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Reply on RC1

Juan Cuesta et al.

Author comment on "Ozone pollution during the COVID-19 lockdown in the spring of 2020 over Europe, analysed from satellite observations, in situ measurements, and models" by Juan Cuesta et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-785-AC2, 2022

Responses to reviewers on
the manuscript "Ozone pollution during the COVID-19 lockdown in the spring 2020 over Europe analysed from satellite observations, in situ measurements and models" by Cuesta et al.

We would like to thank the reviewers for their suggestions that have improved the clarity of the paper. All their remarks have been addressed in the Revised Manuscript (RM). Their main suggestions have been followed by:
(i) Shortening and simplifying the paper and the figures (as requested by Referees #1 and #2), through suppressing from the main manuscript the results of the version C2 of the CHIMERE model, only showing results for only one period (1-15 April) and mainly in terms of daily averages. A single exception to this is one figure, which is important for comparing the results of the current paper with other publications. Some of the withdrawn results have been moved to the supplement as suggested by referee #1.
(ii) An additional figure (Figure 8) is added that briefly describes the link with the variability of ozone at the free troposphere and the link with the stratosphere, as well as the associated meteorological conditions at the free troposphere (as suggested by Referee #2).
(iii) A new figure (Figure 7) addressing the point stated by Amir Souri concerning the influence of meteorological conditions in near surface ozone over the Iberian Peninsula
(iv) Two additional Figures (Figs. 4b and 5d) and Table 3 show the model smoothed by the averaging kernel of the satellite approach and the direct comparison with respect to satellite data, as requested by Referee #1.

These main points are described in detailed in the following paragraphs, as well as all additional and specific points remarked by the Referees. All other points have also been clarified and addressed thoroughly

Responses to comments from Referee #1

General Remarks
This paper describes changes in atmospheric pollutants over Europe in April 2020, attributable to the COVID-19 lockdowns and 2020 atmospheric conditions. The data and

methods used are valid, and the results appear generally reasonable and plausible. The paper fits well into the scope of ACP. It adds some new insight to the large body of existing literature on atmospheric changes due to the COVID-19 lockdowns and is in principle suited for ACP. I found the paper interesting but hard to read. Presentation and conciseness should be improved substantially. To me, there is way too much non-essential information in the paper, and a lack of focus on clear take-home messages. Essentially, the paper is trying to do too much: i) Compare tropospheric average satellite data with surface in-situ data, ii) compare observations with simulations, iii) compare two quite different model simulations, one of them (C2) giving unrealistic low changes between 2020 and 2019, (iv) compare two fairly similar (and even overlapping) periods in April 2020, with little or no significant difference and (v) compare surface daily averages with maximum surface 8 hour averages, again with no major differences.
In the end, this becomes very confusing, and I can't see a clear storyline. I think this paper needs major revisions and should become substantially shorter and more concise.

Agreed and withdrawn from the main manuscript. As suggested by Referees #1 and #2, we have substantially shortened the main RM, limiting the description of the results in terms of one model setup, one period and one main metrics for surface ozone. Some of withdrawn material is provided as supplement. Additional specific comments on this topic are also provided in the next answer.

Major Suggestions
I strongly suggest shortening the paper considerably and to remove a lot of the non-essential material. The removed material could either be dropped completely or could be moved to a supplement. In a supplement it would still be published and available for people needing the extra information. Shortening will allow a much clearer and concise paper, focused on the major points.
The CHIMERE model version C2 seems to underestimate NO2, and COVID-19 related NO2 reductions by a large margin. C2 also shows unrealistically small COVID-19 related ozone changes. What is the point of having simulation C2 in the paper? I suggest dropping all C2 related information (and possibly move C2 related information to a supplement).
Is there any major take home message for the difference between the two periods April, 1 to 15, and April, 1 to 30? I don't see neither a large nor an important difference. Therefore, I suggest retaining only the April 1 to 15 period (with the clearest COVID-19 effects) in the main paper, and to drop the April 1 to 30 period (or move it to a supplement).
Is there any major take home message for the difference between surface MDA8 and daily averages? I don't see neither a large nor an important difference. Suggest dropping MDA8 (or move it to a supplement).
In this way, the paper would be shortened considerably, and become much more concise and focused. While rewriting, the English should be improved in many places as well.

