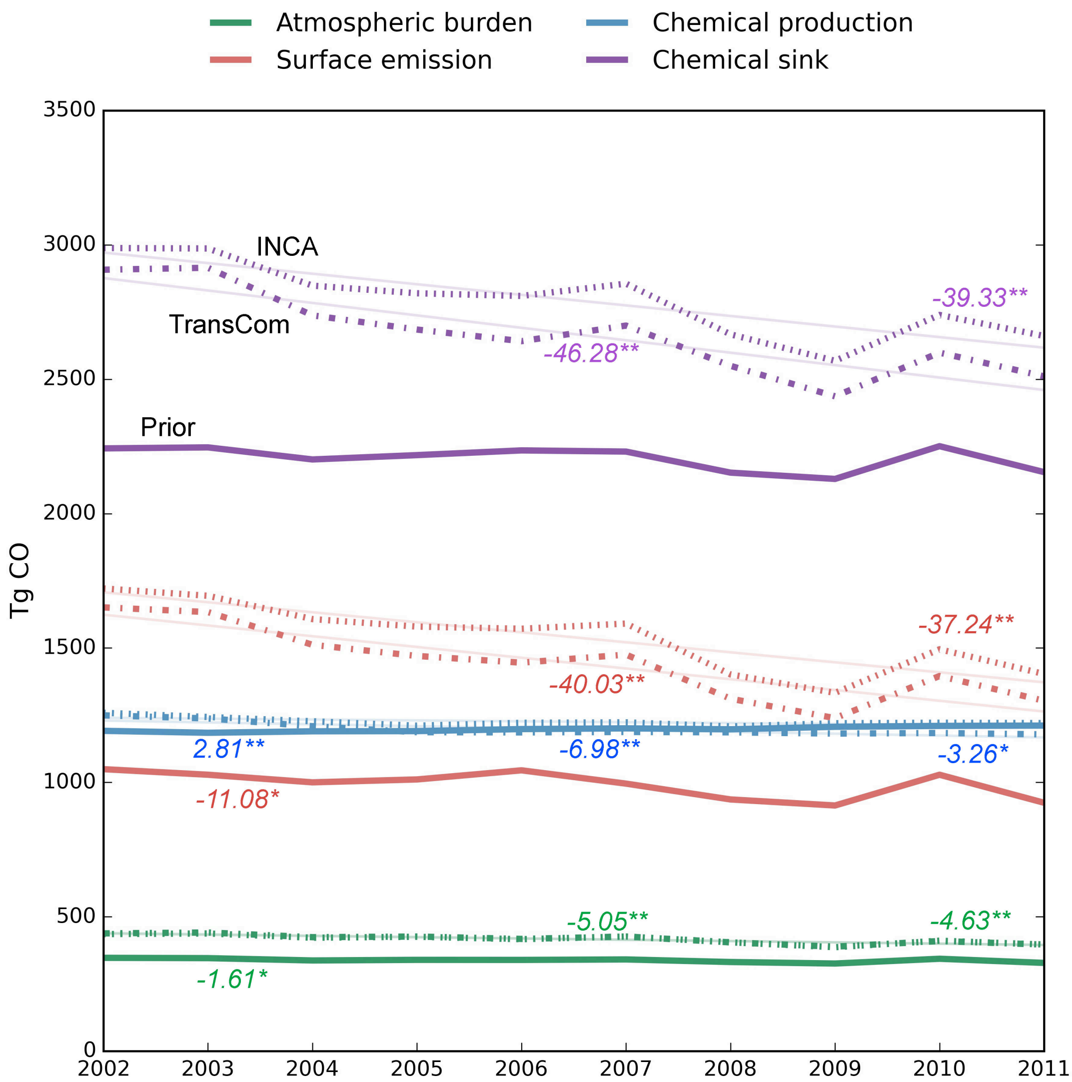
