We would like to thank reviewer 1 for the effort, interesting questions, and constructive comments. Below we address the points one by one. The reviewer's comments are listed in cursive, with our answers in blue and excerpts from the revised text in red.

Overall – This paper provides a useful study of trade-offs for the inversion of carbon monoxide emissions for both the data and a priori considerations. I find that the claim that the method could be suitable for near-real-time inversions is not supported by the results. However, the main conclusions are sound and I think the paper could be published after addressing some concerns.

1. Near real-time suitability: No timing trade-offs were presented, in fact the only discussion of this was that 5 real-world days were required for each inversion and more for satellite full res. Also, the grid resolution did not allow characterization of biomass burning events, a primary motivation for near-real-time. It was suggested regional analyses, with a finer zoomed resolution, would be possible, but these were not demonstrated here.
We thank the reviewer for the comment. Discussions about creating a near-real-time setup as a longer-term goal were largely removed in a previous iteration of the manuscript. We apologize for any confusion caused by the remaining references, which have now been removed.

2. Use of OH monthly climatological fields (L. 137) should have more discussion of why this choice is applicable for the TROPOMI time range.
OH in the global atmosphere is relatively well buffered, and the Spivakovsky climatology still complies with observed methyl chloroform loss rates. This climatology is still used in studies investigating recent periods. More appropriate OH fields are being explored in ongoing investigations. To address the raised concern, a small paragraph (L153ff) has been added to the manuscript:
"Jiang et al. (2017) show that OH is well buffered in the atmosphere on a global scale over the past decades, as indicated by a low month-to-month variability in the methyl chloroform loss rate, and as such the TransCom OH climatology is still considered applicable to recent years, as in e.g. Naus et al. (2022)."

3. The only comparisons are for the surface CO flask observations. It would be of interest to see comparisons to other independent CO satellite observations.
We thank the reviewer for this useful suggestion. However, we believe that such a comparison would extend beyond the scope of this manuscript. As an outlook, combined inversions driven by TROPOMI and IASI or MOPITT are planned for the future. At this stage, a direct comparison of the inversion results to other datasets is not trivial due to their different vertical sensitivities in combination with other model limitations. Most notably, the OH climatology needs more work before such an effort becomes meaningful.

4. Other suggestions: L 66: should include the following reference: Naus et al. (2022), Sixteen years of MOPITT satellite data strongly constrain Amazon CO fire emissions, Atmospheric Chemistry and Physics, 22(22), 1473514750, doi:10.5194/acp-22-14735-2022.
This is a valid suggestion. Given that the recommended paper was not published when this manuscript was first submitted, we have now included it where appropriate. Throughout the manuscript, references to recent studies have been added.

5. Plots: Lines indicating color in the plot legend are difficult to distinguish - maybe make these thicker.
Fig. 5 and Fig. 6 have been updated accordingly.

6. Readability suggestions:
We thank the reviewer for these suggestions. Corresponding updates have been incorporated throughout the manuscript. For the sake of brevity, they are not repeated here.