Agreed and withdrawn from the main manuscript. The original manuscript intended to show consistency or its absence between different periods, model setup and ozone metrics. We agree with the referees that presenting only the main results will simplify the manuscript and provide a clearer message for the reader. In consequence, the RM only shows CHIMERE simulation with the C1 setup which is simply called "CHIMERE", focused on the single period 1-15 April (where the greatest changes for the COVID-19 lockdown period are observed) and uses daily averages of surface ozone concentrations. The only exception to this is a figure (Fig. 3) that uses the same metrics and period as two other papers largely compared and cited in the RM (Ordonez et al., 2020; Souri et al., 2021).
Results for the whole month of April and the model setup C2 have been moved to the supplement. We have chosen to show C2 results in the supplement, as its comparison with C1 clearly reflects the uncertainties in the modelling tools,

which is an original and interesting aspect of the paper.
The choice of daily averages for describing surface ozone distribution is supported by the fact that this metrics shows the same variability as satellite data of lowermost tropospheric (LMT) ozone, a fair correlation, and a smaller average shift as compared to surface MDA8 ozone. This last aspect is shown in a new statistical comparison between satellite data and surface MDA8 ozone shown in Table 4. These results suggest a larger variability and a larger negative shift for MDA8 surface ozone than for daily averages, when compared with IASI+GOME2 LMT ozone. As suggested by the reviewer, we have substantially shortened and simplified the RM. Several analyses and comparisons have been moved to the supplement.
The English language of the whole manuscript has also been revised when rewriting.

Minor Points
Is it correct that the surface data are only taken at the locations and days of largely clear-sky IASI+GOME2 measurements? Is there a large difference to taking all surface data? Might be necessary to mention that, if necessary, even show a plot.

Clarified and comparison added. Yes, it is correct that Figure 1b only considered locations and days with coincident IASI+GOME2. However, very small differences are seen between the average of all in situ surface data and that coincident with IASI+GOME2. Thus, an additional figure is not necessary.
The clear similarity between these two averages is shown in a new case in Table 2 and lines 251-253 of the RM as " … while Fig. 1b only considers in situ data coincident in time and space with satellite data (although very little differences are seen for the average of the whole in situ dataset). " and lines 265-267 "When considering the average of the whole in situ dataset, the only notable change is an increase in the standard deviation of 13% for the surface data (see Table 2).".

Are the model simulation data sampled at the satellite locations and days, or at the ground-based locations and days, or are all model data used?

Agreed and revised. The original manuscript does show the average of all days and locations for the CHIMERE model. Although very similar, we agree that for a more consistent comparison between Fig. 1 and 4, they all should use the same sampling as satellite and in situ data. Therefore, the new Figures 2b and 4 of the RM also show the average of model data coincident with satellite data. Table 2 of the RM shown the statistical indicators for data only coincident with IASI+GOME2.

The -8 ppb shift of the 2020-2019 delta in the IASI+GOME2 data compared to the same delta in the surface data should not be called "bias". It is not a "bias", it is a larger observed difference, and in section 3.1.1 the authors mention a number of possible reasons.

Agreed and revised. We agree that a difference between the surface measurements and satellite retrievals of LMT ozone is not a bias since they do not refer to the same atmospheric layers. We have corrected this indication in the whole RM, indicating it as a "difference" or a "shift".

One thing not mentioned is ozone reduction in the upper troposphere, resulting from the Arctic stratospheric "ozone hole" in March and April 2020. Given the wide satellite averaging kernels, this may well contribute to the larger 2020 to 2019 difference seen in the satellite data. See also Steinbrecht et al. 2021, Bouarar et al. 2021, Miyazaki et al.

2021, Ziemke et al. 2021 for more context. These references should generally be considered more to provide context for the paper.

