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Interactive comment on "On the relationship between tropospheric CO and CO_2 during KORUS-AQ and its role in constraining anthropogenic CO_2" *by* Wenfu Tang et al.

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General Comments

this paper examines the role of carbon monoxide (CO) measurements in a joint inversion of CO and CO_2 fluxes over East Asia. The paper builds on methodologies and results from some previous papers but delves further into the specific role of CO in improving the inversion.

I found the paper difficult to assess mainly because I have never felt comfortable that I understand the methodology of Palmer et al. (2006). It's quite possible therefore that

what I'm about to say is wrong. I also want to add that I am taking advantage of the discussion period to comment on the paper, i.e. this is not a review.

As I understand it the Palmer et al. (2006) methodology defines independent Jacobians for CO and CO_2 i.e. there is no physical coupling between the two sources or between each source type and the other observation type. These relationships are introduced by correlations in the observation and prior covariance matrices. It's obvious this works for prior correlations and less obvious (though still true) that it works for observations.

It is worth comparing the approach with what happens in nature. Sources are clearly related since some CO_2 fluxes are the result of combustion which may also produce CO with an uncertain yield. Writing the problem as $F_{CO} = \alpha F$
$F_{CO2} = (1-\alpha)F$ where F is the combustion flux and α the CO yield is how an inventory model would write this. If we linearise this relationship we can, I think, rewrite the problem with separate CO and CO_2 fluxes. With judicious choices of correlations we can probably ensure that the eigen-vectors of the prior covariance matrix (the space in which the inversion really takes place) reflect the underlying physical relationships. It's not obvious to me how to do this but the generated slopes between CO and CO_2 (as used in the paper) does seem a reasonable way to test it. So far so good.

The case for the observational correlations seems harder. What we shorthand as observational errors describe the differences between the modelled value with true inputs and the observed value (Rayner et al., 2019, Section 5). It encapsulates both model and instrumental/retrieval error. It is true that correlations in the observational covariance $\mathbf{R}$ do change the posterior uncertainty and mean for fluxes even if the fluxes are not coupled through the Jacobian.

Following notation of Rayner et al. (2019) consider the simplest case of two unknowns and two observations with an identity Jacobian $\mathbf{H} = \mathbf{I}$ and an identity prior covariance matrix $\mathbf{P} = \mathbf{I}$. Assume an observational covariance matrix $\mathbf{R} = 1\,\alpha$
$\alpha 1$.

Applying the Sherman–Morrison–Woodbury formula (Cressie and Johannesson, 2008) to generate the posterior covariance $\mathbf{A}$ we see $\mathbf{A}_{1,1} = 1 - \frac{2}{4-\alpha^2}$. The key point here is that the posterior uncertainty, and consequently the posterior estimates for the fluxes are quite dependent on the choices of these correlation parameters. If this is true then the paper needs to spend some more effort on either justifying or testing the sensitivity to these parameters.

Specific Comments

L447 Is an r value of 0.51 really moderate here, 25% of variance?

L451 You should not be quoting p values here, the p value measures the chance that there is a relationship at all which is not interesting in this case.

References

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