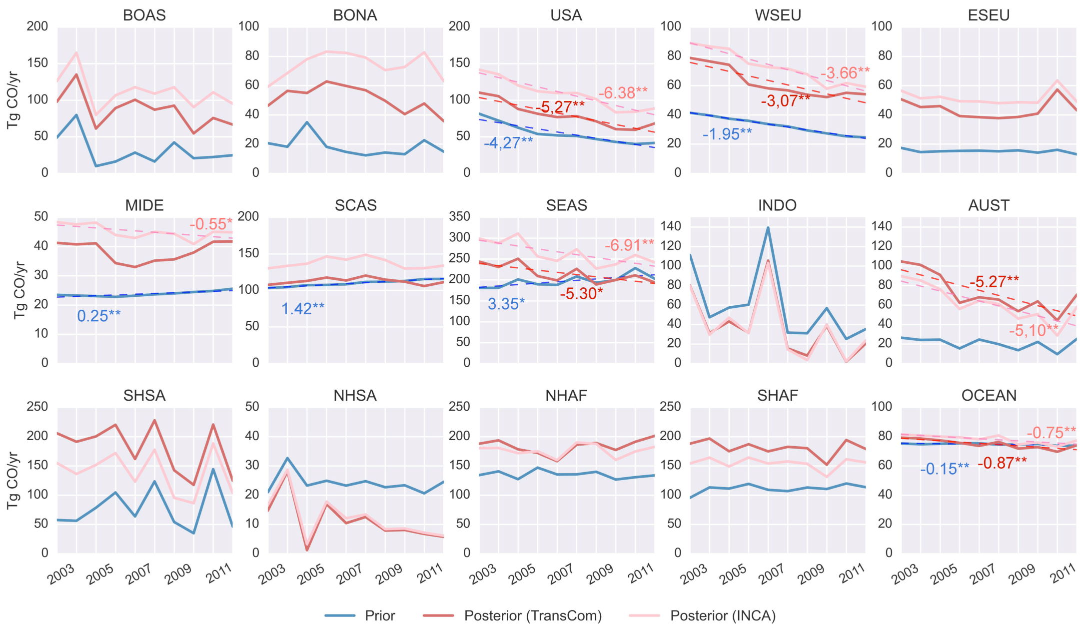


