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Review of acp-2021-1005

Anonymous Referee #2

Referee comment on "Numerical simulation of the impact of COVID-19 lockdown on tropospheric composition and aerosol radiative forcing in Europe" by Simon F. Reifenberg et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-1005-RC2, 2022

Review of " Impact of reduced emissions on direct and indirect aerosol radiative forcing during COVID–19 lockdown in Europe" by Simon F. Reifenberg et al.

In their manuscript "**Impact of reduced emissions on direct and indirect aerosol radiative forcing during COVID–19 lockdown in Europe**", the authors simulate the effect of the covid-19 lockdown on atmospheric composition and radiative fluxes with the ECHAM5/MESSy (EMAC) model and compare the results to aircraft observations from the coinciding BLUESKY aircraft campaign performed in May/June 2020 over Europe. As expected from the emission reductions as well as demonstrated by prior work, such as the CovidMIP intercomparison study published before submission of the manuscript, the associated reductions in aerosol concentrations lead to a reduced aerosol radiative forcing over the focus area.

Overall, this could be an interesting study by a large group of authors with considerable expertise. However, in the light of a good number of existing studies on this topic, the manuscript in its current form falls short of providing many new insights. This is on the one hand because the (interesting) comparison with the aircraft campaign data has no influence on the actual simulations performed, it is primarily there to assess the model as "reasonable", so I do not concur with the description of the use of an "observation-guided model". And on the other hand, this is because the current description of the model and simulations and the analysis of the results does not provide the necessary details on the underlying processes, leaving the authors and readers to speculation about the processes underlying some of the key findings.

I note that the first author is an early career researcher so these comments are meant to be constructive – it should be possible to address these shortcomings through major revisions. However, I also note that much of this task should have been taken care off by the very experienced group of co-authors, assuming they have provided feedback, and not be left to the reviewers. I will start off with some general comments, followed by detailed feedback below:

General comments:

Introduction:

The literature review tends to omit primary references and focuses on recent work, with quite a few imprecise descriptions of key processes (that this long list of expert authors could easily address).

Methods:

The description of the model and of the setup of the simulations is insufficient, which affects reproducibility and the ability to interpret the results.

Context

The manuscript entirely ignores an international model intercomparison project on this very subject, CovidMIP, which has been published months before submission of this manuscript (Jones et al, GRL, 2021). (Disclaimer: I am not involved in CovidMIP.) Clearly, the results of this study should be put into the available context but beyond that, it needs to be clear what additional insights are gained other than the focus on a specific area. The availability of a dedicated aircraft campaign provides ample opportunity to do this but is currently not exploited beyond a baseline evaluation of the model.

Analysis

The interpretation of the results tends to be quite speculative and is held back by not tracing the perturbations through the full chain of relevant processes and by a lack of dedicated sensitivity studies necessary to back up some of the interpretation of the results.

Specific comments:

Introduction

Where possible, please use primary references. For example, the trade-off between GHG and aerosols was not discovered in 2019 or the dependence of forcing on surface albedo is not something new from the Belloin et al. (2020) paper…

Line 73: *"The cloud albedo effect can be enhanced by the Twomey effect"* is very confusing as they tend to be used synonymously. I do not understand what is meant here.

Line 79: *"The same effect arises in aircraft flight tracks…"* claims analogy between ship-tracks (albedo enhancement of existing clouds via Twomey effect or LWP increase) and the formation contrails but this is really not the same.

Line 88: Cloud lifetime effect is introduced without giving credit to Albrecht and treated as a fact, rather than a long-standing (and often disputed) hypothesis. The cited references are fairly outdated.

Data and methods

Aerosol cloud interactions are key to the derived radiative effect but the description of their representation in the model is inadequate:

Line 132: *"The aerosol–cloud interactions are based on the aerosol microphysics parameterization of Pringle et al. (2010) including aerosol aging and the continuous calculation of aerosol number concentration depending on the mass mixing ratio and mixing state."* This is not a description of aerosol-cloud interactions but of the underlying aerosol microphysics. Which key processes are represented and how? To name a few: updraft velocities, activation, the link from activated particles to CDNC (in particular in presence of existing droplets), the effect of CDNC on cloud microphysical (through autoconversion/accretion) and radiative properties.

Line 135: *"Large-scale cloud formations and prognostic variables depending on cloud microphysical processes follow the work of Lohmann et al. (2007); Lohmann and Hoose (2009); Bacer et al. (2018)."* This seems unlikely as Lohmann et al describe a cloud microphysics scheme and you seem to refer to the cloud fraction scheme (which presumably is Sundquist but this is not described at all).

Line 140…: *"We performed four simulations … without cloud–aerosol interaction"* casually

refers to simulations performed without cloud-aerosol interactions. This is not a trivial exercise using a two-moment cloud microphysics as clouds droplet number concentrations are prognostic and, if decoupled from aerosols, need to be initialised somehow (and the base-state will affect the results due to inherent nonlinearities) but no details are given on how this is done.

It is difficult to put the results from this study into the wider context, such as AeroCom or CovidMIP, without a summary of the of ERFari and ERFaci from PD-PI simulations. As we currently have limited constraints on ERFaci from observations, it is important to know where the model lies in the ERF uncertainty range e.g. from IPCC AR6 or the Bellouin et al (2020) assessment. This should be included and discussed either in the methods or results section.