Agreed and added text and figure in the RM. We agree and appreciate the suggestion from the referee. In the RM, we have added a new Figure 8d showing IASI+GOME2 measurements of stratospheric ozone and a statement about the contribution of the reduction of stratospheric ozone in 2020 over Northern Europe, as compared to 2019, and the suggested references.
This sentence is given in line 281-284 of the RM " This is mainly associated with the reduction of anthropogenic emissions at large scale during the pandemic lockdown in 2020 and in lower degree to a large 2020 springtime ozone depletion in the Artic stratosphere (less than one quarter of the observed tropospheric anomaly, see also Bouarar et al. 2021, Miyazaki et al. 2021, Ziemke et al. 2021). " and lines 463-465 "Over the North Sea, the reduction of upper tropospheric ozone at 6-12 km of altitude is strengthened by a depletion of stratospheric ozone occurring in 2020 (see in Fig. 8d as ozone anomalies with respect to 2019)."

Fig. 7: What would the modelled 2020-2019 difference look like for the simulation(s), if the wide satellite averaging kernels were applied? Would that result in larger negative anomalies more like the satellite observations?

Clarified and Added Figures in the RM. We agree that it is relevant to show the effect of satellite retrieval sensitivity and the vertical consistency between CHIMERE and the satellite product. The new Figures 4b and 5d shown model results smoothed by IASI+GOME2 averaging kernels. For total changes between 2020 and 2019, we remark that their main effect is smoothing and reducing the changes in ozone simulated by the model. However, we see a clear difference with respect to surface data when analyzing the simulated changes associated to COVID in terms of LMT ozone and also smoothing with IASI+GOME2 averaging kernels. In these two last cases, CHIMERE only simulates a weak reduction of ozone over all Europe and no ozone enhancements. This suggests a clear underestimation of the simulated changes associated to the pandemic lockdown averaged within the LMT (< 3km) as compared to IASI+GOME2 satellite observations.
This information is provided in the RM in lines 309-311 "This is similarly found for LMT partial columns from CHIMERE smoothed by IASI+GOME2 averaging kernels (for accounting for the satellite vertical sensitivity, Fig. 4b), except for simulated enhancements over the Atlantic and Central-eastern Mediterranean." And lines 386-392 "On the other hand, we notice clear differences in the simulated changes associated with the pandemic lockdown for model-derived concentrations integrated up to 3 km of altitude (LMT) and also when smoothing with IASI+GOME2 averaging kernels (Fig. 5d). In these two last cases (see Table 3), CHIMERE only simulates a weak reduction of ozone over all Europe and ozone enhancements become negligeable. The range of variability of simulated concentrations decrease by more than a factor 10, although the correlation with respect to IASI+GOME2 data remains fair (around □0.5). This suggests a clear underestimation of the amplitude of the effect of the pandemic lockdown simulated at atmospheric layers above the surface and within the LMT (< 3km) as compared to IASI+GOME2 satellite observations.".

One drawback of the regional model simulation domain is that it does not account for the hemispheric scale emission and background ozone reductions (and for changes in the stratosphere?). This may help to explain why all observed anomalies seem to be

substantially larger than the simulated anomalies in Fig. 7 (see also discussion of Fig. 5 around line 360). CHIMERE C2 looks kind of useless with nearly no simulated anomaly- just drop it.

Agreed and clarified. We agree with these statements about the lack of background ozone reduction in 2020 due to the regional domain and the lack of variability between 2020 and 2019 for the modeled stratospheric contribution. They were implicitly considered in the original manuscript, but we agree to make a clearer statement.
In the RM, we provide more straight forward statements as (lines 46-48): "Moreover, a significant ozone decrease observed at large hemispheric scale is not simulated since the modelling domain is the European continent. As simulations only consider the troposphere, the influence from stratospheric ozone is also missing." and lines 492-494 "Furthermore, the model does not simulate the ozone decrease observed at large hemispheric scale nor the stratospheric influence, as the simulation domain covers Europe and the troposphere.".

Figure 9 presents essentially the same information as Fig. 8. The only reason to keep Fig. 9 would be to also show satellite measured NO2 columns. Without those, I would drop Fig. 9 (or move to supplement).

Clarified and withdrawn from the main manuscript. The difference between Fig. 8 and 9 of the original manuscript was one is affected by vertical mixing (surface concentrations) and the other not (total column concentrations). Their comparison shows the role of vertical mixing within the atmosphere, which is the main explanation from differences between the simulations with the two model setups (C1 and C2, this last one shown in the supplement only). Therefore, the former Figure 9 of the original manuscript is withdrawn from the RM but included in the supplement.