Fig. 10. Regional annual CO emissions from 2002 to 2011. The prior emissions are shown in blue and the posteriors are shown in red. Dark-red represents posterior with TransCom-OH and pink-red represents posterior with INCA-OH. Linear fit is shown when statistically significant, and slopes are denoted with corresponding colour. * denotes significant at 95% confidence level, and ** denotes significant at 99% confidence level. The notations for the sub-regions are listed below, and the extent of each region is shown in Fig. S1. BOAS - Boreal Asia, BONA - Boreal North America, USA - USA, WSEU – West Europe, ESEU – East Europe, MIDE – Middle East, SCAS – South Central Asia, SEAS – South East Asia, INDO- Indonesia, AUST – Australia, NHSA- North Hemisphere South America, SHSA - South Hemisphere South America, NHAF - North Hemisphere Africa, SHAF – South Hemisphere Africa, OCEAN – all ocean emissions.



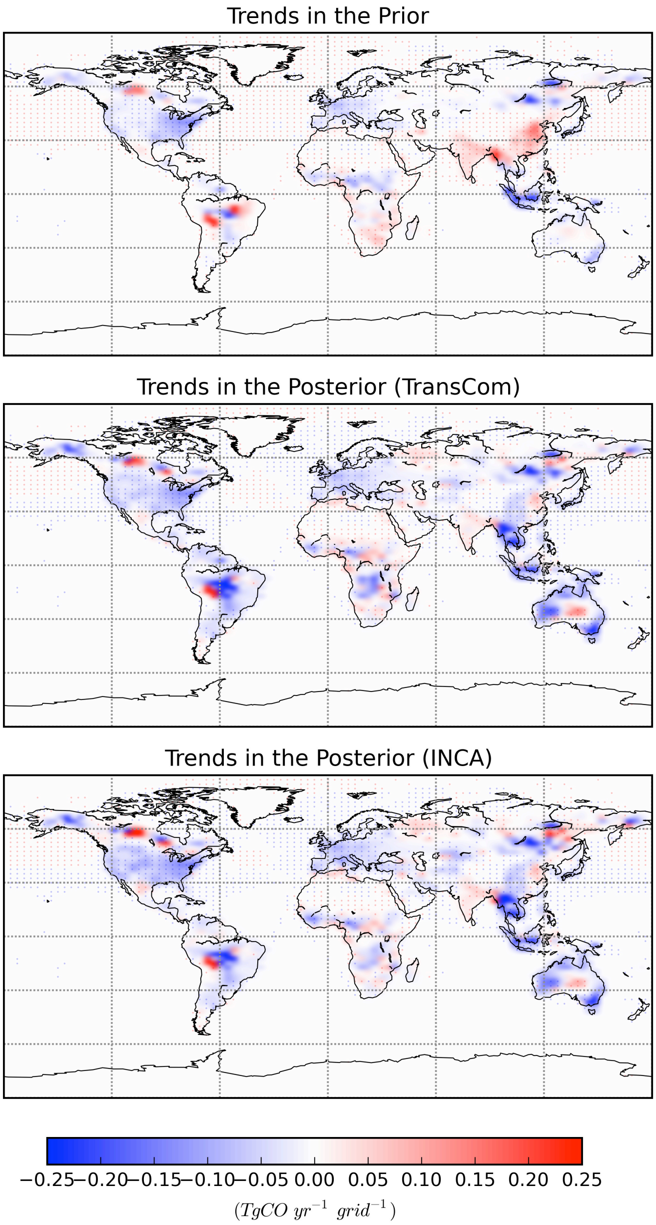
2. There is a large discussion around regional trends, but many of the trends discussed in the text are not in fact significant according to the figures (some are highlighted in the next section). This needs to be made clearer in the text, and I would suggest spending more time on the trends that are in fact significant. For example, one region that is not really discussed but appears interesting is AUST: the prior suggests no trend, but the posterior suggests a very strong negative trend. AUST is referenced briefly in the section on BB, but from comparing Figs. 2b and 9, it looks like the trend is partly driven by locations where BB doesn't dominate. More discussion would be worthwhile.

This confusion carries through to the figures. For example, Fig. 10 includes a lot of trend lines for trends that are highly insignificant, and these are misleading. I would suggest that Figs. 8 and 10 should only show trend lines when the trends are signif- icant. It may also be worth highlighting these by e.g. making the s & p values bold for significant trends, or adding an asterisk. Another option is to put all the values in a table, including their significance, and only include on the figure slopes where the trend is significant.

We thank the reviewer for pointing this out. We have revised this issue in two aspects. First, we have re-run the entire assimilation using improved segmentation methods. In the previous version, in order to save computational time, we did split the period into segments of 14 months with a 2-month overlap between adjacent years. We thought that this approach would not influence the inverted flux trends under the assumption that the initial conditions for each segment are properly optimized by the system with just 2 months of overlap, a spin-up time that is about the lifetime of CO in the atmosphere. Under closer scrutiny, it appears that this segmentation actually generates a small discontinuity in the concentration time series that affects some regional signals. Therefore, we have re-run the inversion with only three segments and with an overlap of 4 months between adjacent segments with initial conditions optimized upstream. The main conclusions are not affected, but all figures will be updated. Some regional trends are slightly changed as shown in Fig. 9 inserted below.

Second, we have improved the notation of regression slope and significance for Fig. 8 (on page 2 of this reply) and Fig. 10 (on page 3 of this reply) as shown above. The discussions about trends are more focused on the significant ones according to the updated results. The negative trend in the posterior emission in SEAS is in fact significant, and we added discussion on the trends in AUST, which is probably explained by the change in burned area as indicated by the newly released GFED4s dataset.

Fig. 9. Trend distributions of CO surface emissions from 2002 to 2011 (a) in the prior, (b) in the posterior with TransCom-OH and (c) in the posterior with INCA-OH.



