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Comment on acp-2021-326

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Referee comment on "Plant gross primary production, plant respiration and carbonyl sulfide emissions over the globe inferred by atmospheric inverse modelling" by Marine Remaud et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-326-RC2, 2021

This study advances the COS inversion approach to a new level by simultaneously incorporating COS and CO2. The investigators provide a thorough presentation of the many aspects of their inversion framework and their use of validation data shows the strength of their approach. I have some minor comments below that may improve the communication of this work.

Anthropogenic: It is nice to see our anthropogenic inventory being applied in this study because much has changed in the understanding of anthropogenic emissions since the earlier Kettle inventory was published. While our approach is based on the best available bottom-up information, there are high uncertainties in both the emission factors and the proxy data used for spatial scaling. These uncertainties mean that the anthropogenic inventory is more suitable for analysis over very large regions such as analysis with atmospheric COS measurements from background sites (e.g. MLO). This inventory is less suitable for analysis with atmospheric COS measurements that have significant influence from emissions in smaller regions (e.g. Paris/GIF). A few clarifying points could be added to the mansucript to help the reader understand this distinction.

Biomass Burning: I am also happy to see our open burning inventory applied in this study. An important note is that the open burning inventory does not include biofuels or agriculture waste. Biofuels and agriculture waste were estimated in Campbell et al. (GRL, 2015), with biofuels being about 3 times as large as open burning. Consider using all three components (biofuel, open, ag residue) in your study or add a note that open burning was included while biofuels and agriculture waste were neglected.

Plant Sink: Some quantification of the error due to the use of a zero-order plant sink instead of an online, first-order approach would be helpful. One way to quantify this error might be to use your posterior COS concentrations and use the lowest model layer in a temporally-explicit way to adjust the plant sink and then run that adjusted plant sink through your transport model and compare to your posterior COS at the sample sites. Perhaps make another version of Fig 5 for the supplement with an additional line that is the run discussed above. And perhaps also add a tropical forest site. Although there are no observation sites in the tropics, it may still be helpful to see how different a zero order and first order approach might be in this region.

If using such an approach in the future it might also be important to further estimate the difference between the lowest model layer in the atmospheric transport model and the leaf boundary layer.

Your approach to adjusting the zero order plant sink by the time evolving hemispheric means looks like a great way to account for seasonal changes in the mean hemispheric concentrations. You may also want to discuss the large geographic and vertical changes or try and quantify these in your work. The drawdown from the free troposphere to the canopy can be quite large. For example our work in the redwoods, we report measurements from the free troposphere, boundary layer, and canopy that each have a significant drop (Campbell et al., JGR-B, 2017).

Consider adding a sentence explaining how you estimated the constants in equations 6, 7, and 8.

In disregarding the LRU and referencing previous studies that have shown that this is an acceptable simplification for regional and global atmospheric simulations you might also explain the physical reason for why this is acceptable (atmospheric mixing causes the plant sink signal from multiple parts of the day). Also when noting the LRU relationship to light at ecosystem scale (P12,L21) you might add a reference for where this was first observed at ecosystem scale in Maseyk et al. (2014)

Ocean fluxes from direct and indirect sources are optimized using the same control parameter but these direct and indirect ocean sources come from very different processes that have distinct geographic and seasonal variation. It might be important to try an alternative simulation in which the direct and indirect sources are optimized using distinct sets of control variables. Perhaps this could be discussed as a possible direction for future work.

P5L19 "In practice, we considered only…" Consider adding some text to not if this leaves you with enough observations at each site. Since your control variable is amplitude does this mean you need enough observed samples per month to constrain the amplitude at each site? For example, would you want to make sure a site had at least one measurement per month in a given year to use that year's data in your inversion?

P23L20 "The seasonal cycle is degraded…" you might want to expand this sentence into a few sentences to explain more clearly to the reader what you are talking about.

Figure 1 is a very helpful illustration of the regions of influence. However some additional text could be helpful here. In particular, this figure (as well as some of the text) will leave readers thinking that the inversion has very little to offer in terms of information about the tropics. However as we see from your validation data in the tropics (HIPPO and MIPAS), the inversion does an excellent job of improving model skill in the tropics in general. While there might not be much to offer in terms of the tropical forests, it seems that SMO and possibly other sites are sufficient for improving the marine boundary layer in the tropics. Providing this context along with Figure 1 could be helpful.

Overall, this is an exciting paper that pushes the COS inversion approach into new territory. In particular, their study points to the need for additional tropical observations and improved bottom-up information on anthropogenic sources. Given the dearth of measurements on these two important points, it seems that there is significant opportunity to further advance the method and generate new discoveries in carbon cycle science.