1 Reply to Anita Ganesan

We are grateful for the time you have taken to review our manuscript. We have addressed all the technical corrections in the main text following your comments. Please find our point-by-point reply below:

P5L8: M should be bolded M should not be bolded here as it referred to the non linear transport model. Thus, the bolded M referred to the linear tangent - the atmospheric transport model that was linearized with respect to the fluxes and initial mixing ratios.

Figure 1: unit parentheses are inconsistent in text and in colorbar legend We replaced $\mathrm{ppm}/(\mathrm{kg}/\mathrm{m}^2/\mathrm{s})$ by: $\mathrm{ppm}/\mathrm{kg}/\mathrm{m}^2/\mathrm{s}$ in the text legend.

RMSEs to 2 decimal points - is that needed throughout? - I would consider reducing consistently throughout text, tables, figures. We have reduced the RMSEs to 1 decimal point.

Additionally, we have changed the format of Tables 5 and 6 to make it more readable. In Table 6, we replaced "Gpp" by "vegetation fluxes", which is more correct.

2 Reply to the reviewer 1

Please find our point-by-point reply below. We would like to thank the reviewer for her/his second constructive evaluation of our paper. Their original text is reproduced in bold type.

The paper has greatly improved. I still do think, however, that the validation with local measurements in Japan and France weaker the paper. The authors conclude that the analysed measurements are too local. So, leaving this part out would bring better focus to the paper. I attach a pdf with some minor typos.

Our point may not have been clear enough, but in principle the measurements that we used in France and Japan are mostly broadscale and should therefore be appropriate for a model at the resolution of ours. First, the measurements in Japan in summer are useful to evaluate the COS ocean fluxes as the station samples background air masses coming from the eastern Pacific. In winter, measurements reflect the transport of anthropogenic emissions from China to the western Pacific by the Asian monsoon outflow (Hattori et al., 2020). Second, the GIF measurements are representative of background air masses even though the inventory points to a hot spot (Belviso et al., 2020). Indeed, there is a large uncertainty in the European inventory of COS due to large uncertainties in the emission factors and the proxy used for spatial disaggregation, with the consequence that most European capitals (e.g., Paris, Madrid, London, etc.) are wrongly estimated to be COS hot spots. Our analysis points to the imperative of improving the anthropogenic inventory if we want to use these measurements to constrain the GPP over Western Europe.

For these reasons, we decided to keep the evaluation with measurements in Japan and France, but we have clarified our argument in the following ways. Page 27, line 6 (new manuscript) "These measurements are mainly broadscale and should therefore be fairly reproduced by the LMDz ATM. In winter, the confrontation of the posterior concentrations against the measurements serves at evaluating the spatial distribution of the Zumkehr anthropogenic inventory over the eastern part of China. The same comparison analysis in summer serves at evaluating the posterior oceanic fluxes, as these measurements sample ocean air masses." Page 28, line 7: "Measurements at site GIF represent background values of COS in western Europe and no COS anthropogenic sources has been detected in the vicinity of the station (Belviso et al., 2020). "

Page 5, line 30: We replaced materiel by material.

Page 10, line 30: We replaced base soil by bare soil.

Page 28, line 16 : **I still think the evaluation of these sites (to validate posterior fluxes) is kind of misplaced in this paper. The resolution of the model and the inversion are too coarse to represent these sites well. Without these comparison the paper is better in my opinion.** As explained above, we have decided to keep the validation with measurements in Japan and France. Page 28, line 30: **I would be more critical about a "coarse resolution model" being capable to resolve these issues.** For that reason, we already wrote the following sentence in the discussion: *Further work should include a more thorough evaluation of European anthropogenic sources using COS retrievals from Fourier transform infrared spectrometry (Wang et*

al., 2016; Krysztofiak et al., 2015) along with a high-resolution (e.g., 0.5) chemical transport model in order to correct the spatial distribution of these emissions.

Page 30, line 30: We replaced TeragramS per year by $TgS.yr^{-1}$.