3. Most of the figures are not colorblind-friendly. Figures 3, 4, 6, 7, 8 and 9 all use red and green to contrast two different simulations, which many people cannot inter- pret (see e.g. http://www.somersault1824.com/tips-for-designing-scientific-figures-for-color-blind-readers/). An easy solution would be to change all use of green in these figures to blue.

Thanks very much for this reminder. We have updated all the figures systematically with color schemes of red and blue.

Specific Comments (page, line)

14506, 24-28: This discussion centers around one of the insignificant trends; given this I'm not sure it belongs in the abstract (at least not without explicitly stating that it is insignificant).

In the previous version, China was mentioned particularly in the abstract (for which the trend was significant), but the regional emission was shown for the entire SEAS in Fig. 10, which was a bit misleading. Nevertheless, in the revised results, in which the trends are more robust with the improved segmentation method, the trends in SEAS are indeed significant, so are the trends cover China as shown in Fig. 9. Therefore, we are actually discussing a significant trend.

14507, 22-25: Warner et al. 2013 (doi:10.5194/acp-13-12469-2013âĂ˘l) also evaluated CO trends from satellite, and should be cited here.

Thanks for mentioning this study. In the previous version, we cited only the Worden et al. 2013 paper, which synthesized 4 satellite retrievals, and was therefore considered representative of recent trends in satellite observations. The Warner et al. 2013 paper has been added in the revised paper.

14511, 14-18: I am confused by the treatment of CH2O here. What happens to surface emissions of CH2O? Are these included in the CH2O production term? Please clarify.

There are direct CH_2O emissions (as shown in Figure 1) in our model both from biogenic and biomass burning emissions in addition to the CH2O chemical productions from VOC oxidation. The surface emissions are optimized together with the total quantity VOC emissions that will turn into CH_2O (there is a single scaling factor for both). We have added this description in the paper.

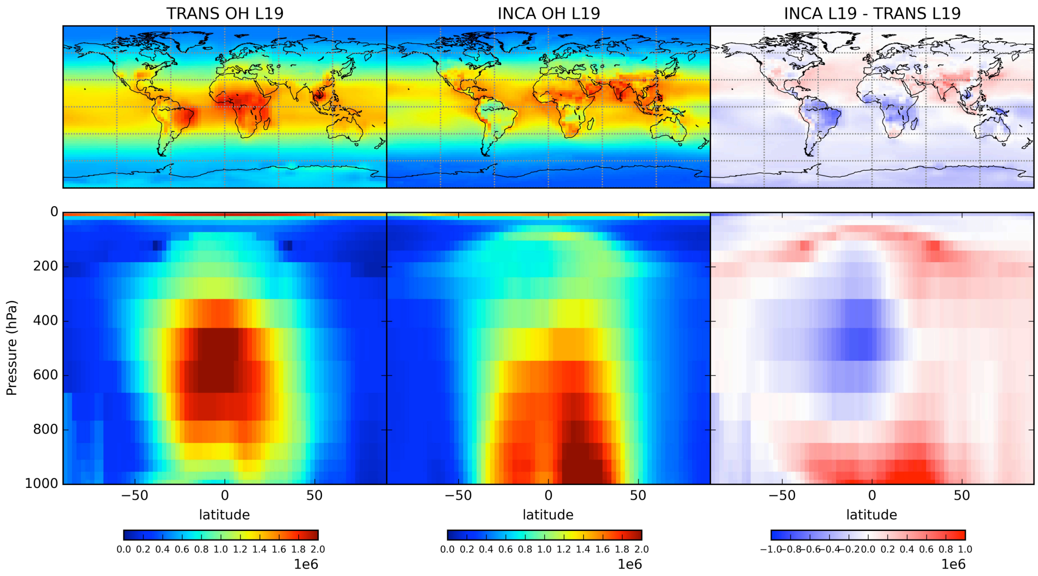
14511, 18-23: I would like to see more justification for splitting OH longitudinally only in the NH. I would expect the active biogenic VOC chemistry in the SH (e.g. the Amazon) to have a large impact on OH that is not longitudinally homogeneous. Is this split neglected because of a lack of SH data constraints, or for some other reason?

