Atmos. Chem. Phys. Discuss., 15, C4080–C4085, 2015
www.atmos-chem-phys-discuss.net/15/C4080/2015/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 26 June 2015

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.

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 significant. 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.

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 interpret (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.

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).

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

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.

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?

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.

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

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

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.

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

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?

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.

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.

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.

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.

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

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.

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).

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.

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.

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

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).

14525, 1: ESEU trend is not significant.

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).

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

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

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

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).

Technical corrections

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

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

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

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.

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

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

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

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

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

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.

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14505, 2015.