There is a lot of duplication / redundancy between Section 2 and introductory paragraphs in Section 3. I suggest dropping or shorten these text parts in Section 3.

Agreed and shortened. For avoiding redundancy, the introductory part of section 3 is substantially reduced in the RM.

Fig. 5 and other places. I am missing a direct comparison between modelled 2020-2019 differences (with averaging kernels?) and satellite observed differences. Was this not done, or was it omitted for the sake of conciseness?

Clarified and Table 3 added. Direct comparisons between model simulations and satellite data were indeed previously omitted for sake of conciseness. For providing information about it while keeping the manuscript short, we have added an additional Table (Table 3) that provides the statistical scores of such satellite/model comparisons.

Detailed Comments
line 23: replace "particularly enhanced" be "better"?

Agreed and corrected as suggested.

line 30: "bias" is the wrong word. "difference"?

Agreed and corrected as suggested in the text and the figures.

line 31: add "and averaging kernels extending into the upper troposphere"?

Clarified. The averaging kernels of the IASI+GOME2 lowermost tropospheric ozone retrievals only reach the middle and sometimes the upper troposphere over ocean. However, they only extend within the lower troposphere (below 5 and 6 km of altitude) over land and they and peak around 2 km of altitude (see Cuesta et al., 2013).
Therefore, such a statement is added in the RM (31-32) " ... be explained by the fact the satellite approach retrieves partial columns of ozone with a peak sensitivity above the surface (near 2 km of altitude over land and averaging kernels reaching the middle troposphere over ocean). ".

line 34: replace "for withdrawing" by "by subtracting"?

Agreed and corrected as suggested.

line 36: Is this not a null statement? Before you have said that both observational datasets are more or less consistent. Now you have subtracted the same meteorology from them, and they are still consistent. With the exception of a few really unusal cases, I would expect them to be consistent also after a subtraction or addition.

Clarified. The statement is no null since the correction for the two datasets is not the same. Surface data are corrected using model simulations of surface ozone and satellite data with model simulation of ozone integrated below 3 km of altitude (LMT) and smoothed by the averaging kernels. If those corrections were significantly different, corrected surface and satellite datasets could have differed.
This aspect is clarified in the RM as (line 36) " Using adjustments adapted for the altitude and sensitivity of each observation ".

line 39: replace "highlight the" by "provide".

Agreed and corrected as suggested.

line 48: since the models underestimate so much, you should explain possible causes in the abstract as well (e.g. missing reductions in emissions and background ozone outside of the model domain).

Agreed and accentuated. Those aspects were mentioned in the original abstract. These statements are rewritten in a more direct way in the RM as (lines 44-50) " Moreover, a significant ozone decrease observed at large hemispheric scale is not simulated since the modelling domain is the European continent. As simulations only consider the troposphere, the influence from stratospheric ozone is also missing. Sensitivity analysis also show an important role of vertical mixing of atmospheric constituents, which depend on the choice of the meteorological fields used in the simulation, for better matching the observed changes of ozone pollution during the lockdown. ".

Also: since the models perform so poorly, how can you be sure they get the meteorological changes from 2019 to 2020 right?

Clarified. A verification that the meteorological changes between 2020 and 2019 are sound is provided by a comparison with other independent studies from literature (particularly Ordonez et al., 2020 and Souri et al., 2021). The

consistency of these corrections is thoroughly described in section 3.2 of the RM and Figure 3b of the RM.

Additional References

Bouarar et al. (2021). Ozone anomalies in the free troposphere during the COVID-19 pandemic. Geophysical Research Letters, 48, e2021GL094204. https://doi.org/10.1029/2021GL094204Miyazaki et al. (2021). Global tropospheric ozone responses to reduced NOx emissions linked to the COVID-19 worldwide lockdowns. Science Advances, 7, eabf7460. https://doi.org/10.1126/sciadv.abf7460
Ziemke et al. (2021). Evaluation and Validation of Tropospheric Ozone Hourly and Daily Maps Measured from EPIC, OMPS, OMI, and MLS Satellite Instruments. Presented at CEOS AC-VC 17 meeting. https://ceos.org/document_management/Virtual_Constellations/AC-VC/Meetings/AC-VC-17/3.Wednesday-Ozone/3.04_ziemke_v1.ppt