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Interactive comment on "Sensitivity of inferred regional CO source estimates to the vertical structure in CO as observed by MOPITT" *by* Z. Jiang et al.

Anonymous Referee #2

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General Comments:

This manuscript presents sensitivity studies of top-down estimates of regional CO sources to: a) differences in the information content between profile retrievals and surface retrievals, and b) differences in model representation of OH spatiotemporal distribution. The premise of these sensitivity studies is that errors in modeled vertical structure of CO (and assumptions of OH) translate to errors in inferred CO sources, especially when investigating local-to-regional emissions. While I commend the authors for tackling this issue (which is certainly challenging), this issue is not something new. As noted by the authors, several studies have reported these errors (including previous

studies by the authors themselves, as well as inversion studies in the CO2 community). It begs the question whether this manuscript provides a unique contribution to inverse modeling studies. There are certainly interesting dimensions (or components) of the problem that requires attention which will help the community to improve accuracy in emission estimates. The manuscript however focuses (at least from the reviewer's point of view) on comparisons and sensitivity, which is already known to account for the major portion of the systematic uncertainties of the source estimates.

The reviewer recommends a major revision for this manuscript. Overall, the reviewer finds this manuscript to be a bit confusing, unclear, and unfocused. Please see specific comments for details of major concerns.

Specific Comments:

Title: It is unclear whether the author is referring to the sensitivity of inferred regional source estimates to the 'modeled' vertical structure. First of all, the reviewer suggests using 'top-down' rather than 'inferred' since there are other means of inference that doesn't involve inverse modeling. Second, it is not that the vertical structure of CO as seen from MOPITT is wrong, the sensitivity is due to the fact that the modeled vertical structure is not represented accurately (and that this error in the model is not represented in the inversion accurately) leading to errors in the estimates. The reviewer suggests modifying the title.

p. 1 line 14. What do you mean by 'signals'

p. 1 line 15-16. sensitivity …. to the 'modeled?' vertical CO distribution

p. 1 line 17-18. Suggests to use consistent terminology (to avoid unnecessary confusion) on 'assimilation' and 'inverse analysis'.

p. 1 line 19-20. a reduction . . . and an increase . . . relative to ???

p. 1 line 21. . . . suggesting an overestimate of the a priori isoprene source of CO. . . Is this due to errors in modeled vertical structure (that is unaccounted for) rather than sources (e.g., isoprene oxidation). It is unclear (even upon reading the text) that it is possible to tease out (or disentangle/attribute) this discrepancy.

p. 1 line 25. . . . discrepancies in convective transport in the model . . . How do you know this? Please cite or show.

p. 2 line 26-27. . . . from the CO profiles were significantly higher than those estimate from the surface layer retrievals during . . . Does the CO profiles also include the surface layer retrieval? What is the reason behind using only the surface layer retrieval? Shouldn't the default be using the profile or retrievals with at least 2 pieces of information (TIR – free troposphere and NIR –surface). The reviewer understands that some retrievals are derived from TIR radiances and a comparison of information content between retrievals is informative in itself but it appears it is not the focus of this manuscript. If it is, please state/describe it explicitly.

p. 2 line 29-33. . . . vertical transport of air from the North American and European boundary layer is slower than from other continental regions. . . and North America and Europe is more chemically aged . . . Can this be just due to errors (bias) in model transport (i.e., issues of representing frontal systems or synoptic meteorology or even mesoscale convection)? If so, it is unclear if we can make some conclusions on relative age of air unless when compared to observed tracers.

p. 2 line 42-43. . . . should the implication be more towards the use of vertical profile datasets?

p. 3 line 59. . . .included in the inverse analysis of CO2 (sources and sinks?) . . .

p. 3 line 70-71. Suggestion: in model parameterization of convective transport, chemical sink of CO, and long-range transport.

p. 4 line 74. What do you mean by CO signals?

p. 4 line 91-94. Please rephrase. Why would errors in CO accumulate in the free troposphere? Also, if the manuscript focuses on convective transport, shouldn't CO

be more mixed across the layers? It would be informative to show vertical/horizontal distribution of OH since the two versions of OH distribution may not only be different in the vertical but also near/over source regions and downwind.

p. 5 line 99. . . . Are section 4 results using pseudo data still?

p. 6 line 114-118. . . .true (actual) atmospheric state . . . please qualify that z here is in fact the true layer averaged CO state at MOPITT grid levels.

p. 7 line 151-152. Please elaborate on biogenic vocs (i.e. MEGAN versions) since this is discussed later on.

p. 8 line 162-164. Please make N and i in yi italic (consistent with eq 2). Fi(x)? yi is a vector of observed concentrations at a given time (does this mean also at a given space —horizontal and vertical?)

p. 8 line 164. . . .which represents the transport of the CO emissions . . . suggest qualifying this since F(x) represents not only transport but also chemistry.

p. 8 line 165-167. . . . is the a priori estimate . . . (of what?). also, please add dimensions of Se and Sa so it becomes clearer.

p. 8 line 169. . . .but is a set of scaling factors S such that x=sigma xa. Is S sigma?

