Response to reviewers

Reviewer's comments are shown with a *grey background and in italics, like this*.

Reviewer #1

The manuscript has been greatly improved by accounting for the effect of reduced CO and NMVOC emissions and by including an ensemble of simulations to characterize uncertainties. I have a few minor suggestions:

Line 97-98: Minor sinks of methane include oxidation by Cl. How is that accounted for in the methane lifetime calculation? Probably not important for the results, but better to include for completeness.

Many thanks for pointing out this omission. We have now included the Cl sink in the whole atmosphere methane lifetime calculations. It shortens the lifetime and perturbation lifetime (as expected), but has an only minor impact on the results (also as expected).

Fig. 1 Caption: better to briefly describe what perturbation is performed in the caption (e.g., 20% reduction of global anthropogenic emissions including NOx, CO, and NMVOC), so a reader does not have to look for the information in the text.

We have added clarification to the captions for Figure 1 and Figure 2.

Line 133: Remove "As for NOx,"

We have removed.

Reviewer #2

In the revised manuscript, the authors include the influences from CO and NMVOCs. But the results are still estimated using the sensitivity for board [sic: broad] regions over 2000. For different sectors, the emission reductions during the COVID-19 lockdown are different. Thus, the emission reductions may be heterogeneously distributed. I agree with the authors that the emission changes can influence OH and methane chemical sink. However, considering the nonlinear OH chemistry, and the spatial and temporal heterogeneous emission changes, I still think such simplifications are not enough for estimating the OH and CH4 changes in response to emission changes. The main conclusion may be right, but I suggest the model simulations using the emission inventory given by Lamboll et al. (2021) are needed to support the conclusion.

We appreciate this reviewer's concerns and agree with many of these points. We also agree that new model simulations using the Lamboll et al. (2021) inventories would be very useful to support (or not support) our findings. However, this, in our opinion, would be a different study altogether, and is for the future. We acknowledge that OH chemistry is non-linear, and that by using HTAP simulations with a base year of 2001, that this somewhat compromises our results, since emissions have evolved since 2001. We also acknowledge that the emissions changes seen during the lockdowns will differ spatially from the emissions changes used in the HTAP experiments. On the more positive side, we note that the -20% emissions changes applied in the HTAP experiments are remarkably similar in magnitude to the emissions reductions estimated over the lockdowns, so that at least this source of error should be minimised. The HTAP study is also incredibly useful in providing a multi-model ensemble, allowing us to assess differences in sensitivities across several models, and estimate an uncertainty range. In addition, the regional experiments performed within

HTAP are unique for a co-ordinated global multi-model study, and this provides further incredibly useful extra information, allowing us to break down regionally the origin of the lockdown influence on methane. To perform a multi-model HTAP-like regional set of experiments with the Lamboll et al. (2021) emissions would be ideal to answer these questions in more detail, however that would be a major effort, and is something the community should consider for the future (perhaps this paper could provide a basis for such a study). Until such a study has been done, it is impossible to say whether the simplified estimations presented here are more or less realistic. We argue that the results we present here are useful, but have several caveats, as pointed out by the referee, and as we have clearly acknowledged in our paper. One of the points of writing the paper (as with most or all papers?) was to stimulate the community to perform further research to confirm or refute our results, as they appear to be important (we note both referees indicate the scientific significance of our results as outstanding).

Besides, in my last comments, I mentioned that the author should clarify how they estimate the results from the original data given by Fry et al. (2021). I cannot find it in the revised manuscript. Also, I suggest the authors list the sensitivity of CH4 changes to NOx/CO/NMVOC emissions used to estimate the values in table 2 in the manuscript.

We have now added some Supplementary Information that hopefully comprehensively clarifies how we have calculated our results using the original data in Fry et al. (2012). The supplementary information is a spreadsheet with multiple pages that steps through the calculations from the raw model data used in HTAP to produce Figure 4 in Fry et al. (2012). We recreate that figure, which shows the equilibrium response of methane to emissions changes in the HTAP simulations, then extend the analysis to produce the sensitivities in Figures 2, 3 and 4 of our paper. The values for the plotted sensitivities in these figures are given in the supplementary Table S10. We appreciate that our methodology, whilst fundamentally relatively simple, has multiple non-obvious steps, and we hope this supplementary information provides sufficient clarification to make it possible for anyone interested in reproducing or analysing our results or developing our methods to do so.

Note to the editor and reviewers

In revising the analysis to satisfy the reviewers, we found some (ultimately relatively minor) additional errors in our analysis contained in the revised submission of 3rd August. We have corrected those errors in this version, but it has not significantly altered our overall results.

The first error was that we realised the HTAP NOx experiments were 20% perturbations to *all* anthropogenic emissions, not just surface emissions. This meant aviation was already included, and by adding it we were accounting for it twice. In fact, this was a bit more complicated, as the surface emissions reductions during lockdown were about 15%, whereas the aviation reductions were 23%, so about 8% of the aviation reduction did still need to be added. We have attempted to clearly describe how we have done this in the revised version.

The second error was that we realised we were using the incorrect HTAP emissions changes to produce the sensitivities in Figures 2-4. The supplementary information in Fiore et al. (2009) provides both total emissions data and anthropogenic emissions data. We had been erroneously using the total emissions rather than the anthropogenic emissions to calculate the 20% emissions changes applied in HTAP. The sensitivities calculated now are more like the estimates in the initial response to reviewers presented in the ACPD discussion.