Comment on acp-2021-326

Anonymous Referee #1

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-RC1, 2021

This paper addresses the global COS budget, and how it can help to constrain GPP. To that end, an analytical inversion is set up to assimilate CO2 and COS observations from a limited number of sites globally. COS uptake by the biosphere is coupled to GPP by a Leaf Relative Uptake (LRU) approach. In this way, COS observations inform about GPP, an important term in the CO2 budget. The impact of adjusted GPP on the CO2 budget can be compensated by adjusting the CO2 respiration term, a term that does not affect COS.

Technically, this is neat piece of work. The results indicate increases in GPP at high Northern latitudes, and reduced GPP in the tropics, in line with various recent CO2 inversions.

Most of the attention is focused on the COS budget. However, the important question is: what is the information that we gain about the CO2 budget. In that sense, the paper falls a bit short. Is the revised CO2 budget compatible with "independent" observations (not assimilated)? What follows are a couple of major comments along these lines.

Interpretation

One of the main questions is: how can COS help to better constrain GPP/respiration? So, one would expect an inversion without COS, and with COS to compare. Admittedly, a CO2 inversion that optimizes both GPP and respiration is likely ill-posed, but with proper priors this should be feasible. In a second step, COS is included. This would be a clean way to

estimate the potential of COS. Now, the story is unbalanced, because we cannot "see" the role COS observations played. The key figure is Figure 7, which shows the adjustments in GPP + respiration. Clearly, uptake is larger in the NH high-latitudes (> 50N). Is this supported by independent CO2 observations? The authors now suggest that this result is in line with recent CO2 inversion studies. However, in this paper COS is used as extra constraint, and only a sub-set of the large amount of CO2 observations is used. And the important question is: are the observed changes presented in Figure 8 driven by COS?

The authors show a comparison to SIF only, but I think there are plenty of other metrics, e.g. CO2 at independent stations, that could help to assess the realism of the COS+CO2 inversion. Now, the authors present a "validation" of COS by means of MIPAS, HIPPO, NOAA profiles and stations in France and Japan. Specifically the latter comparison is not very useful, although it points to misplaced emissions in the Zumkehr et al. (2018) study.

Also surprising is the lack of interpretation of the posterior covariance matrix (only the error reduction is presented in Table 6). This is one of the large advantages of the system. This could shed light on how well respiration and GPP can be separated.

In the discussion and perspectives session, the "coupled" aspect of the simulations is totally forgotten: it is all about the COS budget, and hardly about CO2. The issue: "Improving the relationship between COS plant uptake and GPP", would be a place to speculate about e.g. the optimization of the LRU per PFT, and how to further address GPP from both COS and CO2.

The choice of modelling errors (COS/CO2)

From the description of the errors, I get the impression that the errors have been chosen constant in time, and only varying depending on the station. This sounds to me an oversimplification, because e.g. summer hemispheric fluxes are much more uncertain (due to the active biosphere and their coarse representation). As a consequence, you would expect modelling errors to be much larger in summer.

Messy

The manuscript is still very messy, with many small mistakes (C instead of S, comments in French, units wrong (e.g. S12)). I attach an annotated pdf with some (but surely not all) of the errors corrected.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2021-326/acp-2021-326-RC1-supplement.zip