Yes, further splitting of the other three latitudinal bands is limited by the number of surface stations. We have added this information in the text.

14512-14513: It would be nice to see a figure showing both OH fields to clarify this discussion. Altitude-latitude zonal mean cross sections would be ideal.

We have added one figure showing both OH fields as well as differences between the two. Fig. 3 in the new version is inserted below.

Fig. 3. Spatial and vertical distribution of OH concentration in TransCom, INCA and differences between the two. The TransCom OH is interpolated from its original 60 pressure levels into the LMDz 19 eta-pressure levels.



14513, 25-26: Does the uncertainty on CH2O production also include uncertainty on its emission?

Yes. As stated above for 14511, 14-18, the surface CH_2O emissions and chemical productions of CH_2O from VOC emissions are optimized together. There is a single scaling factor for both, so are the uncertainty statistics.

14513, 26: Where are these "initial concentrations" from?

They are simulated from LMDz-INCA full chemistry model and optimized upstream in the revised version. We have added this information in the text.

14515, 4-7: Are there any references for suggesting that some of these observations "may be of lower quality or more difficult to measure?" It would be nice to back this statement up if possible.

The reference is added. Fortems-Cheiney et al., 2012 has explained in details about the observation selection.

14515, 25-28: It's not clear to me what is meant by "yearly means of synoptic variability" – how was this calculated?

It is calculated as the standard deviation of the residual of the detrended and deseasonalized data. We have added this information in the revised paper.

14516, 6-14: Is this a gridded OMI product? If so, what resolution? Do you use the vertical columns here, and are these provided in the product or calculated from slant columns?

We used the OMI CH_2O gridded data with pixel size between 13×24 km^2 at nadir and 26×135 km^2 at the swath edges, providing daily global coverage. We used the vertical columns (provided by the product) without convolution of averaging kernel (not provided by the product). This dataset is available from the following site: http://disc.sci.gsfc.nasa.gov/Aura/data-holdings/OMI/index.shtml. We have added those details in the revised version.

14516, 18: Most of the calculated values shown later include error bars, and trends include significance. This would be a good place to explain how those are calculated.

It follows the widely used statistical approach in estimating the uncertainties of the linear regressions slope. Here we used the stat.linregress function in the scipy package in python2.7. We did not give the details in the method section, as it is not some method unique in our study.

14517, 18-21: It would be nice to include a statistical measure of goodness of fit, especially for the CH4 and MCF which aren't shown and therefore makes me wonder how accurate the claim that they fit "fairly well" is.

Thanks for this suggestion. We have added a table to show the goodness of fit as Table 2 inserted below. The substantial decrease of both the mean bias and root mean squares (RMS) reveals the system ability to draw information from the CH4 and MCF observations.

Table 2. Fitness of CH_4 and MCF observations assimilated in the inversion.

Region	OH-type	CH_4 (ppb)				MCF (ppt)			
		Mean bias		RMS		Mean bias		RMS	
		prior	*posterior*	*prior*	*posterior*	*prior*	*posterior*	*prior*	*posterior*
NH(30-90)	*TransCom*	20.6	2.4	749.6	22.2	1.02	-0.02	1.14	0.05
	INCA	-21.0	2.5	658.1	20.1	0.44	-0.09	0.28	0.07
NH(0-30)	*TransCom*	15.8	1.5	452.6	19.1	0.90	-0.20	0.91	0.16
	INCA	-21.1	-0.1	564.9	13.9	0.40	-0.20	0.21	0.11
SH(0-30)	*TransCom*	10.9	-2.4	222.1	20.1	1.19	0.09	1.61	0.13
	INCA	-14.3	-4.0	264.9	29.5	0.88	0.23	0.94	0.15
SH(30-90)	*TransCom*	14.4	0.2	308.3	4.7	0.88	-0.24	0.90	0.17
	INCA	-7.9	-0.4	96.6	5.7	0.60	-0.06	0.46	0.07

14518, 3-9: Could there be any influence from different amount of land vs. ocean in NH/SH, and different sensitivities of MOPITT over these surfaces? Warner et al. (2013) discuss hemispheric ocean/land differences in AIRS.

