We thank the reviewer for her/his comments. Here they are repeated (in bold) with our replies.

The title of this manuscript suggests the authors intend to explain aerosol radiative effects during the early stages of the pandemic with reduced emissions. However, it is not clear to me that they have made a convincing case to justify the title after reading the manuscript. In particular, in the manuscript, they describe a model (nudged by meteorology) with four simulation runs targeting the period of interest. They say their model does a "reasonable" job in general, but this assertion needs further contextualization and reasoning. Overall, a reader like me is left wondering what the authors are actually trying to tell in this manuscript: Is it the "unique" measurement campaign, a "unique" model they developed to match these measurements, or fundamental new progress and understanding about aerosol radiative effects?

A chemical-climate model of such complexity as the one used our study cannot perfectly simulate observations, not even in specified dynamics mode. On the other hand, a chemistry transport model or air quality model, which might be better suited to simulate the observations, cannot easily be used for radiative impact calculations. We use a CCM (instead of a CTM) to base an estimate of radiative impact (of reduced emissions during the World-wide COVID lock-down) on observations of atmospheric trace gases and aerosols. The agreement between our model simulations and the observations from a tailored measurement campaign using research aircraft is comparably good for most of the trace gases and aerosols, especially when compared to other intercomparison based on other model setup (e.g. **?**Pozzer et al., 2022). We believe that our evaluation is quite comprehensive, although room for further analysis is obviously present. In order to avoid giving the false impression, we agree to change the title of the article, to fit better the content of the manuscript. The new title "Numerical simulation of the impact of COVID–19 lockdown on tropospheric composition and aerosol radiative forcing in Europe" reflects the content of the manuscript, for which we simulated the "unique" measurement campaign during the COVID-19 lockdown.

Major points

- **What is the purpose of this paper? Is it the model only or the associated knowledge/insight produced by this model in concert with the measurements? There is a clear disconnect between the title and introduction on the one hand and the rest of the manuscript on the other. As an example, there is a lengthy discussion of aerosol effects in the introduction, yet it is not clear how anything in the rest of the paper fills any of the many gaps in our understanding of aerosol–cloud interactions. The authors should consider shortening the introduction (pointing the reader to further material) and instead focus on what they actually address.**

  We thank the referee for this suggestion. We have reduced the introduction. In addition, as the referee pointed out, we decide to change the title of the manuscript, to better reflect the content of the manuscript: "Numerical simulation of the impact of COVID–19 lockdown on tropospheric composition and aerosol radiative forcing in Europe". To our knowledge, no studies on European lockdown have been performed for the entire troposphere, by evaluating model predictions with the observation in the entire tropospheric column. Following the comments of the referee #2, we have instead extended the model description to provide more detailed information on the methodology.

- **The model evaluation is incomplete at best, perhaps quite weak. It can also be circular at times. The authors use a nudging technique whereby they anchor**

the model to some meteorology, then they evaluate the model by comparing some model outputs to aforementioned meteorology. Is that an accurate reading? Shouldn't they be the same by definition? More context and thorough explanation is needed here.

This is a classical misunderstanding, at least partly because we did not provide sufficient information about our model setup: we apply a CCM in "specified dynamics" ("nudged") mode, which is established by Newtonian relaxation of the prognostic variables divergence, vorticity, temperature (without global mean), and logarithm of surface pressure. This "nudging" is applied in spectral space by low normal mode insertion, i.e. on the synoptic scale only, in order to nudge the meteorology of the CCM towards the "observed" (indeed analysed or re-analysed) meteorology. Due to the scale limit of this procedure, a CCM in "specified dynamics" mode is still fundamentally different from a CTM, since the nudged CCM develops its own physics on sub-synoptic scales. Therefore, it is required and reasonable to compare the simulated meteorology with the available in-situ observations despite the nudging, as described in line 173. For an in-depth discussion on the Newtonian relaxation technique applied in spectral space, we refer to Jeuken et al. (1996) and Löffler et al. (2016). In our study we show that the agreement between the nudged physical state of the model atmosphere and the observations is good, despite some deviation on specific humidity (a quantity that is NOT nudged). We will clarify this point in the revised version.

- **In general, aerosol indirect effects are challenging and any work purporting to make progress in this field should be scrutinized. So please be precise and forthcoming about what this work actually brings to this field. Again, it is important and timely; so this is not to dismiss this work, but please be as precise as you could to contextualize your work.**

  Indeed, our study does not focus on conceptual advancements or the development of new methodologies. We rather apply a state-of-the-art model to reproduce (by numerical simulation) data observed during the BLUESKY campaign, to further base a qualified estimate of radiative impact (namely of the reduced emissions). We believe our model results to be particularly robust, as the comparison with the observations shows a very good agreement at lower levels, where the highest concentration of aerosols is present. As mentioned above, for clarification, we decided to change the title of the manuscript to focus more on the impact of the world-wide COVID–19 lockdown in Europe.

Minor comments

- **While you are free to make up your own definitions, it is often not a good idea to make up acronyms that have other meanings in popular culture or other fields. For example, "STD" refers to sexually transmitted diseases in general, and in this manuscript, it refers to "standard" or "business as usual" — the authors should consider unifying their approach here: They use "STD," "business as usual," and "baseline" throughout the manuscript. One would suffice, preferably the last one, "baseline."**

  Thanks for pointing this out. We changed STD to BASE.

- **In many places there are added sentences that add no value to the text. For example, line 172 could be deleted; the first part of line 189 could also be deleted;**

lines 105–108 are unnecessary; the majority of line 157 can go as well. More on these in the list of technical comments below.

We have followed some of the suggestions of the referee (e.g., line 105–108). In other cases (e.g. line 172), we decided to keep the sentence as this is needed to reference the figures and the table.

List of technical/specific comments:

Most of the technical/specific comments have been accepted. A few comments deserving a more detailed answer are presented here below.

Also doesn't "aerosol particle number concentrations" cover the previous parts (e.g. ORG)?

The ORG refers only to the organic material present in the aerosols (as mass).

What was the time step of the model? Sampling at 5 minutes seems too frequent for these types of models. Did you simply interpolate from the model time step or what is going on here?

The model set-up used in our work has a time-step of exactly 5 minutes. We added this information on the model description.

Line 360: Could you reflect on this range a little more? Seems insignificant and uncertain to a casual reader.

We have extended the text with the following lines: " The differences between these studies, can partly be attributed to the applied methodologies and general difficulties in discriminating anthropogenic effects from interannual variability. Hence, a study which considers contrail and aerosol effects simultaneously, and covers a longer time period, is recommended to better attribute the causes of the observed changes . "

Code availability: Why list all these details about doing MOU and all that, can you just give the git repository link and tag/commit?

Unfortunately, due to the contained legacy models, we are not allowed to host parts of the code open source or publicly accessible. Our GitLab repository is not open and a URL would be therefore meaningless. This section is standard and used in the sister journals GMD(D) as mentioned in the license section of the MESSy system (https://www.messy-interface.org/).

Data availability: Please make your data available, and refrain from "contact the author" stuff. It doesn't seem open...

The observational data and the model results are available on the HALO (High Altitude Long RAnge research aircraft) database (https://halo-db.pa.op.dlr.de), upon sign of data protocol. This was included in the revised manuscript.

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