p. 8 line 175-178. Why does eq 3 assume that the uncertainty in the emissions is normally distributed about scaling factor one? Please elaborate. Is this part of Sa? What is Sa? Why is there a mention of statistical distribution when in fact the previous discussion is about a cost function? Is x considered a random variable?

p. 8 line 177. . . .because it allows (the) negative emissions. . .

p. 8 line 183. . . .reduce negative gradients effectively . . . please elaborate the meaning of 'negative gradients' and 'effectively'.

p. 9 line 184-190. Why is there a problem with partially offsetting the decrease in gradient? Would this just be increasing the number of iterations to find the minimum? Please clarify. Also, it might be good (for the ms to be more concise) to move the discussion of this transformation and OSSE to a supplement or appendix.

p. 9 line 186-190. . . . Please elaborate on OSSEs. What do you mean by CO emission unchanged? And . . .we reduced the CO emission by 50%. What do you mean by . . .whether the scaling factors can return to true state (1.0). Scaling factors are not exactly the state.

p. 9 line 191-199. Why would there be different treatment of minimization? Should there be consistency in this regard? The reviewer is concerned (as also noted by the authors) that there is inconsistency in the error statistics and assumed error covariances and basic assumption of Gaussian distribution (if this methodology is viewed as similar to Bayesian inversion framework rather than purely variational scheme).

p. 9 line 198 . . .stati(sti)cs.

p. 9 line 203. Please qualify the rationale behind 5x10ˆ17 threshold.

p. 10 line 207-212. . . .which assumes that the mean differences between the model and observations are due to discrepancies in the emissions . . . The reviewer disagrees. The reviewer argues that the mean differences can be also attributed to systematic bias (especially this study on vertical transport) of the model. In fact, the treatment of the observation error here should be improved to account for this systematic bias. And if represented accurately can account for most of the differences in the top-down estimate discussed in this manuscript.

p. 10 line 212. . . .we expect assumption of a uniform observation error to have a negligible impact on our inversion results. . . The reviewer disagrees. The reviewer thinks that misrepresentation of observation error is the crux of the problem. Note that observation error here should also represent errors in F(x).

p. 10 line 215. What is the rationale behind assuming uncorrelated errors? Several

papers have reported the importance of this term in the inversion.

p. 10 line 218-224. Please elaborate on how initial conditions (from KF assimilation) are used in the inversion.

p. 10 line 225 . . .we will show(n) below. . .

p. 12 line 253. What do you mean by free run model?

p. 12 line 257. . . .MOPITT data (are these profiles?).

p. 12 line 267. . . There's a difference between this inversion and Hooghiesmstra et al 2012 since the latter used V4 column CO.

p. 12-13. It can also be argued that the differences (relative to GMD) are due to issues in sub-optimal Kalman filter (i.e. error covariance used to update the surface concentrations).

p. 13 line 292-293. . . .as shown in Figure 5c, the a posteriori emissions. . . These are scaling factors not emissions.

p. 14 306 . . . reduced (by) 32%.

p. 14 305-314. How about fires? Is there a compensating effect of fires and biogenic emissions? What is the impact of inaccurate injection heights?

p. 15 line 320-321. How about transport and mixing?

p. 15 line 323. What version of MEGAN would this be?

p. 15 line 321-322. OH fields are biased high in summer when the CO lifetime is short. CO lifetime is based on loss rate by OH isn't it (or is this residence time)? Please elaborate, especially when compared to line 315-317.

p. 15 line 330-336. What is the implication of this issue? Please elaborate on the importance of this paragraph.

p. 16 line 357. . . .significant(ly) greater.

p. 16-17 line 353-371. Please rephrase or simplify. It is currently hard to follow. The reviewer suggests comparing relative changes rather than magnitude since the priors in Kopacz et al 2010 priors and this manuscript are different.

p. 17 line 368-371. Please elaborate as to why there is discrepancy between the results and Kopacz et al. 2010.

p. 17 line 383. ..meteo(tor)rological.

p. 17 line 384. .. Here (the) the biogenic source(s) are combined with the combustion sources and optimize(d) a the resol(u)tion of the model.

p. 17 line 387 . . .and optimize(d). . .

p. 18 line 403. . . .We beli(e)ve

p. 18 line 401-410. Please rephrase or simplify. The reviewer suggests having description of convection and how this would increase emissions from the profile inversions.

p. 19 line 418. . . .We (we) performed . . .

p. 20 line 439-441. Please elaborate as to why this is a valid conclusion. As noted earlier, this can be just issues with model representation of synoptic and mesocale meteorology. Unless this is corroborated by observations, this may not be a valid conclusion.

p. 20 line 448-451. It would be informative to show vertical structure of OH.

p. 23 line 504-506. . . .due to model discrepancies in the free tropospheric abundance of CO. . . how are these discrepancies evaluated? Discrepancies relative to? It might just be model errors that are unaccounted for.

p. 23 line 523-529. How about using full-chemistry in the inversion rather than prescribed OH?

Figures: Please look at the figures once again and see if they can be deleted (not necessary) or combined.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 22939, 2014.