We did not test the sensitivity of MOPITT over land vs. ocean, it is not the main focus of this study and we cannot cover every aspect without diverging too much.

14518, 9: Fisher et al. 2015 (doi:10.5194/acp-15-3217-2015) also discuss problems in simulating CO vertical profiles in CTMs, specifically in the SH, and could be cited here.

Thanks for the suggestion; we have added the reference here in the revised paper.

14518, 27: Are these exceptions significant? It's impossible to tell from the small figure.

We have updated the figures and plot only significant trends as shown above in Fig. 10. If significant, the slopes are denoted beside the liner fit with * indicating confidence level of 95% and ** for 99%.

> *14519, 5: Again, a statistical measure of fit would be nice – it looks like the inversion gets worse relative to the surface stations, but I think that's just because there are some non-representative large values that draw the eye.*

Thanks for the comments; we have added one table to present some statistics of the fitness as shown in Table 3 on page 2 of this reply. The text has also been revised accordingly.

> *14520, 16: Surely chemistry would play as much or more of a role than NMVOC emissions for CH2O. There are some recent publications looking at this in the SH (Fisher et al., doi:10.5194/acp-15-3217-2015; Zeng et al., doi:10.5194/acpd-15-2615-2015).*

The production of CH_2O is not our main focus in this study. We used the OMI CH_2O column from 2005-2011 to rule out the possibility of large-scale trends in it. At the same time, we acknowledge that our model does not model the CH_2O column perfectly, and further study assimilation OMI CH_2O column would gain some significant benefits.

> *14520-14521: It doesn't make any sense for a trend over East Asia to "cancel out" a trend over the Amazon in a latitudinal mean; these would not be in the same latitude bands. Please rephrase this section.*

Thanks for pointing it out. We have rephrased this section as "the latitudinal mean however dose not bear a significant trend and is thus considered not strong enough to influence the global CO budget."

> *14522, 4-7: Are the lines in the figure the means of the 2 OH simulations? Please clarify in the text and/or figure caption.*

We have updated Fig. 8 inserted above on page 3, and in the new version we kept both values for TransCom and INCA OH. The relative differences are also addressed.

> *14524, 2-6: Boreal trends are not significant in prior or posterior.*

Thanks for pointing this out; we have emphasized this point in the text. "the same sign of the trends in CO emissions is mostly kept between the prior and the posterior, however, the trends are not statistically significant."

> *14524, 13+: Trends in SEAS are not significant in priori or posterior, and in SCAS they are only significant in prior. The discussion that follows is interesting, and still potentially relevant but this needs to be reframed in the context that the optimized simulation shows virtually no trend (highly insignificant with p=0.5).*

As explained in the previous reply, this part has been revised thoroughly. The trends are significant according to the updated assimilation. Associated Figure 10 has also been updated (shown in page 4).

> *14525, 1: ESEU trend is not significant.*

For the sum of ESEU emission, the trend is not significant. However, as shown in the trend distribution, the northern part of ESEU, the positive trends are significant. We will address this information more carefully in the revised text.

14525, 13+: African trends not really significant. It's also potentially worth noting in the following discussion that the posterior seems to overestimate amounts and trends in most of Africa (Figs. 3 & 5).

Thanks for pointing this out. The trends are only significant at some grids in the Sahel region and in parts of the South Africa. For the regional sum, the trends in the SHAF is not significant, but the trends in the NHAF is positive since 2006 onward. We will discuss these trends more carefully in the paper revision.

14526, 17: "global annual emission" – is this the mean over all years?

It means the global emission in a certain year.

14526, 22: and South America, from Fig. 9!

This part has been added.