BLUESKY observational data

Line 164: Measurement cut-offs are quoted but it is not clear if and how these are applied to the model size distributions in the evaluation. Are they explicitly applied for each component, how are internal mixtures dealt with – or are they ignored? And if they are, how would this affect the results?

Results:

Figure 2: The evaluation of O3, CO, NO is looking very good. Has there been any calibration/tuning during the setup of the simulations or is this out of the box?

Line 206: Here and later it is hypothesized that the underestimation of SO2 (and later on SO4) is due to representation of transport from the boundary layer or from the stratosphere to the upper troposphere or due to model short-comings within the stratospheric aerosol chemistry – but no further sensitivity studies are conducted to underpin this hypothesis.

Line 217: *"The measured black carbon (BC) concentrations are captured well by the model close to the surface, while the observational variability is underestimated at high altitudes."* This seems to neglect the significant bias – it looks like median concentrations are almost an order of magnitude out in the upper troposphere? This section also needs to explicitly caution (not only in the caption) that you switched plots from a linear to a log scale…

Line 235: *"A single factor causing the model underestimation of BC and sulfate aerosol concentrations in the upper troposphere, e.g. a localized plume of pollution, is judged*

unlikely, as BC and SO2− do not correlate" I am not sure I follow the logic here. This would be true if both would stem from the same source but for plumes arising from entirely different sources it seems plausible to find low correlations – while biases may still be affected by the same process such as a common transport or removal process.

Line 269: *"It must be stressed however, that those relative changes in 270 the upper troposphere, although significant, have a very minor impact on most trace gas budgets, due to their low mixing ratios at these altitudes."* Mixing ratio is conserved under vertical displacement – you probably mean low concentrations (due to exponential pressure decrease)?

Figure 3 & 5: are concentrations normalized to STP (needs to be clear in the caption)?

I am missing an effort to use interesting measurement data and the evaluation to provide some constraint or context for the following analysis of aerosol radiative effects. As a minimum it would be helpful to analyse if the simulated change in response to the emission perturbations are larger than the underlying model biases (which would add trust) or not (which would add less trust).

Impact on radiation

This section (and subsequent use) should stick to well defined nomenclature of aerosol forcing as used by IPCC, i.e., be clear what is RF, what is ERF, what is ari and what is aci. This also means that ERF should include SW and LW and it is not clear why the analysis is restricted to SW only.

This section would be much more intuitive if it followed the actual chain of processes from aerosol properties, through aerosol radiative properties (AOD, AAOD) all the way to the radiative fluxes.

Line 289: Fluxes defined at what level?

Line 298: *"our radiative effect of all aerosols in our RED simulation for May is of −3.33±1.36 Wm−2, which is close to their value of −2.3 Wm−2."* The definition of "radiative effect" is entirely unclear here.

Line 303: *"the total absorption (clear sky) was decreased by 0.064±0.053 Wm−2, with slightly more than one third of this caused by the BC decrease"* How do you know? No analysis or evidence is provided here and internal mixing makes BC absorption quite nonlinear - if considered in the model (which is not described).

Line 309: *"The decreased heating (for the entire column but mostly 310 at the surface) is due to the reduced absorption by BC".* Again, how do you know?

4.2.2 Aerosol-cloud interactions

This section does not provide sufficient detail to interpret some of the results and the analysis skips crucial steps in the chain of underlying processes. The results are noisy, something that could be addressed through an initial condition ensemble and it is therefore not clear how robust the results are.

Line 314 / Fig 8. What is N – how is it defined? Is it total CN without size-cut off?

Line 323: *"these differences (in N, CDNC, ICNC) can be directly connected to the reduced air traffic present during the lockdown (REDCLOUD)"* UTLS aerosol tends to be dominated by nucleation (not sure if this included in N or not as it is not defined) so the attribution to aircraft is ambiguous. I am really missing a process-based analysis here from emission to CN to CCN/INP to CDNC/ICNC – and from the model description it is not even clear what processes could actually affect CDNC/ICNC. Likewise, the attribution to specific emission sectors should not be based on speculation – it would be trivial to run a simulation with and without the aircraft only emission reductions to make this point.

Line 334: There is really very limited point to quote RF/ERF with three significant figures in the presence of significant noise and uncertainty.

Line 338: *"This confirms the importance of the cloud–aerosol interaction, as mentioned by Hong et al. (2016), Gasparini and Lohmann (2016) and Myhre et al. (2013)."* This is of course not new but the split is highly model dependent so this should be discussed early on (as suggested above).

5 Conclusions

Line 345: *"Nevertheless, problems remain regarding stratosphere–troposphere transport, especially of volcanic influence, which resulted in systematically underestimated SO and*

SO2−of stratospheric origin, and a consequent overestimation of NO−3 (which substitutes the underestimated sulfate in ammonium salts) in the upper troposphere." This could be true but no results are provided to underpin this, nor is a reference given that shows this.

Line 354: *"With reduced emissions, the model simulates a lower number concentration of aerosols; this reduction is located at an altitude too high to effectively influence the cold cirrus clouds"* From the model description it is entirely unclear if or how "aerosols" could actually affect cirrus in this setup.

Line 356: *"The analysis of the indirect aerosol effect did not give any conclusive results, due to the large variability in the calculations caused by the short duration of the lockdown "experiment".* I agree but you also make the argument that these "effects" dominate the overall result, so this suggests that this could be noise?

Data availability

At a minimum, the data going into the plots should be deposited in an open access archive.