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Interactive comment on "New Constraints on Biogenic Emissions using Satellite-Based Estimates of Carbon Monoxide Fluxes" *by* Helen M. Worden et al.

Anonymous Referee #1

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Review of New Constraints on Biogenic Emissions using Satellite-Based Estimates of Carbon Monoxide Fluxes by Worden et al.

This paper deals with the top down estimate of biogenic CO emissions based on the GEOS-Chem model constrained with MOPITT observations. The paper brings interesting results about biogenic CO sources and their seasonal variability. The method provides improved estimation of these emissions. The paper is well structured, clear and well written. It should therefore be published in ACP. Nevertheless, the methodology and results that looks solid are often described too briefly. Some more detailed explanations should be given for some specific points that are detailed below.

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P3: it is mentioned that 3 different MOPITT products are used (columns, full profiles and tropospheric profiles) to empirically evaluate errors due to transport. How is this error estimate integrated in the total error of the posterior fuxes? What are the error values?

P4: could you provide details about prior BB uncertainties? Some values?

P4: why 50% is assumed for BIO and FF prior flux estimates Is this value coming from sensitivity tests with varying uncertainties? Is this the value that provides the best fit between model and observations? This choice should be discussed as well as the metrics and methodology used to evaluate the improvement of the modeled CO distributions relative to the MOPITT observations. And the criteria used to decide that convergence is reached.

P5: the average posterior errors ar given. The different contributions to the error have been mentioned previously (such as the empirical transport error) but we do not have a clear idea about the complete budget. An equation indicating the different contributions to the posterior error and the contribution of each error source to the total error given here would be of interest.

P5: it is unclear to me why posterior error for FF is twice larger than for BIO and BB. I would have expected that this source is better constrained in the prior inventory. And why MOPITT constrain this source much less than the 2 others? Could the authors elaborate on this point?

P6: the present study finds BB emissions (290 Tg/yr) of about 1/3 of those from Folberth et al. 2006 (811 Tg/yr). It is a large difference that is briefly justified by the fact that tropical fires have declined during the 2005-2012 period relative to the one used in Folberth et al. 2006 according to Andela et al. (2017). Could you give more details to convince the reader ?

P7: how is the posterior estimate affected by the change in forcing fields (GEOS FP

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versus GEOS-5? Is the top down method more robust to such changes than MEGAN?

P7: the results concerning the seasonality of the biogenic emissions are very interesting. The coincidence of isoprene and CO bimodal variability gives confidence in these results. Nevertheles, it is a bit desappointing not to have more explanations about the discrepancy between biogenic emissions and LAI variabilities! Are there some possible explanations? Why temperature plays a controling role in this N African Savannahs?

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