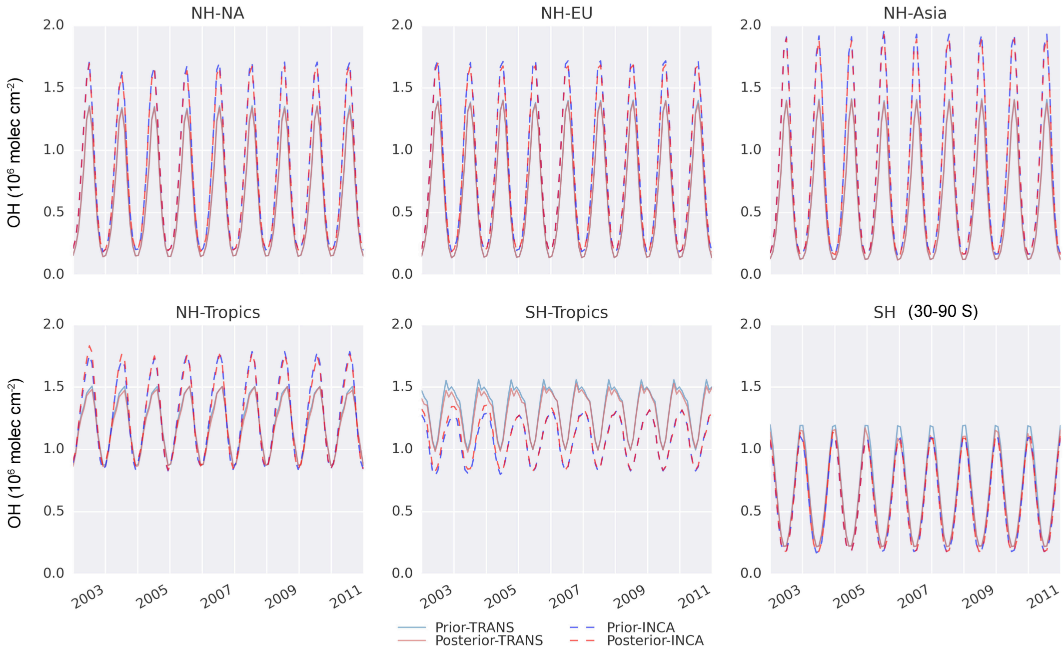
Fig. 6: Which shading is model & which is observations?

The shadings are overlaying and the color differentiation is not so clear. Therefore, we have removed the shading to represent spatial variation in the revised version.

Fig. 7: The change in color scheme for this figure is very confusing. Stick with the same as before (colors show posterior vs. prior, line style shows different OH).

Thanks. We have updated the Fig.7 as suggested here. Use color to show posterior vs. prior and line styles to show different OH.

Fig. 7. Regional volume-weighted monthly mean OH concentrations in the prior and posterior. The results are shown for the 6 big regions in which OH is optimized.



Technical corrections

14508, 6: change "a wrong" to "an incorrect"

Done.

14508, 8: change "more so challenging that" to "more challenging in that"

Done.

14508, 9: change "about a half" to "about half"

Done.

14515, 23: reference to Sect. 2.3.2 must be wrong because this is Sect. 2.3.2, but I'm not sure what section this should be.

Thanks for spotting this. We have revised the consistency in the structure of the manuscript.

14520, 10: change Section 4.1 heading from "concentrations" to "columns"

Done.

14524, 6: change "so are" to "as are"

Done.

14525, 17: change "so are" to "as are"

Done.

Table 1: reference for ocean emissions is different here than the one in the text

It is changed, the Belattaf reference was a French master thesis, we removed it as it is not publicly accessible.

Fig. 4: can you add "n=XX" to each plot to show the number of surface stations averaged in each band?

We have removed this figure, as its content is now summarized in Table 3.

Fig. 5: plots in (a) are too small to see the significance marks; this might work better as 2 columns x 3 rows rather than the way it is now.

We have removed the lower panel to present the distribution of CO. But

Fig. 7: "SH (>30S)" is confusing. I suggest "SH (30-90S)". Fig. 8: add units for the trend values (on figure or in caption). Fig. 10: change "Fig. S1" to "Fig. A1" in caption.

Done.