We thank the editor for the helpful comments on how to improve the clarity of our manuscript. Below we address the points raised by the editor one by one. The editors' comments are listed in *cursive*, with our answers in blue and excerpts from the revised text in red. Any line numbers refer to the latest manuscript version with mark-up.

L51: You may like to omit using "Again" in this sentence, or rephrase "Using the same example ..."
The sentence has been changed as advised and now reads (L45):
"Using the same example of wildfire CO emissions, their effect in the atmosphere is an elevated CO concentration that can be observed and then traced back and attributed to its source using atmospheric modeling."

LL57–59: Please rephrase this sentence (grammar, the second part leaves ambiguity about meaning).
To reduce ambiguity, the sentence has been split in two parts and the second part was further elaborated. It now reads (LL50–53):
"Top-down estimates do not necessarily require observations of the source event itself. Instead, it is usually sufficient to gather observations of the resulting atmospheric tracer concentrations during the time span between the source event and them falling below the detection limit due to loss processes and dispersion."

LL167–170: Looking at Fig.4 of Jiang et al. (2017) I cannot confirm low (implied insignificant) month-to-month variation in MCF loss rate. Perhaps, you imply variation in annual averages? Please use accurately the statistical terms here (and throughout the manuscript), "variability" and "variation" are not the same (see, e.g., `https://web.ma.utexas.edu/users/mks/statmistakes/terminologyrevariability.html`). Whilst we do not know the (spatiotemporal) variability in the MCF rate not being able to measure it everywhere/every time in the atmosphere, we can analyse the variations in the limited set of estimates of average loss rate in time.
We thank the reviewer for pointing out these mistakes. The word variation was replaced by the more accurate term variability, in places where it referred to a natural variation (L245, L285, L384, L715, and in the caption of Table 2). Additionally, we have changed the sentence in LL155–157 to more accurately cite Jiang et al. (2017):
"Jiang et al. (2017) show that OH is well buffered in the atmosphere on a global scale over the past decades, as indicated by the methyl chloroform loss rate varying by only 0.2 % between 2001 and 2015."

L170: You probably refer to Naus et al. (2021) here, not Naus et al. (2022)? The former study analyses a set of available OH distributions. Reference: Naus, S., Montzka, S. A., Patra, P. K., and Krol, M. C.: A three-dimensional-model inversion of methyl chloroform to constrain the atmospheric oxidative capacity, Atmos. Chem. Phys., 21, 4809–4824, `https://doi.org/10.5194/acp-21-4809-2021`, 2021.
The citation of Naus et al. (2022) was intentional. The sentence has been restructured to clarify and now reads (LL157–159):
"Thus, the TransCom OH climatology is still considered appropriate for studies investigating recent years. For example, Naus et al. (2022) use it in the context of inverse modeling of CO emissions up to and including the year 2018."

L357: Please explicate on "in remote regions where transport is slow". What kind of transport is slow and why? I can only think of significant convective transport changes between land and ocean, however that would imply marine remote regions, for example.
Here we conflated slow vertical transport and long transport times to remote regions. The ambiguity should be removed in the revised text, which now reads (LL329–332):
"Overall, harmonizing the mixing ratios modeled in TM5-4DVAR and the observations requires that the model is run over a longer period of time. Such a long spin up period is particularly relevant for high altitude layers, to which transport through vertical mixing is slow, or regions at large distances

from primary sources, to which transport takes a long time."

L363: "Generated emissions" may be ambiguous for some readers (including me), are the emissions optimised during spin-up inversion implied?

Yes, the optimized emissions are implied. The sentence was changed accordingly (L338):

"Therefore, the final month of the spin-up inversion is considered as its spin-down period, during which confidence in the optimized emissions and the resulting mixing ratios is reduced."

LL779–799 and Table 3: I find double ambiguity in providing extrapolated budgets and comparing these with the results of the studies that do not even cover 2018, and fear that such extrapolated figures can be of little use for any follow-up study. I suggest recompiling Table 3 so that it includes only exact estimates (that is, Jun-Dec 2018), or both those of spin-up and main inversion periods (also exact). These, however, will be comparable by anyone who simulates entire 2018, including a possible extrapolation to their annual estimates. Should you like to keep the comparison on the extrapolated terms, please add a short explication on these are obtained from the exact terms, therefore you can omit quoting final extrapolated figures but keep the discussion.

We thank the editor for pointing out this double ambiguity. First, in our initial draft we had intentionally only compared our results to Zheng et al. (2019) (who studied CO emissions up to 2017) because of the ambiguity introduced by comparing results from different time periods and we are not aware of global studies that specifically include 2018. We are aware that the other studies we added as requested by one of the reviewers are further removed in time. However, we would like to point out that the year-to-year variations within Jiang et al. (2017) and Zheng et al. (2019), respectively, are smaller than those between each of those studies and ours. As such, the systematic differences as discussed in manuscript still hold. Second, the magnitude of the biases introduced by comparing budgets for seven months with annual budget terms can be estimated by considering the impact this had on the prior emission, since they are known for the full year. Overall, these biases are small (+4 % for biomass burning and secondary production; < -2 % for fossil fuel) compared to the differences of our results to those reported in other studies. To reduce ambiguity we have removed the improper use of terminology of 'extrapolated annual budget terms', since simply using the unit 'Tg CO yr^{-1}' for a flux is not an extrapolation. The paragraph in question now reads (LL753–766):

"All fluxes in Table 3 are provided in Tg CO yr^{-1}, despite neither inversion period spanning a full year. While this unit allows for an easy comparison to (annual) budget terms published elsewhere, such a comparison must consider that the inversion period of the main inversions includes the biomass burning season, but excludes the increased anthropogenic emissions due to heating during part of the northern hemispheric winter. The biases of such a comparison can be estimated by comparing the prior fluxes from Table 3 for the *reference* inversion to the respective annual budgets of the prior source estimates, which show an overestimation by around 4 % for biomass burning (FINN2.5) and secondary CO production (from TM5-MP) and underestimation by less than 2 % for the anthropogenic emissions (CMIP6). With this caveat in mind, we compare our prior and posterior budget terms with values from other inversion studies with different setups, namely to Jiang et al. (2017, who assimilated MOPITT CO and methyl chloroform surface measurements in the GEOS-Chem model, Müller et al. (2018), who assimilated IASI CO in the IMAGES model, and Zheng et al. (2019), who assimilated MOPITT CO in the LMDz-SACS model. A detailed comparison of these three studies can be found in Elguindi et al. (2020). Compared to the results of either of those studies, our a priori budget terms for secondary CO production and chemical loss of CO to OH are far too large."

The caption of Table 3 was changed to:

"Note that the unit Tg CO yr^{-1} for the columns showing rates was chosen for ease of comparison to other estimates and does not imply annual rates. The rates were obtained from the processed masses divided by the duration of the respective inversion periods, January to June (6 months) for the spin-up inversion and June to December (7 months) for the main inversions."

Table 1: The value for inflation in 'Set 2 – Satellite only' entry is provided in parentheses. Do the

denote anything particular? If so, please include the respective description in the table caption or in a table footnote.

The parentheses were meant to highlight that the inflation factor for the satellite only inversion was not calculated, but rather taken from the reference inversion, as stated in the caption. For clarity, this information was further explicated and moved to the table footnote, which now reads:

"†The inflation factor for the *satellite only* inversion cannot be derived as described in Sect. 3.2.2 since the flask measurements do not contribute to the observational cost in this experiment. Instead, the same inflation factor as for the *reference* inversion is used to ensure consistent weighting against the prior."