Review on **Verifying national inventory-based combustion emissions of CO_2 across the UK and mainland Europe using satellite observations of atmospheric CO and CO_2** by Scarpelli et al., 2024

The manuscript presents CO_2 emissions at European scale investigating the advantage of using additional CO observations in CO_2 satellite and in-situ inversions. The manuscript is well written and relevant for this journal, even though there are missing information and interpretation of the results which would help other readers to fully appreciate this study.
Minor comments are listed below.

General revisions:

Ln. 89. Could you add further information on CO2M such as it spatial and temporal resolution?

Ln. 112 Figure 1. Based on your Figure1, it seems that you are using TCCON data only for CO, but in your manuscript, you mentioned that all TCCON sites have CO_2 measurements. So, you could maybe add the red X points also for the in-situ CO_2 figure. The northern TCCON site in Germany seems to have no CO and CO_2 measurements, can you explain why there are no values for CO_2? Additionally, your caption mentioned 5 TCCON sites used to evaluate your inversions, but only 4 X points are shown in your Figure.

Ln. 119. You mentioned using drought adjusted observations for 2018 without further information. Can you develop why you used it and what it consists exactly?

Ln. 124. You only considered observations for a well-mixed atmosphere using a threshold value for the standard deviation of 0.3ppm. How did you estimate this threshold?

Ln. 127. You mentioned using Nicosia TCCON site located in Cyprus, however based on your Figure 1, this TCCON site is outside your domain. Did you use it consequently suggesting your domain is larger than shown in Figure 1, or did you not use it?

Ln. 130. Section 2.2. It is not clear if you did two separate CO and CO_2 inversions or if you did a joint inversion. It is not explicitly mentioned and would help the reader if it was mentioned at the beginning of this section.
This information can only be found later in your manuscript. Additionally, you do not provide information on how you treat averaging kernel information and the difference in vertical sensitivity between CO and CO_2 measurements to your joint inversion and TNO inversion. Can you give further information? Particularly, do you think that the difference in vertical sensitivity and variation in AVK between CO and CO_2 could impact your CO_2:CO inversions?

Ln. 156. TNO GHG should be more introduce. What TNO stands for? Why do you need to extrapolate data from 2019?

Ln. 163. Can you give examples of fugitives/non-combustion anthropogenic emissions? These examples could appear in the manuscript ln.163.

Ln.169. You re-grid CAMS fields to the GEOS-Chem horizontal spatial resolution of 2x2.5, this is in contradiction with your information line.141 where you mentioned using GEOS-Chem at 0.25x0.3125 resolution.

Ln. 255. Are the 2 scale factors for the chemistry terms linked to the oxidation of CH_4 and NMVOC? If that is the case, that should be clear here. If that is not the case, then further details should be mentioned here for these 2 scale factors.

Ln. 250 – 260. In your second approach of CO_2:CO joint inversion, how do you account for CO_2 production from the CO reaction with the radical OH?

Ln. 295. Any explanations why the bias is larger with CO than with the CO_2-only inversion?

It is nowhere mentioned which emissions are optimized in your inversions. Are the emissions from biomass burning optimized as well as biogenic and anthropogenic emissions?

Ln. 296. Based on which Figure or results do you observed seasonal biases?

Figure 3. Satellite joint inversion shows lower combustion emissions in winter and fall than other months and compared to the other inversions. When looking at Figure A.3, we do observe lower posterior emissions than prior emissions for the same months particularly with satellite observations. Any assumptions why? Are these lower posterior emissions linked to combustion or non-combustion?

Figure 3 and 4. The spatial distribution of observations between satellite and in-situ measurements is not the same over Europe with in-situ observations mainly in northern Europe, however your European a posteriori combustion emission seems to match between both set of observations. How do you explain it? And how would this spatial difference impact your inversions?

Ln. 324. This sentence would need further information and details. I do not see for the joint in situ inversion an increase for all months and all years in Figure 4. Which increase are you talking about? Like all inversions, we can observe a decrease from January to July and then an increase for the rest of the months. Annual and seasonal variabilities seem to agree between all inversions. We do observe an over-estimation for the join inversion at the annual scale compared to the other inversions but not an increase.

Ln. 363. Are we talking about decrease in combustion emissions or an under-estimation/reduction in the estimation of the inversions compared to the prior

emissions? It is confusing to talk about decrease or increase through the results section as it feels there was increase/decrease at national scale through the study period. Results do not suggest that country have reduce/increase their emissions but more that prior estimations are either under-estimated or over-estimated compared to optimized satellite/in-situ emissions.

Ln. 377. Any assumptions why France is showing the largest net sink for both satellite and in-situ inversions compared to other countries?

Technical revisions:

Ln. 81. Among the references cited here, I would suggest adding the MIP studies assimilating CO_2 satellites and in-situ measurements in an ensemble of several atmospheric inversions (Crowell et al., 2019 (https://doi.org/10.5194/acp-19-9797-2019) and Peiro et al., 2022 (https://doi.org/10.5194/acp-22-1097-2022)). Both studies have used OCO-2 measurements and have looked at Europe emissions among several other regions.

Ln. 92. Carbon monoxide is not introduce in the introduction.

Ln. 403. CO2 should be CO_2

Ln. 402 through 405. This sentence is a bit too long which makes it difficult to understand. I would suggest re-writing it.